



Einstein and the “Crucial” Experiment

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considerable experience. The best standard equipment is the use of a spectrophotometer to determine the bands which are present. If the F band is present, the baseline, peak wavelength and width at half height should be determined. From the thickness of the crystal the absorbance in cm^{-1} should be determined. If the oscillator strength² is known, the number of centers per cubic centimeter should be estimated. The expected concentration in KCl and KBr can be predicted from Rögner's data.⁶ Since the temperatures are measured externally to the cylinder, there are certain errors depending on the particular design. However the results should be reproducible. The determination of F-center concentration by Smakula's formula² is fundamental for almost any further use of additively colored crystals.

V. EXPERIMENTAL CONTRIBUTIONS

In addition to the experiments already mentioned, some of the possible experimental continuations¹¹ (say, in a senior project laboratory) using colored crystals are:

- (1) Optical absorption
 - (a) Accurate optical measurements

¹¹ For information concerning experimental techniques and underlying theory, see Schulman and Compton: *op. cit. passim.*, also Markham: *op. cit. passim.*

- (b) Optically induced processes
- (c) Thermally induced processes
- (d) Higher energy effects (x-ray, γ -ray)
- (e) Kinetic studies
- (2) Photoluminescence
 - (a) Thermal effects
 - (b) Emission bands
 - (c) Stimulation bands
- (3) Photoconductivity
- (4) Thermoluminescence
- (5) Resonance
 - (a) EPR
 - (b) ENDOR
- (6) Lattice parameter changes

In crystals containing electron excess-type centers, the principal reactions concern the agglomeration process (production of M, R, and other electron excess centers).

Caution should be exercised in the design of these experiments; the routine production of controlled samples by the method described here is only the initial process, and each salt requires an individual set of minor adjustments. The possibility of the more complicated experiments using the crystals depends on many factors and the literature should be consulted to avoid unproductive exercise.

Einstein and the "Crucial" Experiment

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This paper was presented on 5 February 1969 at a joint symposium of the AAPT and APS on the history and philosophy of physics, held during the AAPT-APS annual meeting.

When asked to speak at this session, I thought it might interest you if I chose a debated episode in the history of recent science that can be illuminated by documents found in the Einstein Archive at Princeton. Over the past few years, I have helped to supervise the cataloging of the papers and correspondence there, and some of you may remember that from time to time I have reported at these meetings what has turned up.

Today I shall use some documentary materials to examine the widespread opinion that it was a crucial *experiment* that led Einstein to formulate the special relativity theory.

Our discussion will start innocently enough, but by the end I hope to have raised some rather unsettling questions, for example these:

What shall the historian of science do when he does not have "all the facts"?

Is there something built into the educational process itself that tends to make us tell a wrong story about scientific developments?

Is our vision of the role of theory in physics today to some degree distorted by an outdated or erroneous philosophy of science?

We start with a letter dated 2 February 1954, about a year before Albert Einstein's death, from Davenport of the Department of History of Monmouth College, Illinois, who wrote to Einstein that he was looking into evidence that Michelson had "influenced your thinking and perhaps helped you to work out your theory of relativity." Not being a scientist, Davenport asked for "a brief statement in nontechnical terms, indicating how Michelson helped to pave the way, if he did, for your theory."

Einstein answered very soon after receipt, on 9 February 1954 (according to the copy of his hitherto unpublished reply in the files of the Einstein Archive of the Institute for Advanced Study at Princeton). Of course, Einstein had frequently been asked about Michelson's influence, particularly during the previous few years (e.g., by Shankland and Balazs). Having perhaps pondered again over the answers he had given earlier, Einstein now seemed remarkably willing to respond to a stranger. He gave not only a detailed reply, but even volunteered his offer to allow the letter to be published, and invited further correspondence¹:

Dear Mr. Davenport:

Before Michelson's work it was already known that within the limits of the precision of the experiments there was no influence of the state of motion of the coordinate system on the phenomena, resp. their laws. H. A. Lorentz has shown that this can be understood on the basis of his formulation of Maxwell's theory for all cases where the second power of the velocity of the system could be neglected (effects of the first order).

According to the status of the theory it was, however, natural to expect that this independence would not hold for effects of second and higher

orders. To have shown that such expected effect of the second order was *de facto* absent in one decisive case was Michelson's greatest merit. This work of Michelson, equally great through the bold and clear formulation of the problem as through the ingenious way by which he reached the very great required precision of measurement, is his immortal contribution to scientific knowledge. This contribution was a new strong argument for the non-existence of "absolute motion," resp. the principle of special relativity which, since Newton, was never doubted in Mechanics but *seemed* incompatible with electro-dynamics.

In my own development Michelson's result has not had a considerable influence. I even do not remember if I knew of it at all when I wrote my first paper on the subject (1905). The explanation is that I was, for general reasons, firmly convinced that there does not exist absolute motion and my problem was only how this could be reconciled with our knowledge of electro-dynamics. One can therefore understand why in my personal struggle Michelson's experiment played no role or at least no decisive role.

You have my permission to quote this letter. I am also willing to give you further explanations if required.

Sincerely yours,
Albert Einstein.

It is a thoughtfully composed reply, and the last letter of Einstein I have been able to find on this subject. We may in fact regard it as an excellent summary of what one can learn from a study of the many other first-hand documents in the Archive dealing with the question. In particular, the text of the letter shows the need for four kinds of sharp differentiations: (1) between the effect the experiment had on the development of physics, and the effect it may have had on the development of Einstein's own thought, his "personal struggle;" (2) between the beauty of the immortal experiment, and its subsidiary place in theory; (3) between the statements Einstein made in direct response to repeated requests to deal with the possible genetic role of the Michelson experiment, and the rather different statement he made whenever he volunteered any comments concerning the genesis of relativity (in which case Einstein almost always spoke only about the experiment of Fizeau and the aberration measurements, insofar as he spoke of measurements at all); and (4) between the large interest the whole question seemed to have held for many people, and the small interest it held for Einstein, who

¹ I thank the Estate of Albert Einstein, and particularly Miss Helen Dukas, for permission to publish this and the other cited documents from the Archive.

This article constitutes a summary of portions of a detailed study scheduled to be published in the summer 1969 issue of *Isis*.

neither could recall whether he really knew of the experiment in 1905 nor seemed particularly disturbed by this fact.

Today I shall not be concerned with providing further support for Einstein's retrospective evaluation (that Michelson's result "has not had a considerable influence" on Einstein's development of the special theory of relativity, that "it played no role or at least no decisive role"). Such a task requires a close reading of the evidence, mostly long available, as found in Einstein's first paper on relativity (in which Michelson's experiment is not specifically mentioned), and in Einstein's other voluminous work, comments and letters. (An essay of this kind will appear elsewhere). In a nutshell, Lorentz's electrodynamics appeared to Einstein to contain too many *ad hoc* features—among them, as one of several factors, the patent artificiality of its explanation of the ether-drift experiments. What we shall ask here is, first of all, this: If we accept Einstein's response, *why* is it that the published evaluations by others, with very few exceptions, are so very different? After all, it has been the overwhelming preponderance of opinion of scientists and others over the last half century that Michelson's was the fundamental experiment that led Einstein directly to his relativity theory. Most of the popular literature and many scholarly discussions concur with the characterization given in the caption under

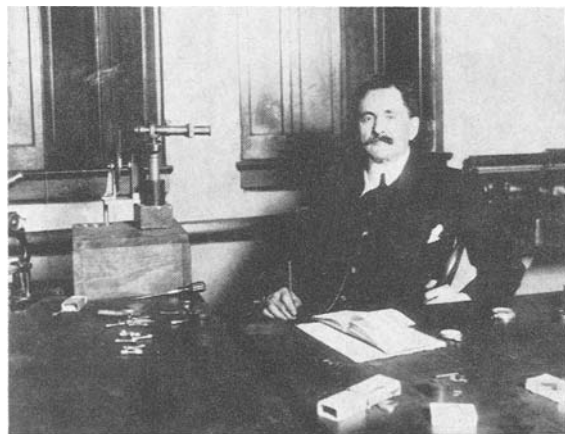


FIG. 1. "Albert Abraham Michelson, first American scientist to win the Nobel prize in physics, made the measurements on which are based Einstein's Special Theory of Relativity. Michelson is shown here in his laboratory at the University of Chicago." Photograph and caption as published in *Your Career in Optics* (The Optical Society of America, New York, 1965), p. 3. Reproduced by permission.

Michelson's picture in a publication of the Optical Society of America: Michelson "made the measurements on which are based Einstein's Special Theory of Relativity."

A more detailed account of the experimental origins of relativity theory, coming from an authoritative source, and printed at an unusual occasion, but otherwise quite representative of recent and current thought, is found in Millikan's essay entitled "Albert Einstein on his Seventieth Birthday." It was the lead article in a special issue in Einstein's honor of the *Rev. Mod. Phys.* **21**, (1949), and the early parts are worth quoting at some length:

The year 1905 was a notable year in that at the age of 26, Einstein published in that year's issue of the *Annalen der Physik* three brief but remarkable papers which have had very important bearings upon my own work as a physicist throughout my whole life. These three papers were on the following subjects: (1) the special theory of relativity; (2) the Brownian movements; and (3) photoelectric stopping potentials.

Every one of these three papers represents new and far-reaching *generalizations* of immense importance. For the first and second of these the stage had already been set, and the *experimental* foundations on which all sound generalizations must rest had already been built. In the case of relativity, the prime experimental builder had been my own chief at the University of Chicago, Albert A. Michelson, who made his first experiment on aether-drift at Berlin in 1881 . . . The special theory of relativity may be looked upon as starting essentially in a generalization from Michelson's experiment. And here is where Einstein's characteristic boldness of approach came in, for the distinguishing feature of modern scientific thought lies in the fact that it begins by discarding all *a priori* conceptions about the nature of reality—or about the ultimate nature of the universe—such as had characterized practically all Greek philosophy and all medieval thinking as well, and takes instead, as its starting point, well-authenticated, carefully tested *experimental facts*, no matter whether these facts seem at the moment to be reasonable or not. In a word, modern science is essentially empirical . . .

But this experiment, after it had been performed with such extraordinary skill and refinement by Michelson and Morley, yielded with great definiteness the answer that there is . . . no observable velocity of the earth with respect to the aether. That unreasonable, apparently inexplicable experimental fact was very bothersome to 19th century physics, and so for almost twenty years after this fact came to light physicists wandered in the wilderness in the disheartening effort to make it seem reasonable. Then Einstein called out to us all, "Let

us merely accept this as an established experimental fact and from there proceed to work out its inevitable consequences," and he went at that task himself with an energy and a capacity which very few people on earth possess. Thus was born the special theory of relativity. (Italics in original.)

I. THE STORY IN DIDACTIC WRITINGS

Many other prominent scientists have proposed quite similar scenarios. So it is not surprising that authors of science textbooks generally mirror, or at least do not contradict, the mythology current among the foremost research scientists.

On the question under study here, the textbooks are virtually unanimous on the history they imply. Selecting practically at random from recent physics texts on your own shelf, you will find essentially the same story as in Millikan's article.

This version is not limited to recent American textbooks, nor indeed to writers who themselves did not participate in the development of the field in its early days. Thus, Max von Laue published one of the first serious textbooks on relativity (*Das Relativitätsprinzip*) in 1911; the "Michelson experiment" is described early: "... the experiment became, as it were, the fundamental experiment for the relativity theory..."

But things get more puzzling when we find that Einstein has left in some of his readers a similar impression in at least two of his own publications. One is his early "gemeinverständliche" (generally understandable, popularized) book, *Über die spezielle und die allgemeine Relativitätstheorie* (1917). Here we find a route which was to become so familiar in almost all subsequent text presentations:

... for a long time the efforts of physicists were devoted to attempts to detect the existence of an aether-drift at the earth's surface. In one of the most notable of these attempts Michelson devised a method which appears as though it must be decisive... But the experiment gave a negative result—a fact very perplexing to physicists. Lorentz and FitzGerald rescued the theory from this difficulty by assuming that the motion of the body relative to the aether produces a contraction of the body in the direction of motion... But on the basis of the theory of relativity, the method of interpretation is incomparably more satisfactory.

While no genetic relation is presented here, it is almost irresistible to read one into the text.

The second of these early publications by

Einstein is his contribution on "Relativity Theory" to a collection of 36 essays by some of the foremost physicists, intended to convey the "state of physics in our time." [E. Warburg, Ed., *Die Physik* (B. G. Teubner, Leipzig, 1915)].

Einstein begins as follows: "It is hardly possible to form an independent judgment of the justification of the theory of relativity, if one does not have some acquaintance with the experiences and thought processes which preceded it. Hence, these must be discussed first" (p. 703). There follows a discussion of the Fizeau experiment, leading to Lorentz's theory based on the hypothesis of the stagnant ether. Despite its successes, "the theory had one aspect which could not help but make physicists suspicious" (p. 705): it seemed to contradict the relativity principle, valid in mechanics and "as far as our experience reaches, generally" beyond mechanics also. According to it, all inertial systems are equally justified. But not so in Lorentz's theory: A system at rest with respect to the ether has special properties; for example, with respect to this system, the light velocity is constant.

Einstein pinpoints the difficulty: "The successes of Lorentz's theory were so significant that the physicists would have abandoned the principle of relativity without qualms, had it not been for the availability of an important experimental result, of which we now must speak, namely Michelson's experiment." There follows a description of the experiment, and of the contraction hypothesis invoked by Lorentz and FitzGerald. Einstein adds to it sharply, "This manner of theoretically trying to do justice to experiments with negative result through *ad hoc* contrived hypotheses is highly unsatisfactory" (p. 707). The preferable conception is to hold on to the relativity principle, and to accept the impossibility-in-principle of discovering relative motion.

But how is one to make the principle of constancy of light velocity and the principle of relativity compatible after all? "Whoever has deeply toiled with attempts to replace Lorentz's theory by another one that takes account of the experimental facts will agree that this way of beginning appears to be quite hopeless at the present state of our knowledge" (pp. 707-708). Rather, one can attain compatibility of the two apparently contradictory principles through a

reformulation of the conception of space and time, and by abandoning the ether.

The rest of Einstein's short essay is concerned with the introduction of the relativity of simultaneity and of time, the transformation equations, and the length measurement of a rod moving with respect to the observer. "One sees that the above-mentioned hypothesis of H. A. Lorentz and FitzGerald for the explanation of the Michelson experiment results as a consequence of the relativity theory" (p. 712). But this result does not seem to be worthy of listing as one of the achievements of the relativity theory a little later: "We will now briefly enumerate the individual results achieved so far for which we have to thank the relativity theory" (p. 712). The list, as of 1915, was not long: "a simple theory of the Doppler Effects, of aberration, of the Fizeau experiment"; applicability of Maxwell's equations to the electrodynamics of moving bodies, and in particular to the motion of electrons (cathode rays, β -rays) "without invoking special hypotheses"; and "the most important result," the relation between mass and energy, although for that there was then no direct experimental confirmation.

Now it is essential to see that both of these publications by Einstein are frankly *didactic*. The essay of 1915 for example, is plainly not meant as an historic account of the road Einstein himself followed. The whole essay is introduced as dealing with the "justification" of the theory of relativity, not with the genesis. It is "the physicists," not Einstein himself, of whom the author says that they would have abandoned the principle of relativity without qualms if it had not been for the Michelson experiment.

Yet without having actually said anything here about his own historic route, Einstein's singling out of the Michelson experiment in his didactic writings cannot have failed to reinforce later, second-hand, didactic writings by others—even after the publication of Einstein's very different and frankly historical accounts, for example in the important interviews which R. S. Shankland published in the *Amer. J. Phys.* **31**, 47–57 (1963).

We get here a hint of a first answer to our question: the reason why there has been so much agreement on the supposed genetic role of Michelson's experiment is the frequently made confusion between science in the sense of the private ac-

tivity, the "personal struggle," and science in the sense of a developed field, a public institution.

If one takes the trouble to read the available documents carefully, one discovers a clear distinction: whenever Einstein writes or speaks on the origins of relativity in the passive voice, in answer to a direct question or in response to an obligation such as at a public lecture, he notes the importance the Michelson experiment had for the further development and acceptance of the theory by *other* physicists. But when he writes or speaks of the influence of the experiment on himself, explicitly and in first person singular, Einstein says in all letters that the effect on himself was "negligible," "indirect," "rather indirect," "not decisive," or (according to his letter to B. Jaffe, published in 1944) at most was of "considerable influence upon my work insofar as it strengthened my conviction concerning the validity" of relativity theory, adding at once "I was pretty much convinced of the validity of the principle before I did know this experiment and its results." We must therefore distinguish between Einstein's evaluations of different effects upon public science and private science. The failure to make such distinctions is among the most insidious defects of our instruction, as I shall have occasion to elaborate below.

II. WHAT DID EINSTEIN SAY TO MICHELSON?

At this point you might well confront me with one well-known document that flatly contradicts everything I have said so far. It is an account of Einstein's speech given in 1931 on the occasion of his visit to Pasadena, California, when, for the first and last time, he came face to face with Michelson. The occasion must have been moving. Michelson, 27 years his senior, was much admired by Einstein from a distance. Shankland later reported that Einstein particularly appreciated "Michelson's artistic sense and approach to science, especially his feeling for symmetry and form. Einstein smiled with pleasure as he recalled Michelson's artistic nature—here there was a kindred bond."

But Michelson was known to be no friend of relativity, the destroyer of the ether. Like so many others, Michelson was convinced that his own ill-fated experiments were the basis for the theory. Einstein reminisced later that Michelson "told me more than once that he did not like the

theories that had followed from his work," and that he had told Einstein he was a little sorry that his own work started this "monster."

Michelson was now 79 years old, weak after a serious stroke that had first forced him to his sickbed two years earlier. The picture taken on that occasion shows the frail old man, standing next to Einstein with his usual erect dignity on this last public appearance; but he was marked for death that came three months later.

Among others present at a grand dinner in the new Athenaeum in Pasadena on 15 January 1931 were distinguished physicists and astronomers: W. S. Adams, W. W. Campbell, G. E. Hale, E. P. Hubble, C. E. St. John, R. A. Millikan, R. C. Tolman, as well as Mrs. Einstein and 200 members of the California Institute Associates. Millikan set the stage with some remarks on what he saw to be the characteristic features of modern scientific thought. It is, in fact, largely the very same material Millikan was to republish 18 years later as part of his introduction for the Einstein issue of the *Reviews of Modern Physics*. But after the sentence, "Thus was born the special theory of relativity," Millikan went on to say in 1931: "I now wish to introduce the man who laid its experimental foundations, Professor Albert A. Michelson . . ."

Michelson kept his short response in the channel laid out for him:

I consider it particularly fortunate for myself to be able to express to Dr. Einstein my appreciation of the honor and distinction he has conferred upon me for the result which he so generously attributes to the experiments made half a century ago in connection with Professor Morley, and which he is so generous as to acknowledge as being a contribution on the experimental side which led to his famous theory of relativity.

In fact, Einstein had not yet responded, if the published record of the meeting is a guide [*Science* 73, 375-381 (1931)]. Millikan next called on Campbell of the splendid group of experimental astronomers, saying, "I am herewith assigning him the task of sketching the development of the experimental credentials of the general theory of relativity." Campbell recounted the success of the three chief tests, in which the California astronomers had played leading roles.

Millikan next started to introduce Einstein, but prefaced it with a last reinforcement of the philosophical message that he had been building

up. Pursuing his theme with tenacity, Millikan now referred to his own "experimental verification" of predictions contained in the early papers of Einstein. Seen from his perspective, Millikan's evaluation of Einstein's paper on the quantization of light energy (1905) was not surprising:

The extraordinary penetration and boldness which Einstein showed in 1905 in accepting a new group of experimental facts and following them to what seemed to him to be their inevitable consequences, whether they were reasonable or not as gauged by the conceptions prevalent at the time, has never been more strikingly demonstrated.

At last, the stage was set, the expectations fully aroused for Einstein's response. What happened now—or rather, what is supposed to have happened—is widely known from the account given in Michelson's only biography available so far, that of B. Jaffe [*Michelson and the Speed of Light* (Doubleday & Co., Inc., Garden City, N. Y., 1960)]. Jaffe writes:

"Einstein made a little speech. Seated near him were Michelson, Millikan, Hale, and other eminent men of science. 'I have come among men,' began Einstein, 'who for many years have been true comrades with me in my labors.' Then, turning to the measurer of light, he continued, 'You, my honored Dr. Michelson, began with this work when I was only a little youngster, hardly three feet high. It was you who led the physicists into new paths, and through your marvelous experimental work paved the way for the development of the Theory of Relativity. You uncovered an insidious defect in the ether theory of light, as it then existed, and stimulated the ideas of H. A. Lorentz and FitzGerald, out of which the Special Theory of Relativity developed. Without your work this theory would today be scarcely more than an interesting speculation; it was your verifications which first set the theory on a real basis.'

Michelson was deeply moved. There could be no higher praise for any man." (p. 168)

This was indeed the kind of response expected from the preparatory speeches; Jaffe gives a natural and clear-cut answer to the question of the possible genetic connection between Michelson's experiment and Einstein's work. He says on another page of the book simply: "In 1931, just before the death of Michelson, Einstein publicly attributed his theory to the experiment of Michelson." (p. 101)

Reading Jaffe's passage carefully we need not go so far. Michelson "stimulating the ideas" of Lorentz and FitzGerald, out of which in turn the

special theory of relativity "developed," is not a scenario in contradiction with the likely chain of events: The then-current Lorentz-FitzGerald contraction-explanation of the Michelson experiment, as found in the works of 1892 and 1895 of Lorentz which we know Einstein had read, by its unappealing *ad hoc* character, compromised further the ether-committed theory of electrodynamics that Einstein already knew for many other reasons to be inadequate. But the remarks "without your work . . . it was your verification . . ." sound indeed like a personal acknowledgement to Michelson, a public attribution of the kind that Jaffe clearly saw in it. And in that case we must confess, as Kepler put it half-way through the *Astronomia Nova*, "Dear Reader, Our hypothesis goes up in smoke."

But there is another explanation. There exists Einstein's German manuscript of his talk, as well as translations of it that were published in 1931 and 1949. From these we can see that Jaffe's widely read version of Einstein's talk has fallen into the trap that had unwittingly been set up for Einstein. Missing from Jaffe's version is a heading a little sentence, and a long ending from Einstein's talk. They make a lot of difference.

The talk starts with "Liebe Freunde!" It is, of course, addressed to the whole company. And just between the last two sentences quoted by Jaffe, we find there was another sentence that switches the discussion away from Michelson and special relativity, and toward the assembled astronomers and general relativity: "You uncovered an insidious defect in the ether theory of light, as it then existed, and stimulated the ideas of H. A. Lorentz and FitzGerald, out of which the special theory of relativity developed. *These in turn led the way to the general theory of relativity, and to the theory of gravitation.* Without your work this theory would today be scarcely more than an interesting speculation; it was your verifications which first set the theory on a real basis." (Italics supplied.) Then follows immediately an acknowledgement of experimental contributions by the California astronomers that had "furnished the real basis for the [general] theory," those by Campbell, St. Johns, Adams, and Hubble.

What emerges is still a fine compliment to Michelson. Yet even standing before him, and under the accumulated pressures of the dramatic affair, Einstein agreed neither with Millikan's

nor with Michelson's version of the genetic connection (nor, of course, with Jaffe's). He did not avail himself of the occasion to say straight out what everyone seemed to have come to hear him say, for example: "Michelson's is the crucial experiment that was the basis for my own work." He rather seems to see Michelson as one of the figures on the continuous, long way leading to relativity theory.

As to the unfortunate omission of the sentence in Jaffe's otherwise very useful book, one knows how such things happen at the most awkward point. The significance lies in this: Mistakes favor the prepared mind. And worse, through no fault of Jaffe's, his evaluation had been repeatedly republished by others who, apparently without scholarly examination of the available original text, have found comfort in his evaluation for their own purpose of forging a tight genetic link from Michelson to Einstein.

III. REASONS FOR LINKING EXPERIMENT AND THEORY

The birth of a new theory as the response to a puzzling empirical finding: this is the message clearly stated by eminent scientists such as Millikan, gladly adopted by textbooks, and found in avowedly historical accounts, too. Why is this sequence so seductive? There are half a dozen reasons. One is that almost every science textbook of necessity places a high value on clear, unambiguous, inductive reasoning. The norm of rationalism in the classroom would seem to be threatened if the text were to allow that a correct inductive generalization may be made without unambiguous experimental evidence. Hence, the likelihood is *a priori* great that any pedagogic presentation of any scientific subject will suggest a clear genetic link from experiment to theory.

Moreover, in a textbook or a didactic essay where a large amount of ground has to be covered, it is likely (for reasons of space if for no other) that the author will select one suitable experiment that can be convincingly presented, rather than a number of different experiments that in historical fact may have been equally good or better candidates.

But in the particular case of the growth of relativity theory, the author of a didactic account has an added incentive to foreshorten the period of doubt in the scientific community that actually

followed upon Einstein's 1905 publication. A student can be expected to accept more easily a theory as non-common-sensical as Einstein's if he can be shown that Einstein, or at least Einstein's contemporaries, became convinced on the basis of some clearcut experiment.

Hence little is said in such books about the sometimes dramatic battles that in fact were required for the gradual acceptance of a new theory. That lack fits in well with another moralizing function of scientific instruction, namely to underplay the scientist's private and emotional involvements in the pursuit of his own scientific work, or in the evaluation of others, to avoid seemingly to place value on his making premature commitments before "all the evidence is in," and in all these ways to introduce the student to what the textbook author usually, perhaps unconsciously, conceives to be the accepted public norms of professional behavior.

Texts probably cannot and certainly do not want to deal with the private aspect of doing science, an aspect which can be so different from scientist to scientist, and one that is so far from being fully understood in any case. It seems simpler to deal with the public aspect of science on which there is (though perhaps falsely) some consensus—the more so, in this case, as Einstein himself, in his didactic publications of 1915 and 1917, had written only of the public aspects of the rise of relativity theory. Therefore, the elements that hold a historian's attention, the elements that carry the possibility for a classic case study of the difference between private and public science—or for that matter of the relative roles of theory and experiment in modern scientific innovation, or of the quasi-aesthetic criteria for a decision between rival conceptual systems embracing the same "facts" in different ways—all these give way in textbooks to other, simpler purposes. It is just a by-product of this attitude that has caused Einstein's supposed use of the Michelson result to have become a fixed part of the folkloric consensus about the history of science, a story as widely known and believed as the story of the falling apple in Newton's garden and of the two weights dropped from the Leaning Tower in Galileo's Pisa—two other cases in which experimental fact is supposed to have provided the genetic occasion for synthetic theory.

There are yet other special circumstances that

have made it almost irresistible for pedagogic accounts to give a place of importance to the Michelson experiment. It is not just any experiment, but rather one of the most fascinating experiments in the history of physics. Its fascination has been felt equally by text writers and research physicists, and derives from its beauty and its mystery. Despite the central position of the question of ether drift in late nineteenth-century physics, nobody before Michelson was able to imagine and construct an apparatus to measure the second-order effect of the presumed ether drift. The interferometer was a lovely thing. Invented by the 28-year-old Michelson in response to a challenge by Maxwell, it was capable of revealing an effect of the order of one part in ten billion. It is to this day one of the most sensitive scientific instruments, and the experiment is one that carried precision to the extreme limit. Einstein himself later paid warm and sincere tribute to Michelson's experimental genius and artistic sense, and he added, "many negative results are not highly important, but the Michelson experiment gave a truly great result which everyone should understand."

The interpretation of the experiment, however, initially presented mysterious difficulty on two levels. One was understanding the way the apparatus works in the context of ether theory, regardless of the meaning of its outcome. On this score, one finds nowadays everywhere some adequate though quite oversimplified outlines of the experiment. But Michelson himself, on presenting an account of his first experiment to the Académie des Sciences in 1882, acknowledged he had made an error in his earlier report of 1881 and had neglected the effect of the earth motion on the path of light in the interferometer arm at right angles to the motion. Potier, who had pointed out the error to Michelson in 1882, was in error also. A debate continued for over 30 years on the question how the moving reflector affects the angle of reflection.

But beyond that, the findings obtained with the instrument were enormously puzzling to everyone at the time, and for many remained so for a long time afterwards. A significant displacement of the interference fringes had been expected, owing to the presumed effect of the ether on the motion of light, but virtually none was obtained. The glorious device had yielded a puzzling, disap-

pointing, even incomprehensible result in the context of the then-current theory. Michelson himself called his experiment a "failure"; the repeatedly obtained null or nearly null results were contrary to all his expectations. And contrary to the stereotype that the true scientist accepts the experimental test that falsifies a theory, he refused to grant the importance of his own result, saying, "Since the result of the original experiment was negative, the problem is still demanding a solution." He even tried to console himself with the remarkable observation that "the experiment is to me historically rich because it was for the solution of this problem that the interferometer was devised. I think it will be admitted that the problem, by leading to the invention of the interferometer, more than compensated for the fact that this particular experiment gave a negative result."

Others were just as mystified and displeased. Lorentz wrote to Rayleigh on 18 August 1892:

I am utterly at a loss to clear away this contradiction, and yet I believe if we were to abandon Fresnel's theory [of the ether], we should have no adequate theory at all. . . . Can there be some point in the theory of Mr. Michelson's experiment which has as yet been overlooked?

Lord Kelvin could not reconcile himself to the negative findings into the 1900's. Rayleigh, who, like Kelvin, had encouraged Michelson to repeat his first experiment, confessed he found the null result obtained by Michelson and Morley in Cleveland to be "a real disappointment."

As L. S. Swenson has pointed out, Michelson and Morley were so discouraged by the "null" result of their experiment in 1887 that they disregarded the promise in their original paper that the measurements, which they had taken during only 6 hours within one 5-day period, "will therefore be repeated at intervals of 3 months, and thus all uncertainty will be avoided." Instead, Michelson stopped their work on this experiment, and turned to the new use of the interferometer for measuring lengths. (As it turned out, it was a wise move. That was the work that led to his Nobel Prize award.) In short, to everyone's surprise, including Michelson's, the experiment had turned out to be one of "test," not merely of "application," to use the terminology of Duhem. It is therefore rather ironic that neither Michelson nor Einstein, in their different ways, considered the

famous experiment as decisive for himself, not to speak of "crucial." However, for most other theoreticians it became a crucial experiment *malgré lui* in the only valid sense of the term, namely, as the pivotal occasion causing a significant part of the scientific community to re-examine its previously held basic convictions.

If the result of the Michelson experiment was a mystery for a long time, the relativity theory for its part was even more mysterious at its announcement in 1905, and remained so for some time afterwards. It took a few years before one could say that even among German scientists there was a preponderance of opinion in favor of it. The turning point came perhaps with the publication of Minkowski's address on "Space and Time" in 1909. Among physicists outside Germany the lag was much longer, and among most teachers, students, and the general public, the lag was longer still.

It now seems almost inevitable that in this situation there occurred a natural act of symbiosis, especially in the didactic literature. It was the marriage of the puzzling Michelson experiments and the all-but-incomprehensible relativity theory. The undoubted result of Michelson's experiments could be thought to provide an experimental basis for the understanding of relativity theory which otherwise seemed contrary to common sense itself. And the relativity theory in turn could provide an explanation for Michelson's experimental result in a manner not as "artificial" or *ad hoc* as reliance on the supposed Lorentz-FitzGerald contraction was widely felt to be.

This strategy was the more necessary for proponents of relativity theory because, after its publication, no new experimental results came forth for many years which could be used to "verify" this theory in the way most physicists are used to looking for verification. On the contrary, as I have indicated elsewhere, the very first response within the scientific community to Einstein's relativity paper of 1905, and in the same journal in which he had published it, was a categorical experimental disproof of the relativity theory by W. Kaufmann. It took until 1915 for Kaufmann's experimental equipment to be shown to have been defective, and for unambiguously favorable results to be established. Max Planck noted in 1907 that Michelson was then still regarded as provider of the only experimental

support. The physicist W. Wien was not convinced of the theory until 1909; and then it was not by any clearcut evidence from experiment, but on more nearly aesthetic grounds, in words which Einstein must have appreciated and which to this day are applicable:

What speaks for it [the theory] most of all, however, is the inner consistency which makes it possible to lay a foundation having no self-contradictions, one that applies to the totality of physical appearances, although thereby the customary conceptions experience a transformation.

What finally made special relativity theory a widely accepted basic part of physics throughout the scientific community were developments far from the scope of Einstein's 1905 paper itself. Foremost among these developments were the *experimental* successes such as the eclipse expedition of 1919, the use of relativistic calculations to explain the fine structure of spectral lines, the Compton effect, etc. In the meantime, for the interested public and for many physicists, Michelson's result was the crutch that supported relativity theory, particularly in the face of its challenging paradoxes and iconoclastic demands.

To summarize, the particular missions of pedagogic accounts backed up by the popular writings of distinguished experimental physicists, and the particular history surrounding the acceptance of the Michelson results and the Einstein publication, together provided two sets of pressures that tended to the same end, namely to proclaim the existence of a direct genetic link between Michelson's and Einstein's work.

I believe these pressures can also be traced to a common root, namely, to a certain philosophical view concerning science as a whole. This view was supported by a vocal group of philosophers in the U. S. and Europe, and was widely current after the victories of the empiricist schools around the turn of the century.

IV. THE DOCTRINE OF EXPERIMENTICISM

There exists a view of science at the extreme edge of the time-honored tradition of empiricism which, for want of a better name, will here be called *experimenticism*. It is best recognized by the unquestioned priority assigned to experiments and experimental data in the analysis of how scientists do their own work and how their work is incorporated into the public enterprise of science. A few examples will suffice to indicate the per-

vasiveness of this attitude. With specific reference to relativity theory, it is well illustrated by Ernst Mach's disciple Joseph Petzoldt, the moving spirit behind the Gesellschaft für positivistische Philosophie of Berlin and its journal, *Zeitschrift für positivistische Philosophie*. As lead article of its inaugural issue (Vol. 1, 1913), he printed the text of the speech he had delivered at the opening session of the Gesellschaft on 11 November 1912. With relativity theory, he said there had come "the victory over the metaphysics of absolutes in the conceptions of space and time," and a "fusion of mathematics and natural science which at last and finally shall lead beyond the old rationalistic, Platonic-Kantian prejudice." But the fixed hinge on which this desired turn of events moved was, again, the Michelson experiment:

Clarity of thinking is inseparable from knowledge of a sufficient number of individual cases for each of the concepts used in investigation. Therefore, the chief requirement of positivistic philosophy: greatest respect for the facts. The newest phase of theoretical physics gives us an exemplary case. There, one does not hesitate, *for the sake of a single experiment*, to undertake a complete reconstruction. The Michelson experiment is the cause and chief support of this reconstruction, namely the electrodynamic theory of relativity. To do justice to this experiment, one has no scruples to submit the foundation of theoretical physics as it has hitherto existed, namely Newtonian mechanics, to a profound transformation. (Italics supplied)

The real enemy and the full ambitions were both revealed again in the next volume (1914), where Petzoldt wrote, "Lorentz's theory is at its conceptual center pure metaphysics, nothing else than Schelling's or Hegel's *Naturphilosophie*." Again, the Michelson experiment, as the one and only experiment cited, is given the credit for ushering in the new era: "... the Einsteinian theory is entirely tied to the result of the Michelson experiment, and can be derived from it." Einstein himself "from the beginning conceived of the Michelson experiment relativistically. We are dealing here with a principle, a foremost postulate, a particular way of understanding the facts of physics, a view of nature, and finally a Weltanschauung The sequence Berkeley-Hume-Mach shows us our direction and puts into our hands the epistemological standard."

A few years before, Michelson had been awarded the 1907 Nobel Prize in Physics "for his optical precision instruments and the search which he has

carried out with their help in the fields of precision metrology and spectroscopy." The relativity theory was, of course, still far too new and was regarded as too speculative to be mentioned in the citations or responses; indeed, at the time Petzoldt was writing his eulogies to it, the theory had become too speculative for Mach himself. (The Nobel Prize Committee did not award Einstein's prize until 1922, and then, as Einstein was specifically reminded by the Committee, it was for the experimentally well-confirmed work on the photoelectric effect). In any case, theory was not of interest on that day in 1907. As the presentation address by K. B. Hasselberg showed, the award to Michelson was clearly motivated by the same experimenticist philosophy of science of which we have had ample indications. Hasselberg said,

As for physics, it has developed remarkably as a precision science, in such a way that we can justifiably claim that the majority of all the greatest discoveries in physics are very largely based on the high degree of accuracy which can now be obtained in measurements made during the study of physical phenomena. (Accuracy of measurement) is the very root, the essential condition, of our penetration deeper into the laws of physics—*our only way to new discoveries*. It is an advance of this kind which the Academy wishes to recognize with the Nobel Prize for Physics this year. (*Italics supplied*).

Somehow, everyone managed to keep a decorous silence on the experiment which Petzoldt and others of his persuasion were soon to hail as the crucial turning point for physics and for Weltanschauung. Nobody referred here to Michelson's ether-drift experiments—neither the Swedish hosts, nor Michelson himself in his responding lecture ("Recent Advances in Spectroscopy"). It was as embarrassing an experiment now for experimenticists with ether-theoretic presuppositions as it was welcome later for experimenticists with relativistic presuppositions.

What about the philosophers of science? In their work, the discussion of relativity theory is quite frequently found linked tightly to the Michelson experiment, though rarely more enthusiastically than in one of the essays, entitled "The Philosophical Dialectic of the Concepts of Relativity," collected in honor of Einstein in Schilpp's volume:

As we know, as has been repeated a thousand times, relativity was born of an epistemological

shock; it was born of the "failure" of the Michelson experiment . . . To paraphrase Kant, we might say that the Michelson experiment roused classical mechanics from its dogmatic slumber . . . Is so little required to "shake" the universe of spatiality? Can a single experiment of the twentieth century [*sic*] annihilate—a Sartrean would say *neantiser*—two or three centuries of rational thought? Yes, a single decimal sufficed, as our poet Henri de Regnier would say, to "make all nature sing." [P. Schilpp Ed., *Albert Einstein, Philosopher-Scientist* (Library of Living Philosophers, Evanston, Illinois, 1949), pp. 566-568.]

Einstein chose not to respond to this apotheosis of the Michelson experiment in his replies to critics published at the end of the same volume. But he makes a lengthy and subtly devastating reply to another essay in this collection, that by Hans Reichenbach (on "The Philosophical Significance of the Theory of Relativity"), which has the same kind of experimenticist basis.

We must remember that over the years Reichenbach was one of the most persistent and interesting philosophical analysts of the epistemological implications of relativity, and he published several attempts to cast the theory into axiometric form. But Reichenbach's empiricist conviction never flagged. For example, he wrote that Einstein's work "was suggested by closest adherence to experimental facts . . . Einstein built his theory on an extraordinary confidence in the exactitude of the art of experimentation." [H. Reichenbach, *From Copernicus to Einstein* (Philosophical Library, New York, 1942), p. 51.] The only specific historic experiment Reichenbach associates with the genesis of Einstein's theory is, of course, the Michelson experiment; for example, "the theory of relativity makes an assertion about the behavior of rigid rods similar to that about the behavior of clocks . . . This assertion of the theory of relativity is based mainly on the Michelson experiment." [*The Philosophy of Space and Time* (Dover Publishing Co., New York 1957); a translation by M. Reichenbach and John Freund of H. Reichenbach, *Die Philosophie der Raum-Zeit Lehre*, Walter de Gruyter & Co., Berlin, (1928), p. 195.] In fact Reichenbach and his faithful followers prefer to give a crucial place to the Michelson experiment in the supposed development rather merely showing the usefulness of the relativity theory. Thus Reichenbach claims, "It would be mistaken to argue that Einstein's

theory gives an explanation of Michelson's experiment, since it does not do so. Michelson's experiment is simply taken over as an axiom." (*op. cit.* p. 201).

In Reichenbach's essay in Schilpp's collection of 1949 honoring Einstein, he reverts to the same points (e.g. p. 301), but they are only preludes to the conclusion that "it is the philosophy of empiricism, therefore, into which Einstein's relativity principle belongs. . . . In spite of the enormous mathematical apparatus, Einstein's theory of space and time is the triumph of such a radical empiricism in a field which had always been regarded as a reservation for the discoveries of pure reason," (pp. 309-310).

In his reply to this essay at the end of the volume, Einstein devoted most of his attention to a denial of this claim. He preferred to hold fast to the basic conceptual distinction between "sense impressions" and "mere ideas"—despite the expected reproach "that, in doing so, we are guilty of the metaphysical 'original sin.'" (p. 673). Einstein pleads that one must also accept features not only of empiricism but also of rationalism, indeed that a "wavering between these extremes appears to be unavoidable." (p. 680). He adopts the role of the "non-positivist" in an imaginary dialogue with Reichenbach, and urges the useful lesson of Kant that there are concepts "which play a dominating role in our thinking, and which, nevertheless, cannot be deduced by means of a logical process from the empirically given (—a fact which several empiricists recognize, it is true, but seem always again to forget)." (p. 678).

The difference is, of course, one of relative scientific taste or style. To Reichenbach, the interest in a scientific theory does not reside in the details of its historical development in the work of an actual person. As Reichenbach said honestly, "The philosopher of science is not much interested in the thought processes which lead to scientific discoveries; he looks for a logical analysis of the completed theory, including the relationships establishing its validity. That is, he is not interested in the context of discovery, but in the context of justification." (*Ibid.*, p. 292).

Unfortunately, however, Reichenbach and his followers have not always remembered his laudable attempt to distinguish clearly between private and public science, nor have they always adhered to his wise disclaimer of interest in the

thought processes leading to the discovery. The desire to see a theory as a logically complete structure arising from empirical observations brings them, after all, to assume presumed historical sequences on the road that led to the discovery. Thus, implicit history is produced after all, e.g., that "Einstein incorporated its [the Michelson experiment's] null result as a physical axiom in his light principle," and similar attempts at "the unravelling of the history" of relativity theory.

When such attempts are then faced with some of the direct evidence against the priority and importance of the Michelson experiment in Einstein's thinking, the response is this: without the genetic role of this particular experiment, an understanding of the discovery of the theory would become "quite problematic," and one would be left "puzzled concerning the logical, as distinct from psychological grounds which would then originally have motivated Einstein to have confidence in the principle of relativity without the partial support of the Michelson-Morley experiment. . . ." [A. Grünbaum, *Philosophical Problems of Space and Time* (Alfred A. Knopf, Inc., New York, 1963), p. 381.] Not surprisingly, Jaffe's authority is invoked at that very point to the effect that, "In 1931, just before the death of Michelson, Einstein publicly attributed his theory to the experiment of Michelson." (*Ibid.*)

To Einstein himself, the possibility of a lack of sufficiently secure "logical" grounds in the original motivation was not a puzzle. It was a well recognized fact of actual scientific work. Indeed, he went out of his way to make this point. For example, when asked to provide a statement honoring Michelson at the Centenary celebration on 19 December 1952, Einstein stressed that "The influence of the crucial Michelson-Morley experiment upon my own efforts has been rather indirect," and he ended with a paragraph that is as significant as it may have appeared gratuitous on that occasion:

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

This is entirely in accord with the honest self-appraisal of the experience many a creative scientist has had in his own work. Einstein was perhaps more forthright in this confession, one

that is so far from the still widely current myths which present scientific research as the inexorable result of the pursuit of logically sound conclusions from experimentally indubitable premises. The truth, alas, is different.

Einstein freely explained that matters are not so simple, e.g., in speaking to Shankland about the origins of his own work of 1905, Shankland reported [Amer. J. Phys. 31, 48 (1963)]:

This led him to comment at some length on the nature of mental processes in that they do not seem at all to move step by step to a solution, and he emphasized how devious a route our minds take through a problem . . . It is only at the last that order seems at all possible in a problem.

Similarly, in commenting on the correct view a historian should take of the work of physicists, Einstein told him: "The struggle with their problems, their trying everything to find a solution which came at last often by very indirect means, is the correct picture." (*Ibid.*, p. 50). Contrary to the expectations of systematizers, axiomatizers, text writers, and others who, as we have seen, yearn for linearized sequences both in scientific work itself and in accounts given of such work, Einstein opposes with the gentle warning that there is no straightforward logical way, at least not to a theory of such magnitude as that contained in his work of 1905.

It was, after all, a warning Einstein had made repeatedly—from about 1918 on, and more emphatically from about the early 1930's. Examples may be found in his essay for Max Planck in 1918: "There is no logical way to the discovery of these elementary laws. There is only the way of intuition" (based on *Einfühlung* in experience). It is repeated in his Herbert Spencer lecture of 1933 (concerning the "purely fictitious character of the fundamentals of scientific theory"), and in his Autobiographical Notes written in 1946 ["A theory can be tested by experience, but there is no way from experience to the setting up of a theory" (p. 89)]. Again, when J. Hadamard asked Einstein for a self-analysis of his thought processes, Einstein replied

The words or the language, as they are written or spoken, do not seem to play any role in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images which can be "voluntarily" reproduced and combined.

In believing that there can be an essential abyss between experience and logically structured theory, and in believing also in the related distinction between "sense impressions" on the one hand and "mere ideas" on the other, Einstein separated himself from most of the prominent philosophies of science of his time. That he did not do this lightly we know from many evidences, including the frequency with which he kept coming back to reiterate these points over the years.

V. A LESSON FOR THE HISTORIAN OF SCIENCE

Historians are quite used to finding large discrepancies between the documentable history of science on the one hand, and, on the other, the popular history found in texts and in the writings of some eminent scientists and some philosophical analysts. We have looked here at a quite limited case, but there were some more widely applicable conclusions. Above all, we are forced to ask anew what are the most appropriate styles and functions of historical scholarship today, particularly against the background of the prevailing experimentalist doctrine. On this point, Einstein's own opinion is illuminating. Shankland had asked Einstein during their first conversation in 1950 whether "he felt that writing out the history of the Michelson-Morley experiment would be worthwhile":

He said, 'Yes, by all means, but you must write it as Mach wrote his *Science of Mechanics*.' Then he gave me his ideas on historical writing of science. 'Nearly all historians of science are philologists and do not comprehend what physicists were aiming at, how they thought and wrestled with their problems. Even most of the work on Galileo is poorly done.' A means of writing must be found which conveys the thought processes that lead to discoveries. Physicists have been of little help in this, because most of them have no 'historical sense.' Mach's *Science of Mechanics*, however, he considered one of the truly great books and a model for scientific historical writing. He said, 'Mach did not *know* the real facts of how the early workers considered their problems,' but Einstein felt that Mach had sufficient insight so that what he said is very likely correct anyway. The struggle with their problems, their trying everything to find a solution which came at last often by very indirect means, is the correct picture.' (*op. cit.*, p. 50).

In discussing the approach of "nearly all historians" (perhaps somewhat too brusquely), Einstein accentuates the need for historical work

to deal with the private phase of scientific effort—how a man thinks and wrestles with a problem. In discussing the physicists themselves (perhaps also somewhat too brusquely), Einstein accentuates the need for a particular kind of historical sense, one that largely intuits how a scientist may have proceeded, even in the absence of "the real facts" about the creative phase. It is a challenging statement, nothing less than a recommendation to adopt for research in the history of science a lesson Einstein had learned from his research in physics: *Just as for doing physics itself*, Einstein here advises the historian of science to leap across the unavoidable gap between the necessarily too limited "facts" and the mental construct that must be formed to handle the facts. And in such an historical study, *as in physics itself*, the solution comes often "by very indirect means," and the best outcome one can hope for is not certainty but only a probability of being "correct anyway."

One can well agree with this call for new ways of writing about the thought processes that led to major discoveries, without having to agree at this



FIG. 2. Einstein on bicycle in front of Hale's Solar Laboratory during his 1931 visit to Pasadena. (Courtesy of California Institute of Technology.)

late date with the particular model of Mach's *Science of Mechanics*. The most obvious difficulty with following Einstein's advice is of course the unspecifiability of "sufficient insight." Another is that any study of the processes of discovery—that evanescent, partly unconscious, unobserved, un-verbalized, unreconstructable activity—is by definition going to yield a report with apparently vague and contradictory elements. Yet another is that the invitation to leap courageously may cause even some of the most pertinent and easily available documents (historical "facts") to be overlooked. And a fourth trouble is that there are some problems which now seem largely unsolvable by any method, and may remain so for a long time: the problem of genius, of reasons for thematic and aesthetic choices, of interaction between private and public science, not to speak of the problem of induction.

Ernst Mach himself would perhaps have objected to Einstein's characterization of his work on the history of science, laudatory though it was intended to be. But Einstein was right nevertheless in ascribing the Mach, and recommending to others, an unconventional method, despite the difficulties and dangers it may pose. For in this way one can at least hope to penetrate beyond the more pedestrian or trivial aspects of an historic case of such magnitude, to recognize more fully the feat of intellectual daring and superb taste that was needed to create the theory.

Of course, experiments are essential for the progress of science. Of course, the chain from a puzzling new experiment to a theoretical scheme that explains it is the more usual process, particularly in the everyday accomplishments of most scientists. Of course, experiments influenced also the developing thought processes of the young Einstein struggling with the problem of understanding electrodynamics in a new way, to get at the "heart of the matter." Of course, Michelson's experiment played an indirect role in this, if only because Einstein found one inadequacy in H. A. Lorentz's theory of electrodynamics to be that "it was leading to an interpretation of the result of the Michelson-Morley experiment which seemed to be artificial," as Einstein wrote in his Michelson Centenary message.

And yet, the experimenticist fallacy of imposing a logical textbook sequence, rigorously from experiment to theory, must be resisted. Not only

is it false to the actual development of historic cases of thought processes that may have led to major scientific discoveries. Not only might the doctrine, if taken seriously, inhibit creative work in science. But worse, by drawing attention primarily to the externally visible clay that provides factual support and operational usefulness for the developed theory, it does not do adequate justice to the full grandeur of the theory. The basic

achievement of Einstein's theory was not to preserve hallowed traditional concepts or mechanisms; it was not to produce a logically tightly structured sequence of thoughts; it was not to build on a beautiful and pedagogically persuasive experiment. Rather, the basic achievement of the theory was that even at the cost of sacrificing all these, it gave us a new unity in the explanation of nature.

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The Lorentz Theory of Electrons and Einstein's Theory of Relativity

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The development of Lorentz' theory of electrons is reviewed insofar as it relates to the problem of the electrodynamics of moving bodies. It is shown that the principle of relativity did not play an important role in the Lorentz theory, and that though Lorentz eventually realized the distinctions between his own work and that of Einstein, he was unwilling to completely embrace the Einstein formulation and thereby reject the ether.

INTRODUCTION

Almost to a man, his peers and colleagues have referred to H. A. Lorentz as one of the greatest physicists of the latter quarter of the nineteenth

century and the first quarter of the twentieth century. Lorentz had a very wide range of interests,¹ however, in this paper I explore only the development of Lorentz' theory of electrons

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¹H. A. Lorentz (1853-1928) spent most of his active years as Professor of Theoretical Physics at the University of Leiden in a chair specifically created for him. (G. L. de Haas-Lorentz, in G. L. de Haas-Lorentz (ed.), *H. A. Lorentz: Impressions of his Life and Work* (North-Holland Publ. Co., Amsterdam, 1957), p. 34.)

Of the many accolades bestowed on Lorentz, only a few can be mentioned here. According to A. D. Fokker, "When we review Lorentz' *opera in toto*, it becomes clear that he took over the nineteenth century, scientifically speaking, into the twentieth [century]... No one could have advanced classical theory farther than he did... He drew from it the utmost consequences." (A. D. Fokker, in *H. A. Lorentz: Impressions of his Life and Work*, p. 78.) At the time of Lorentz' death, Leigh Page commented that "... The World has lost one of the great mathematical physicists of the last century." (L. Page, *Amer. J. Sci.* **15**, 374 (1928).) Max Born referred to Lorentz as, "... The leader and representative of a period of physics that we contrast today as the classical period of our discipline to the newly raised, revolutionary developments of 1900." (M. Born, *Nachr. Akad. Wiss. Göttingen, Math.-physik. Kl., Mitt.* (1928-29), p. 69.)

Writing in 1955 just prior to his own death, Einstein said, "At the turn of the century, H. A. Lorentz was

regarded by theoretical physicists of all nations as the leading spirit; and this with the fullest justification. No longer, however, do physicists of the younger generation fully realize, as a rule, the determinant part which H. A. Lorentz played in the formulation of the basic principles of theoretical physics. The reason for this curious fact is that they have absorbed Lorentz' fundamental ideas so completely that they are hardly able to realize the full boldness of these ideas and the simplification which they brought into the foundations of the science of physics." (A. Einstein, in de Haas-Lorentz, *loc. cit.*, p. 5.)

Lorentz shared the Nobel Prize for 1902 with his student Zeeman. His analytic powers can be illustrated by the story Born tells of the work for which they won the prize: Lorentz heard Zeeman describe the effect named after him at the session of the Amsterdam Academy held on the last Sunday in Oct., 1896. He made public his explanation of the effect the following day complete with his prediction of the polarization of the "Zeeman spectral lines." (Born, *loc. cit.*, p. 72.)

Lorentz' work was not confined to the physics to be described in the text below. Besides his work on the electron theory, which he applied to phenomena in moving bodies, he used the theory to explain conduction in metals, heat flow, reflection, and refraction, and other optical and physical phenomena. (Cf. H. A. Lorentz, *The Theory of Electrons* (Leiden, 1909; revised ed., 1915; repr., Dover