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THE EFFECTS OF NATIONAL SCIENTIFIC STYLE ON THE UNDERSTANDING OF SCIENTIFIC INNOVATION--SPECIAL RELATIVITY, A CASE HISTORY. FINAL REPORT.

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COMPARED ARE THE RESPONSES TO EINSTEIN'S THEORY OF RELATIVITY IN FOUR COUNTRIES BETWEEN THE YEARS 1905 AND 1911. THE COUNTRIES STUDIED ARE GERMANY, FRANCE, ENGLAND, AND THE UNITED STATES. ON THE BASIS OF THE RESPONSE, NATIONAL SCIENTIFIC STYLES ARE IDENTIFIED, AND THESE STYLES ARE RELATED TO PREVIOUS NATIONAL CHARACTERISTICS OF DOING SCIENCE AND TO THE STRUCTURE OF THE EDUCATIONAL SYSTEM IN THE FOUR COUNTRIES. (DS)

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Yellow Springs, Ohio

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Resume

This study is a comparison of the response to Einstein's theory of relativity in four countries between the years 1905 and 1911. The countries studied were Germany, France, England and the United States. This accounts for over 99% of the literature on the subject during those years. The major hypothesis of the study is that such a comparison will reveal national scientific styles which can in part be characterized by certain typical responses to any scientific innovation.

That there were gross national differences in the response to the Theory of Relativity there can be no question. In the United States the theory was at first ignored and then rejected as being contrary to common sense and impractical. It was only made acceptable to the scientific community after that community had become convinced that the entire theory, from postulates to conclusions was demonstrable by experiment. In England, the theory received scant attention at first and then was almost uniformly rejected as not being consistent with what the English believed they knew about the characteristics of electromagnetic radiation. It was not acceptable until it was translatable into a form which made it compatible with a luminiferous ether. In France, there was no response to Einstein's theory. As we will develop later, for a variety of reasons, Henri Poincaré chose to ignore the theory. As Poincaré went, so went France. Only in Germany does one find the kind of dispute, articulation and dialogue that one would expect to find with the advent of innovation. No doubt this is in part due to the fact that Einstein himself was German, but it cannot explain all of the differences. Others knew of the theory but only

in Germany was there a characteristic exhibited which I now consider to be a crucial variable for the development of understanding of a scientific theory--elaboration. It was only in Germany that Einstein's theory was not only examined and discussed on its own merits, but was elaborated on: extended and modified in the light of recognized shortcomings.

Among the many factors that might affect such response, the study focusses on the effects of the structure of the educational system in each of the countries. In England, as we will show, almost all mathematical physicists trained at Cambridge in a particularly uniform rigorous pattern, in France the education of scientists was ultimately in the control of the Academie des Sciences, and Henri Poincare was the most influential personage in that organization. In the United States, the education of scientists was in its most rudimentary and pragmatic form, there being at the time only two or three recognized graduate centers: Harvard, John Hopkins, and Yale. One recalls that the great Willard Gibbs could not get a job at Yale in the sciences, but was hired to teach Classics. Only in Germany was there a multivarious educational system: Great centers of learning vying for great men, students traveling between institutions exposed to widely differing viewpoints, and active and public confrontation and competition within the structure of the educational system; only in Germany was their Wissenschaft an ethos which dictated not only that the results of research be worthwhile, but which put a premium on the proper conduct for obtaining those results.

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The Effects of National Scientific Style on the Understanding of Scientific Innovation: Special Relativity, A Case History

I. Introduction

At the end of the nineteenth century, J.T. Merz produced a monumental document--A History of European Thought in the Nineteenth Century.[1] The work is monumental on several counts--first for the breadth and depth of the undertaking--there seemed to be nothing outside of Merz's ken, and second for the clearness with which Merz saw the century just past, a century in which he himself had lived, had been trained, and had plied his trade of intellectual history. That a contemporary could see such a large part of the shape of the forest in the midst of all the trees is itself remarkable. Of course Merz must have realized the difficulty of writing about the recent past, especially a past which he himself had helped build. One of his theses and one which reflects something of Merz's own intellectual characteristics, was that there was _a_ Scientific Spirit in Europe and that this scientific spirit which had begun in France, had spread to Germany and thence across the channel to England was now solidified and unified:

> A hundred years--even fifty years--ago, it would have been impossible to speak of European Thought in the manner in which I do now. For the seventeenth and eighteenth centuries mark the period in which there grew up first the separate literature and then the separate thought of the different countries of Western Europe. Thus it was that in the last

1. J.T. Merz, _A History of European Thought in the Nineteenth Century_(4 vol.; Chicago: William Blackwood and Sons, 1904-12) This book has recently been reprinted by Dover Publications, 1965.

> century and at the beginning of this, people could make journeys of exploration in the regions of thought from one country to another, bringing home with them new and fresh ideas . . . .
> . . . . In the course of our century Science at least has become international: isolated and secluded centres of thought have become more and more rare. Intercourse, periodicals, and learned societies with their meetings and reports, proclaim to the whole world the minutest discoveries and the most recent developments. National peculiarities still exist, but are mainly to be sought in the remoter and more hidden recesses of thought, suggest, rather than clearly express a struggling but undefined idea . . . .We can speak now of European thought, when at one time we should have had to distinguish between French, German and English thought. 2

It was this point of view, as expressed by Merz, which can be cited as motivating the present study. When I first read Merz's book, I was skeptical that one could in fact, identify such a scientific spirit exhibiting the kind of uniformity that Merz implied. Nor did I think that one would have to search out the "remoter and more hidden recesses of thought" to find significant differences. This skepticism was based on the belief that national differences persist, despite communication and that these national differences are more than a matter of the difficulty of language translation. They arise as part of the metaphysical baggage acquired by every mother's son, by every teacher's student. Coupled with my interest in the history of the Theory of Relativity and the problems of communicating to students other than scientists the inner vitality and creative tensions that grow out of differences between scientists, the study almost outlined itself: National differences in the response to Einstein's Theory of Relativity. The countries chosen were France, Germany,

2. *Ibid.* Vol I, pp. 16-20.

England, and the United States. The first three of these are those treated by Merz. The period of time chosen was the years 1905 to 1911. During that period, over 99% of the literature on Einstein's theory emanated from the four countries[3] and in the year 1911, with the first Solvay Conference, the thoughts of many of the contributors to the theory of relativity turned to matters of quantum mechanics. While not fully accepted, the formulas, if not the spirit of relativity was recognized as being necessary.

Hopefully, such a study might also shed some light on the relationship between the development of intellectual understanding on the part of a culture and the teaching of the same understanding. For it is clear that one can only teach (and for that matter learn) within the limits proscribed by understanding and that understanding, is limited by, among other things, conditions peculiarly local, including perhaps, chauvinistic loyalties (e.g. the English reification of Newton) and localized educational modes. In effect this would lead to re-inforcing pattern in which the educational establishment created the framework of understanding and that understanding in turn propagated the educational establishment.

That there were gross national differences in the response to the Theory of Relativity there can be no question. In the United States the theory was at first ignored and then rejected as being contrary to common sense and impractical. It was only made acceptable to the scientific community after that community had become convinced that the entire theory, from postulates to

3. M. Lecat, Bibliographie de la relativité (Bruxelles: Maurice Lamertin, 1924), pp. 201-202

conclusions was demonstrable by experiment. In England, the theory received scant attention at first and then was almost uniformly rejected as not being consistent with what the English believed they knew about the characteristics of electromagnetic radiation. It was not acceptable until it was translatable into a form which made it compatible with a luminiferous ether. In France, there was no response to Einstein's theory. As we will develop later, for a variety of reasons, Henri Poincaré chose to ignore the theory. As Poincaré went, so went France. Only in Germany does one find the kind of dispute, articulation and dialogue that one would expect to find with the advent of innovation. No doubt this is in part due to the fact that Einstein himself was German, but it cannot explain all of the differences. Others knew of the theory but only in Germany was there a characteristic exhibited which I now consider to be a crucial variable for the development of understanding of a scientific theory--elaboration. It was only in Germany that Einstein's theory was not only examined and discussed on its own merits, but was elaborated on: extended and modified in the light of recognized shortcomings.

As the investigation of the response to Einstein's theory progressed, it became increasingly clear that there was one variable between countries which seemed to be crucial and which by itself although not sufficient, did help provide a context in which to understand the patterns of differing responses between countries. The variable in question was the _structure_ of the educational system. In England, as we will show, almost all mathematical physicists trained at Cambridge in a particularly uniform rigorous pattern, in France the education of scientists was ultimately in the control of the _Academie des Sciences_, and Henri Poincaré was the most influential personage in that organization. In the United States, the education of scientists was in its most rudimentary and pragmatic form, there being at the time only two or three recognized graduate centers: Harvard, Johns Hopkins, and Yale. One recalls that the great Willard Gibbs could not get a job at Yale in the sciences, but was hired to teach Classics. Only in Germany was there a multivarious educational system: great centers of learning vying for great men, students traveling between institutions exposed to widely differing viewpoints, and active and public confrontation and competition within the structure of the educational system; only in Germany was their Wissenschaft[4] an ethos which dictated not only that the results of research be worthwhile, but which put a premium on the proper conduct for obtaining those results.

While it would be a mistake to make a causal connection between the responses to Einstein's theory and the structure of the University systems, the elucidations of each of these factors helps to shed light on the other. No doubt, to some extent, the responses to scientific innovation and the particular structures of the University systems were both shaped by common factors. While we will not dwell on that question here, it is because of the excellence of sholarship by men like Merz that we can feel confident that answers will be forthcoming.

The study is divided into five sections. The first section deals with two major theoretical schemes, by Lorentz and by Abraham, which were proposed prior to the introduction of Einstein's theory to explain the same range of phenomena. These two theories are singled out because it was against these

4. Cf. Merz, loc. cit., pp. 202 ff.

theories that the theory of relativity was most often compared. Following the survey of the Lorentz and Abraham theories, the response in each of the countries is examined beginning with Germany and then in turn, France, England and the United States. The significance of this order is only that it allowed for the most natural and convenient development of the ideas under consideration.

* * * * *

I would like to take this opportunity to thank the many people who have aided and supported me during the course of this enterprize. I am especially grateful to Professor Vernon Cannon (Antioch College), Professor Leonard Nash (Harvard University), the late Professor G.E. Owen (Antioch College) and Professor Gerald Holton (Harvard University). In one way or another, each of these men seriously modified and shaped by own development at crucial points in my life. I am particularly indebted to Professor Holton who was gracious enough to open to me a significant part of his own work. Without his encouragement, his aid and support this study would never have come to fruition.

Financial assistance for the prosecution of this research came from several sources. I thank the Office of Education and Antioch College, both of whom provided financial support at different stages of the work.

Finally, like many of those who came before me, I pay homage to my wife and my family who have borne the brunt of the human burden that is a necessary part of the scholarly undertaking. They have been magnificent.

II. Theories Prior to Einstein's Special Theory of Relativity

The work of Einstein in the area of the Special Theory of Relativity has, on the one hand, been closely linked with that of H.A. Lorentz, and on the other hand, been constrasted sharply with the work of Max Abraham. A brief analysis of both of these theories will facilitate later discussion.

A. The Lorentz Theory Applied to Systemsin Motion with Respect to Each Other

H.A. Lorentz was a great man. Almost without exception, his peers and colleagues have referred to him as one of the greatest physicists of the latter quarter of the nineteenth century and the first quarter of the twentieth century.[5]

5. H.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, "Reminiscences," in G.L. de Haas-Lorentz(ed), H.A. Lorentz: Impressions of his Life and Work(Amsterdam: North-Holland Publishing Co., 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, "The Scientific Work," in Ibid., p. 78.)

In his eulogy to Lorentz, Max Born referred to him as ". . . Der Füher und Repräsentanten in einem Abschnitt der Physik, den wir heute als die klassische Periode unserer Wissenschaft der neuen, im Jahre 1900 anhebenden revolutionären Enwicklung gegenüberstellen" (M. Born, Göttingen Nachrichten, Geschafliche Mitteilungen, 1928/29, pp. 69-73, p. 69. ". . . the leader and representative of a period of physics that we constrast today as the classical period of our discipline to the newly raised, revolutionary developments of 1900.")

Einstein himself wrote: "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 fullyrealize, as a rule, the determinant part which H.A. Lorentz played in the formation 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

Almost from the start of his professional career, Lorentz began on a program of bringing unity to the structure of physics. In this effort, he attempted to unite the ideas of Fresnel[6] on the interaction of ether and matter with Maxwell's description of electromagnetic phenomena and the "atomic" view of electricity of Weber and Claussius.[7] According to Born,[8] Lorentz' doctoral thesis decisively decided the dispute over the nature of the ether by showing that it was impossible to suppress the longitudinal wave that was created in any model

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these ideas and the simplification which they brought into the foundations of the science of physics."(Albert Einstein in de Haas-Lorentz, loc cit., p. 5.)

Lorentz shared the Nobel Prize for 1902 with his student Zeeman. He was a member of many scientific societies including the Royal Society which bestowed many honors on him during his lifetime. These included the Rumford medal (1908), and the Copley medal (1918).

Lorentz' work was not confined to the physics to be described in the text. 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, refraction, and other optical and physical phenomena.(Cf., Lorentz, Theory of Electrons(Leiden, 1909, revised edition, 1915; repr Dover, 1952.) He worked on the fundamental aspects of the kinetic theory of gasses. Among his other accomplishments were the overseeing of the construction of the Zuiderzee dam. (J. Th. Thizsse, "Enclosure of the Zuiderzee", in de Haas-Lorentz, loc. cit., pp. 129-44.) The extent to which Lorentz was viewed as a national hero in the Netherlands may be guaged from the fact that the Dutch national telegraph service suspended operation for three minutes at noon in honor of Lorentz on the day of his funeral.(de Haas-Lorentz, in Ibid., p. 150 f.)

6. It should be pointed out that of all of his predecessors, Lorentz revered most the work of Fresnel. Cf. de Haas-Lorentz, loc cit., p. 32.

7. H.A. Lorentz, La théorie électromagnétique de Maxwell et son application aux corps mouvants. (Leiden, 1892), p. 71.

8. Born, loc. cit., p. 69. Lorentz' thesis was printed in Zs für Math. u Phys 22: 1,205, 1875.

of the ether that was an elastic solid. Henceforth Lorentz was to argue for an electromagnetic ether whose properties were defined solely by the equations used to define it.[9]

Three years later, in 1878, Lorentz published a work on dispersion in which he made a radical departure from his predecessors.[10] Lorentz assumed that the ether was the same in and out of matter, ether was not affected by matter; the ether was absolutely fixed. These two assumptions, that the ether remains unaffected by matter and that the ether does not partake in the motion of matter[11] were two of the foundations of Lorentz' theory of electrons which was developed in a series of publications beginning in 1892. Our interest in the theory will be restricted to those aspects which pertain to the electrodynamics of moving bodies.[12]

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9. Ibid. For a review of various elastic models of the ether, see E.T. Whittaker, A History of the Theories of Aether and Electricity(Thomas Nelson and Sons Ltd, 1910; revised and enlarged edition, New York: Harper Torch books, 1951.) References are to the 1951 edition. Cf. H.A. Lorentz, Lectures on Theoretical Physics, tr. L. Silberstein and P.H.Trivelli, (3 Vol.; London: Macmillan and Co., 1927) Vol. I, "Aether Theories and Aether Models," pp. 3-74.

10.Cf. Born, loc cit., p. 70.

11.It is worth noting that the assumption of an absolutely fixed ether was first made by Fresnel.

12.In particular we will examine the following publications:
Lorentz, La Théorie électromagnétique de Maxwell . . . .
Lorentz, Versuch Einer Theorie der Electrischen und Optischen Erscheinungen in Bewegton Körern (Leiden, 1895; second unaltered edition, Leipzig, 1906)
Lorentz, "Electromagnetic Phenomena in a System Moving with any Velocity less than that of Light", Proc. Acad. Sci. Amsterdam 6: 809, 1904. The article has been reprinted in A. Einstein et al, The Principle of Relativity (New York: Dover Publications Inc., n.d.) pp. 9-34. All references to this paper will be to the Dover edition. For other aspects of the Lorentz Theory of Electrons, see, Lorentz, Theory of Electrons; Whittaker, loc. cit. Chapter 13, "Classical Theory in the Age of Lorentz".

Lorentz began in 1892 investigation by making a sharp distinction between ether and matter, though unlike his British counterparts, he did not specify the nature of the ether.[13]

> Je nommerai matière tout ce qui peut être le siège des courants ou deplacements de l'éctricité et des mouvement électromagnétiques. Ce nom sera donc appliqué à l'éther tout aussi bien qu'a a matière ponderable. 14

In keeping with his attitude toward the ether and his desire to combine particular notions of electricity with Maxwell's description of electromagnetic phenomena, Lorentz proposed that all such phenomena could be understood in terms of the interactions of fundamental electrical particles (electrons) which made up material, and the ether.[15] Among other assumptions, Lorentz held that the ether permeates these particles, and that the particles were perfectly rigid spheres which could only translate or rotate.[16]

Lorentz now applied these fundamental considerations together with Maxwell's equations to various electrical phenomena including the propagation of light in moving media. His ultimate goal was the derivation of the Fresnel dragging coefficient for moving media.[17] That is if the medium had a velocity v relative to some observer, then the velocity of light in the medium would be altered in the direction of motion by the factor

$$v(1-1/n^2)$$

13. For some British speculations on the nature of the ether see part IV below, passim. Cf. Lorentz, Lectures on Theoretical Physics, Vol. I, pp. 1-74.

14. Lorentz, La Theorie electromagnetique de Maxwell . . . . p. 47. "I will call matter all that which can be the seat of electrical currents or electrical displacements of electromagnetic movement. The name ether will be applied to all else besides ponderable matter."

15. Ibid. p. 71

16. Ibid. pp. 70-73

17. Ibid. pp. 6,163 ff.

where v is the velocity of the medium and n is the index of refraction for the medium. Lorentz was in fact able to derive the dragging coefficient on the basis of the model we have outlined, as long as one neglected second order and higher order terms in v/c.

Three years later, Lorentz again attacked the problem of the electrodynamics of moving bodies.[18] In this work, Lorentz sought to simplify the theory of three years earlier while at the same time bringing still more phenomena into account. Whereas in 1892, Lorentz had insisted that the aether be considered as absolutely fixed, his requirements concerning the motion of ether were now a little less stringent:

> Dass von absoluter Ruhe des Äthers nicht die Rede sein kann, versteht sich wohl von sielbst; der Ausdruck würde sogar nicht einmal Sinn haben. Wenn ich der Kürze wegen sage, der Äther ruhe, so ist damit nur gemeint, dass sich der eine Theil dieses Mediums nicht gegen den anderen verschiebe und dass alle wahrnehmbaren Bewegungen der Himmelskörper relative Bewegungen in Bezug auf den Äther seien. 19

As in the past, Lorentz refused to speculate in any way about the nature of the ether. Clearly, it had become the benchmark of absolute space.

The simplification of the theory resulted from the introduction of a new set of transformation equations. Up to 1895, Lorentz had simply employed the Galilean transformation equations when describing phenomena in frames of reference moving with respect to the ether. That is, since Maxwell's equations were assumed to hold for the rest frame, the frame of the ether, in another frame of reference Maxwell's equations would have to be modified. The fact that all

18. Lorentz, Versuch Einer Theorie . . . .

19. Ibid., p. 4. "It is self-evident that there cannot be any sense in the phrase absolute rest of the aether. When I say in a short hand way, the ether rests, I only mean that one part of this medium is not displaced relative to another and that all preceptible motions of heavenly bodies are in relation to the ether."

attempts to measure the velocity of the earth with respect to the ether to the first order in v/c had failed, was interpreted to mean that the description of phenomena in frames of reference other than the ether frame must be the same, at least to the first order in v/c.[20]

The new transformation involved how time would be measured in frames of reference moving with respect to the ether frame. According to Galilean relativity the measurement of intervals of time was an invariant. That is if in one frame of reference, Σ, the time interval Δt were measured, in another frame of reference Σ' the interval $\Delta t'$ would be equal to Δt for the interval between the same two events. The new transformation proposed by Lorentz

$$t' = t - \frac{vx}{c^2}$$

where vi is the velocity of the frame of reference with respect to the ether and x is the distance coordinate in the moving frame at which the measurement of time is made, no longer left the measurement of time intervals as an invariant.[21]

Though this was a very bold and radical step to take, Lorentz had surprisingly little to say about the sharp departure such a transformation represented. He designated the time t' as "local time" (Ortzeit) as opposed to the general, or true time (allgemeiner Zeit), t.[22] Otherwise he made little comment, at that

20. A succinct way of stating this fact would be to invoke the principle of relativity as a first order approximation. The fact that we have not done this in the text reflects the fact that Lorentz himself did not, for whatever reason, invoke the phrase, "principle of relativity" for his own work until after Einstein's publication. More of this below.

21. Lorentz, Versuch einer Theorie . . . ., p. 49.

22. Ibid.

time, on the meaning of the transformation. He did remark that he intended it to be little more than an aid to calculation.

Lorentz gave no indication of how he had arrived at this transformation equation. However, when this new set of transformations was applied to Maxwell's equations, they retained their form, at least to a first order approximation. This is just the result that had been found to exist experimentally.[23] Furthermore, not only was Lorentz now able to derive the Fresnel dragging coefficient with greater ease than he had been able to do in 1892; but he was also able to subsume all other first order phenomena into his theory of electrons.[24]

In view of the fact that this 1895 work of Lorentz has been cited by some authors as the precursor to Lorentz' theory of relativity[25] or at the very least as the primer which Einstein used in forming his own theory of relativity,[26] one would expect to find some statement of the principle of relativity. In fact, there is no statement of the principle and the reason is fairly simple. In Lorentz' theory, the principle of relativity is not strictly true; it is only a first order approximation. One should expect to find effects of the motion of the earth with respect to the ether in experiments sensitive enough to reveal second order effects in v/c.

On the other hand, Lorentz was quite aware that several second order experiments had been done to measure the drift of the earth through the ether and

23. Ibid., pp. 82 ff.

24. Ibid., pp. 83-114.

25. Whittaker, op. cit., Vol. II., pp. 27 ff.

26. Cf. G.H. Keswani, "Origin and Concept of Relativity," The British Journal for the Philosophy of Science 15: 286-306, 1965.

that the results of these experiments had been null. In fact, the last chapter of the 1895 treatise was entitled "Versuche deren Ergebnisse sich Nicht Ohne Weiteres Erklären Lassen."[27]

The experiment cited by Lorentz which by far has received the most attention is the Michelson Morley experiment.[28] Lorentz noted that the null result could not be explained away by simply assuming that the earth dragged the ether completely since that conclusion would contradict the partial drag hypothesis of Fresnel and himself which explained most of the other experimental results.

Rather, as is well known, Lorentz repeated a suggestion that he had made three years earlier, that the arm of the interferometer in the direction of motion contracted just enough to compensate for the difference in time that one would expect between light moving with and against the motion of the earth and light moving ransverse to the direction of the motion of the earth. Lorentz acknowledged that the very same hypothesis had been suggested by Fitzgerald and was somewhat unsettled by the apparent ad-hoc nature of the hypothesis. He did

27. Lorentz, Versuch einer Theorie . . . ., pp. 115-138, "Investigations whose results cannot be explained without further Ado." This translation is by Gerald Holton Cf. G. Holton, "Influences on and Reception of Einstein's Early Work in Relativity Theory" (Mimeo, 1965) pp. 32 ff.

28. A.A. Michelson, "The Relative Motion of the Earth and the Luminferous Ether," American Journal of Science 28: 120-29, 1881
A.A. Michelson and E.W. Morley "On the Relative Motion of the Earth and the Luminferous Ether", American Journal of Science 34: 333-45, 1887.

try to provide what he considered to be a natural justification for making such a hypothesis:

> So befremden die Hypothese auch auf den ersten Blick erscheinen mag, man wird dennoch zugeben müssen, dass auch die Molecularkräfte, ähnlich wie wir es gege wärtig von den electrischen und magnetischen Kräften bestimmt behaupten köanen, durch den Aether vermittelt werden. Ist de so, so wird die Translation die Wirkung Zwischen zwei Molecülen oder Atomen höchstwahrscheinlich in ähnlicher Weise ändern, wie die Anziehung oder Abstossung zwischen geladenen Theilchen. Da nun die Gestalt und die Dimensionen einses festen Körpers in letzerer Instanz durch die Intennität der Molecularwirkungen bedingt werden so kann dann auch eine Aenderung der Dimensionen nicht ausbleiben. [29]

This 1895 paper was Lorentz' last major effort to treat the electrodynamics of moving bodies until 1904.[30] In the years immediately following the publication of the 1895 paper, several important events occured which greatly influenced his later, 1904 work. First Henri Poincaré, who had devoted much attention to the theory of the electrodynamics of moving bodies, had given Lorentz' theory a great deal of attention.[31] By 1900 Poincaré had begun to talk about the "Principle of Relativity" which to Poincaré was an empirical principle. He urged that Lorentz incorporate such a principle in his theory and that Lorentz recast the theory in such a way as to remove the ad-hod aspects.[32] Second,

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29. Lorentz, _Versuch einer Theorie . . . ._, pp. 123-24. "As strange as this hypothesis may seem at first glance, it must nevertheless be admitted that it is not too far out, as long as one assumes that even molecular forces are mediated by the other, just as we today assert we can determine electrical and magnetic forces. If this is so, then the translation will very likely change the interaction between two molecules or atomes just as the attraction or repulsion between charged particles. Then since the form and the dimensions of a rigid body will depend in the final analysis on the molecular interaction, such a change of dimensions can not fail to appear.".

30. Lorentz did publish a brief paper in 1900 suggesting, without proof that perhaps the mass and shape of the electron would be altered as its velocity approached the velocity of light. (_Phys. Zs. 2_: 78, 1900)

31. See part IV, below.

32. _Ibid._

Kaufmann had begun to publish data and results of his experiments on the mass of the electron. For the first time it was felt, there was reliable information on which to base judgments of the mass of swiftly moving electrons.[33] Third, the theory of Max Abraham, based on a perfectly rigid electron was published beginning in 1902.[34] The Abraham theory was rival to the Lorentz theory.

In 1904, Lorentz published his final version of a second order theory.[35] His paper was, he said, not only necessary in the light of recent second order ether drift experiments, but in addition,

> Poincaré has objected to the existing theory of electric and optical phenomena in moving bodies that, in order to explain Michelsion's negative result, the introduction of a new hypothesis has been required, and that the same necessity may occur each time new facts will be brought to light. Surely this course of inventing special hypotheses for each new experimental result is somewhat artificial. It would be more satisfactory if it were possible to show by means of certain fundamental assumptions and without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system. . . . I believe it is now possible to treat the subject with a better result. 36

Lorentz began this paper with several assumptions. First of all, he assumed the validity of Maxwell's equations in a frame of reference at rest with respect to an observer. Next he assumed a new set of transformation equations--the Lorentz transformations which would leave the form of Maxwell's equations unaltered

33. See Part III below.

34. See below.

35. Lorentz, "Electromagnetic Phenomena . . . ."

36. Ibid., p. 13.

in all inertial frames of reference.[37] In fact some eleven such assumptions have been identified in this 1904 paper:

> . . . Lorentz' great paper which appeared. . . /in 1904/ and typified the best work in physics of its time--a paper which declared to be based on "fundamental assumptions" rather than on "special hypotheses"--contained in fact eleven ad hoc hypotheses: restriction to small ratios of velocities v to light velocity c; postulation a priori of the transformation equations (rather than their derivation from other postulates); assumption of a stationary ether; assumption that the stationary electron is round; that its charge is uniformly distributed; that all mass is electromagnetic; that the moving electron changes one of its dimensions precisely in the ratio of $(1-v^2/c^2)^{1/2}$ to 1; that forces between uncharged particles and between a charged and uncharged particle have the same transformation properties as electrostatic forces in the electrostatic system; that all charges in atoms are in a certain number of separate "electrons"; that each of these is acted on only by others of the same atom; that the atoms in motion as a whole deform as electrons themselves do. 38

Armed with these hypotheses, Lorentz was able to predict values for the mass of the electrons which were dependent on the electron's velocity. These predictions agreed as well with the data as those of Abraham. The transformation equations cited above insured that no experimental result should be obtained which would reveal the absolute motion of the earth.

In the following year, Einstein published his theory. This 1904 paper then, may justly be considered Lorentz' final effort at a second order theory. Several conclusions must be drawn concerning this theory in order to fully appreciate Lorentz' own attitudes toward Einstein's theory. First of all as Holton has

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37. Ibid., pp. 13-14. The Lorentz transformations are given by

$$x' = \frac{x - vt}{\sqrt{1-v^2/c^2}},\quad y' = y,\quad z' = z,\quad t' = \frac{t - \frac{vx}{c^2}}{\sqrt{1-v^2/c^2}}$$

38. Gerald Holton, "On the Origins of the Special Theory of Relativity," Amer. J. Phys. 28: 627-36, 1960. p. 630.

pointed out,[39] there is no statement of a principle of relativity, as such in Lorentz' paper. In fact, one concludes from reviewing Lorentz's work that he himself did not put much emphasis on such a principle. It was only necessary in his theory that the apparent inability to detect the motion of the earth through the ether should be accounted for. This is accomplished in Lorentz's terms by a remarkable compensation of effects, in particular, the contraction of bodies in the direction of motion.[40] Similarly the invariance of the velocity of light is a <u>result</u> of the <u>assumed</u> transformation equations. In Einstein's theory, as we will emphasize over and over again the constancy of the velocity of light is a postulate which, with the principle of relativity, leads to the transformation equations.

Though both theories employ the same transformation equations and hence predict the same results, these predictions generally have very different meanings in the two theories. To Lorentz, the contraction of lengths was primary; it was a real effect explainable in terms of the interactions of molecules. To Einstein the length contraction was an artifact of measurement, a result of the fact that observers in different frames of reference would disagree on how the measurement was made. Similarly, disagreements in time were only aids to calculation for Lorentz whereas for Einstein, the time measured in each frame of reference had physical significance.

There were some internal inconsistencies in the Lorentz theory. The Lorentz electron had become deformable--it had to contract in the direction of motion.

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39. Holton, loc. cit.

40. Lorentz, "Electromagnetic Phenomena . . . ." pp. 28-29.

Yet as Abraham was quick to point out,[41] such a deformable electron required non-electromagnetic forces to maintain its stability and form. However, one of the correlaries of the Lorentz theory was that all matter was electromagnetic in origin.

Lorentz did not like philosophy[42] and did not allow philosophical considerations to intrude in his struggle with physical problems. It is understandable then that his treatment of the problem of the electrodynamics of moving bodies should not start from fundamental considerations about the nature of space and time. These were, to him, self-evident in their classical form.[43] Given the primacy of physics to Lorentz, it is not surprising to find that Lorentz began with phenomena and then reasoned by to the kinds of transformations necessary to account for the appearances in different frames of reference.

41. Max Abraham, "Die Grudnhypothesen der Elektronentheorie" Phys Za. 5: 576-79, 1904.

42. G.L. de Haas-Lorentz, loc. cit., p. 26 and passim.

43. For one of Lorentz' last statements on the subject see, H.A. Lorentz, Problems of Modern Physics (Boston: Ginn and Co., 1927), pp. 20-22.

B. Lorentz' Attitudes Toward Einstein's Theory of Relativity

As Whittaker has reported,[44] Lorentz' convictions about the meaningfulness of absolute time and of the independence of space and time variables remained unchanged to the end of his life. It was not a matter of Lorentz closing his mind to a new idea. Lorentz struggled with the new point of view for the last twenty years of his life until he was able to explicate and incorporate the formalism completely while rejecting the new interpretation.[45]

Lorentz' first public notice of Einstien's contribution appeared in the last paragraph of a series of lectures given at Columbia University in 1906.

> His /Einstein's/ results concerning electromagnetic and optical phenomena . . . agree in the main with those which we have obtained in the preceeding pages. The chief difference being that Einstein simply postulates what we have deduced, with some difficulty and not altogether satisfactorily, from the fundamental equations of the electrodynamic field. By doing so, he may certainly take credit for making us see in the negative results of experiments like those of Michelson. . . . Not a fortuitous compensation of opposing effects, but the manifestation of a general and fundamental principle.
>
> Yet, I think, something may also be claimed in favor of the form in which I have presented the theory. I cannot but regard the ether which can be the seat of an electromagnetic field with energy and its vibrations, as endowed with a certain substantiability, however different it may be from ordinary matter. 46

It wasn't until ten years later, by Lorentz' own account that he fully realized the differences between his own work and the work of Einstein:

> If I had to write the last chapter now, I should certainly have given a more prominent place to Einstein's theory of relativity

44. Whittaker, op. cit., Vol. II, p. 36.

45. Born, loc. cit., pp. 71-72.

46. Lorentz, Theory of Electrons, pp. 229-30.

> . . . by which the theory of electromagnetic phenomena in moving systems gains a simplicity that I had not been able to attain. The chief cause of my failure was my clinging to the idea that the variable t, only can be considered as the true time and that my local t' must be regarded as no more than an auxiliary mathematical quantity. 47

From his writings and speeches after Einstein's publication one gets a clear picture that Lorentz wanted not only to divest the ether of any substantiation, he wished as well to embrace the principle of relativity. But Lorentz was unwilling to reject the concept of the ether as the medium of propagation of electromagnetic radiation. He pleaded that the ether, though divested of most material properties be left enough substantially so that it made sense to talk about an absolute frame of reference.[48] The principle of relativity remained, for Lorentz, an empirical principle. Lorentz' view then was that while the absolute frame of reference existed, nature had conspired to prevent us from determining our motion with respect to it. As Lorentz himself recognized, it was very difficult for a person, trained in a certain mode of thought, and devoting a good part of his life to the persual of questions framed by that mode of thought to change his views very drastically. Lorentz' greatness can be measured by the degree to which he was able to incorporate the new theory of relativity into his own work even if he was not able to fully accept it. The very very end of his life, Lorentz pursued the dream that all of physics would be comprehensible under one theory governed by one set of assumptions.

47. Ibid., p. 321 fn 72* Starred fn were addend to the 1915 edition.

48. Lorentz, Lectures on Theoretical Physics . . . pp. 208-11.

As attractive as such a view might be, even Lorentz, was unable to succeed in that endeavor.[49]

49. The following contain Lorentz' view in the later years of his life:
Lorentz, The Einstein Theory of Relativity(New York: Berntano's, 1920)
Lorentz, "Alte und Neue Fragen der Physik," Phys Zs. 11: 1234-57, 1910
Lorentz, "Dis Maxwellsch Theorie und die Elektronentheorie" in E. Warburg(ed), Physik(Leipzig: B.G. Teubner, 1915)
Lorentz, Problems of Modern Physics: A Course of Lectures Delivered in the California Institute of Technology/1922/ Harry Bateman (ed), (Boston: Ginn and Co., 1927).

C. The Abraham Theory of the Electron

Abraham's theory of the electron, a rival to the Lorentz theory, though little known today exerted a great deal of influence on the physics of his time. In fact, between the years 1902 and 1904, the experimentation on the mass of the moving electron was considered a vindication of Abraham's theory. The Abraham theory was developed shortly after Kaufmann had published in 1901 his first tentative results on the variation in mass of the electron as a function of the electron's velocity.[50] Besides the experimental issue, Abraham was intrigued by the hope of using mechanics on electromagnetic theory. Then too Abraham's earlier training in electrodynamics and his work with Maxwell's equations had led him almost naturally to the point of attempting to build a consistent and universal physics based on electrodynamics.[51]

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50. W. Kaufmann, "Die magnetische und elektrische Ablenkbarkeit der Becquerelstrahlen und die Scheinbare Mass der Elektron" Nachr Ges. Gott., 1901, pp. 143-55. Walter Kaufmann (1871-1947) received his doctorate at München in 1894. His work on the specific charge of the electron was done while on the Faculty at Göttingen in the years following his doctorate. In 1908 he became director of the Technische Hochschule, Köningsberg.

51. Max Abraham (1875-1922) worked in several fields surrounding his greatest expertise: Maxwell's theory. Besides his theory of electrons he worked on problems related to light pressure and theoretical questions in radio transmision. Abraham is best remembered for his two volume text book, Theorie der Electrizität which went through three editions in his lifetime. This book trained a generation of physicists in Germany and the use that Abraham made of vectors insured that formalism quickly spread in use throughout Germany. Cf. M. Born and M. von Laue, "Max Abraham," Phys Zs. 24: 49-53, 1923.

The basic underlying assumptions of Abraham's theory were first that the conception of the ether was valid; second that the differential equations of the electromagnetic field (Maxwell's equations)

> maasgebend für die Dyanmaik des Elektrons, und somit auch für die Mechanik der aus Elektronen, zusamengestzen Materie. 52

Abraham's approach was to determine theoretically, the inertia due to the self induction of the electron as it moved through its own field and the induction due to any external field that the electron found itself in. Given numerical values for the parameters, one could then compare the results thus obtained with Kaufmann's results. If agreement was substantial, then one could say with some assurance that the mass of the electron was purely electromagnetic--due solely to the induction of its own charge. In order to handle such an analytically complex situation, Abraham found it necessary to make an analogy to the handling of alternating current problems by applying a quasi-stationary analysis to determine the force on the electron. By "quasi-stationary" motion, Abraham meant that the velocity of the electron changed very little in the time required for light to transverse the diameter of the electron (about 10^{-23}sec.). In order to insure the stability of the electron, Abrham had to assume that the electron was perfectly rigid and did not alter its form when in motion. Abraham's prediction for the transverse mass of the electron was[53]

$$m = m_0 \frac{1}{\beta^2}\left(\frac{1+\beta^2}{2\beta} \ln\left[\frac{1+\beta}{1-\beta}\right] - 1\right) =$$

$$m_0\left(1 + 2/5\,\beta^2 + \frac{3}{2\cdot5\cdot7}\beta^4 + \cdots +\right) \quad \text{where } \beta = v/c$$

52. Abraham, loc. cit., p. 20. "apply to the dynamics of electrons and to the mechanics of matter composed of electrons."

53. Ibid., pp. 32-38.

Noting that there was a high degree of uncertainty in Kaufmann's results, Abraham was nonetheless pleased to find agreement between his predictions and Kaufmann's data. He felt justified in concluding that

> Die Trägheit des Electrons is ausschliesslich durch sein electromagnetischer Feld verursacht. 54

Abraham's program, like Lorentz' was an attempt to bridge the gap between the well grounded classical point of view in which electromagnetic radiation was transmitted by some continuous medium and the recent resurgence of an atomistic approach to electricity itself. Unlike Lorentz, Abraham had not attempted to build an overarching theoretical structure which would include all phenomena. In fact, throughout his career, Abraham deferred to Lorentz' explanations of all experiments which had been designed to detect motion of the ether.[55] It was only in the case of the structure of the electron that Abraham stubbornly, for reasons we will investigate, maintained his own theory. An obvious reason for Abraham's optimism with regard to his own theory prior to Lorentz' 1904 publication was the fact that Abrham had the only theory in print which made predictions concerning the change in mass of the electron and which also concluded that the mass of the electron was entirely electromagnetic in origin. In the year following the publication we have just described, 1903, Abraham published what must be considered his definitive paper on the dynamics of electrons.[56] Whereas in 1902 Abraham had been

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54. Ibid., p. 40. "The inertia of electrons is caused exclusively by its electromagnetic field."

55. Cf. Abraham, Theorie der Electrizität, 1st ed., Vol. II, pp. 370 ff.

56. M. Abraham, "Prinzipien der Dynamik des Elektrons", Ann d Phys 10: 105-79, 1903

reasonably satisfied with the agreement between his theory and Kaufmann's data, he now revealed that his original results had not agreed too well withKaufmann's figures, but that Kaufmann had since found an error in his calculations and a better method for measuring the electric field strength in his apparatus.[57] Theory and experiment now agreed to such an extent that Abraham could once again say that "Die Masse ds Elektrons ist rein elektromagnetischer Natur."[58]

In 1904 Lorentz published his second order theory and made rival predictions for the mass of the electron. According to Lorentz his concept of the deformable electron led to the following expression for the mass of the electron:[59]

$$m = m_0 (1-\beta^2)^{-1/2} = m_0 (1 + \tfrac{1}{2}\beta^2 + \tfrac{3}{8}\beta^4 + \cdots +)$$

Abraham's response was immediate.[60] He questioned most the stability of the kind of electron Lorentz had proposed. In order to insure stability, a force would be required other than the internal electrical forces of the rigid electron because when the deformable electron undergoes acceleration, the increase in energy is greater than that due to the change in velocity.[61]

57. Ibid., p. 107.

58. Ibid., "The mass of the electron is purely electromagnetic in nature."

59. Lorentz, "Electromagnetic Phenomena . . . ." p. 31.

60. Abraham,"Die Grundhypothesen . . . ."

61. Ibid., p. 578.

Although Abraham was willing to concede that the expression for the mass of the electron in the Lorentz theory was simpler than his own expression, the fact that Lorentz had to introduce additional, non-electric forces made that theory far more complicated in Abraham's eyes. If as Lorentz claimed, he wished to base his views on the belief that electrodynamics was fundamental, then he could not make the hypothesis of a deformable electron. Such an electron required non-electromagnetic forces to maintain its stability.[62]

It must be emphasized however, that Abraham's theory was quite limited in scope. For most phenomena, Abraham had no other choice but to rely on Lorentz' theory. He was in the paradoxical position of using the fundamentals of the Lorentz theory to build a theory of the nature of the electron which was contrary to Lorentz' own theory. Given his own theory, it is difficult to see how he would have been willing to use and accept Lorentz' other conclusions about the contraction of real objects and local time without applying them to his own electrons. Abraham did not give much consideration to the Lorentz transformation equations. Had he done so, he would have had to reject one of his fundamental assumptions--that it was possible to identify a special frame of reference in which the velocity of light was a constant. The evidence suggests that Abraham wished to limit considerations of the contraction of objects to gross matter. Had he recognized the transformation equations as generally applicable, he would have been forced to apply them not only to gross matter, but to electrons as well and would have eradicated the possibility of a substantial,

62. Cf. Ibid. passim; Abraham, Theorie der Elektrizität, 1st Ed. pp. 170-73.

fixed ether. In support of this point of view, we can cite Abraham's feeling that the contradiction should be real and measureable in a coordinate system at absolute rest,[63] and his underlying faith in the reality of the ether.

> . . . er liebte seinen absoluten Äther, seine Feldgleichungen, sein starren Elektron wie ein Jüngling seine erste Flamme, deren Andenken kein späteres Erlebnis auslöschen kann. 64

In 1905, Einstein's theory of relativity was published. It's predictions on all emprical matters was the same as the Lorentz theory. It was often confused with the Lorentz theory. At the time of its introduction there were then two major sets of predictions concerning the relative motion of electromagnetic bodies.[65] Those of Lorentz and those of Abraham. Since they made very similar predictions on the only data then available, the mass of the moving electron, experiment had not yet been able to distinguish between them. It was generally believed that the matter would be decided by more sensitive and careful experiments of the type that Kaufmann had already done.

It will be most convenient to consider these experiments as part of the response to Einstein's theory in Germany. We turn then directly to that question.

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63. <u>Ibid.</u> p. 376.

64. Born and Laue, <u>loc. cit.</u>, p. 51. " . . . he loved his absolute ether, his field equations his rigid electron like a youth loves his first flame, whose memory no later experience can extinguish."

65. A set of predictions by Bucherer have not been considered here. His role will be taken up in part three but since his theory was not really given much consideration we have chosen to ignore it in this introductory section.

III. The German Response to Einstein's Theory of Relativity

Of the four countries considered in this study, Germany is in many respects unique in its response to Einstein's theory of relativity. In sheer quantity, no other country comes close to matching the output of German physicists on the subject of relativity during the period 1905-1911.[66] It might be thought that the fact that Einstein himself wrote in German would in and of itself explain this phenomenon. Unfortunately, such a simple, straightforward explanation will not suffice. It is not simply the quantity of German response to Relativity which is so startling. What really distinguished between the German response and the response from other countries is the variety of points of view that one finds in Germany. As we will demonstrate in this and succeeding sections, it is only in Germany that one finds a spectrum of response to Einstein's work. Besides all of the objections to the theory of relativity which can be identified with the French, English, and American response, one finds in Germany enthusiastic support. Only in Germany were there people of whom one can say, "he understood Einstein's program." Not all of those, who by their response to Einstein's work demonstrated understanding of the

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66 Of the literature on relativity handled in this study, some 85% is of German origin. Lecat estimated that of the literature published since the seventeenth century which might bear relevance to the study of the nature of time and space and to the specific study of the laws of physics in coordinate systems moving with respect to each other, 30% is of German origin. The next ranking country in terms of the production of original papers was England which produced 15% of the literature in the same period. Cf. Lecat, loc. cit., pp. 200 ff.

theory, were willing to accept it. Nevertheless, in most cases in Germany, Einstein's work was, almost from the start, taken seriously and given careful consideration.

It would be quite beyond the scope of this section to cover in detail all of the literature produced in German on the subject of Einstein's theory of relativity during the years 1905-11. Rather we will examine several classes of responses associated with certain problems and individuals. The choice was of course to some extent arbitrary; however, we believe that regardless of how the choice might be made, the issues associated with those choices would be the same. The items chosen include, the mass of the moving electron; the meaning of a rigid body; general dynamics and the thermodynamics of moving cavities.

A. Measurements of the Specific Charge of High Velocity Electrons 1905-11

From the time that Lorentz published his 1904 paper, the question of the agreement of experiment with theory loomed larger and larger. Both Abraham and Lorentz claimed that their theories agreed with experiment. To many physicists, the question of the proper theory rested almost solely on the degree to which it compared favorably with the measurements of the mass of swiftly moving electrons.

For example, Bucherer in 1905[67] noted that up to now Lorentz' theory resulted in predictions in the change of the transverse mass of swiftly moving electrons which did not deviate any more from experiment

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[67] A.H. Bucherer, " Das deformierte Elektron und die Theorie des Elektromagnetisumus", Phys Zs. 6: 833-34, 1905. p. 833.

than Abraham's rigid electron theory. However, Bucherer continued, a recent oral communication from Kaufmann had indicated that very recent measurements were not in agreement with the rigid electron theory. Bucherer felt that this indicated the failure of the Lorentz program:

> Somit würde es scheinen, als ob der Versuch Lorentz durch seine Hypothese der Dimensionsänderung eines bewegten Systems des wankende Gebäude der Maxwellschen Äthertheorie zu stützen, als misslungen zu betrachten wäre. Ein gleiches würde won den Versuchen anderer zu sagen sein, welche auf anderem Wege zur selben Formel für die transversals Masse gelang sind.[68]

Whether or not Bucherer was referring to Einstein's paper, which had been published two months earlier, as "other research" is not known. There is no doubt however that Kaufmann was aware of Einstein's theory when he published his detailed results in 1906.[69] Kaufmann's explication of Einstein's theory is worth examining in detail. Kaufmann recognized that Einstein's theory began with two postulates: The principle of relativity and the postulate of the constancy of the velocity of light and that these two postulates led to a new conception of simultaneity for spatially separated points. He recognized that Einstein's theory arrived at results in an inexorable fashion from the two postulates and contrasted that to the arbitrariness of the Lorentz formulation of the same equations.[70]

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68 Ibid. "Therefore it appears as if the research of Lorentz with its hypothesis of dimensional changes of moving systems to support the shaky structure of the Maxwellian ether theory will have to be thought of as failing. The same will have to be said for other researches which arrive at the same formula for transverse mass in other ways.

69 W. Kaufmann, "Die Konstitution des Elektronen", Ann d Phys 19: 487-53, 1906.

70 Ibid., pp. 491-92.

It is clear, therefore, that Kaufmann understood the distinction between Einstein's program and that of Lorentz. His motivation for undertaking another set of experiments to determine the specific charge of electrons moving with different velocities was to distinguish, if possible, between various theories. The experimental arrangement was somewhat different than the modern experiments in which a velocity filter of crossed fields is used to select electrons of a certain velocity. Electrons from a Radium source are accelerated in an electric field and a magnetic field, the fields being arranged so that they are parallel to each other. The electron, therefore, is accelerated in one direction by the electric field and in a perpendicular direction by the magnetic field. If the field parameters and constants of the apparatus are known, then by measuring the deviations of the electron image on a photographic plate, one can determine the ratio of the charge to the mass of the electron, e/m. Assuming that the charge remains constant, any variation in that ratio can be ascribed to changes in the mass.

As Bucherer had reported, Kaufmann felt that his new data now decidedly favored the theory of Abraham:

> <u>Die Messüngergibnesse sind mit der Lorentz-Einsteinschen Grudnanahme nicht vereinbar. Die Abrahamsche und die Buchersche Gleichung stellen die Beobactungsresultate gleich gut dar. Eine Entscheidung zwischen beiden durch Messung der transversalen</u>

Masse der strahlen erscheint einstweilen als unmöglich.[71]

If his data did not support the Lorentz or Einstein theory, then those theories and with them the principle of relativity must be rejected:

> ...Betrachtet man dieses [Lorentz's and Einstein's predictions] aber als widerlegt, so wäre damit auch der Versuch, die ganz Physik einschliesslich der Elektrodynamik und der Optik auf der Relativbewegung zugründen einstweilen als misglückt zu bezeichnen....
>
> Wir werden viel mehr einsteilen bei der Anahme verbleiben müssen, dass die physikalischen Erscheinungen von der Bewegung relativ zu einem ganz bestimmen Koordinaten System abhängen, das wir den absolut ruhenden Äther bezeichen.[72]

Kaufmann went on to say that if we have not yet succeeded in detecting by optical or electrodynamic experiments an influence of the motion of the earth through the ether, this did not exclude the possibility of such detection in the future.[73]

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71. Ibid., p. 495. "The results of measurements are not in accord with the Lorentz-Einstein basic assumptions. The results of observations represent Abraham and Bucherer equations equally well. Meanwhile the distinction between the latter two theories by measurement of -rays appears to be not possible." (Emphasis in original.)

Bucherer's theory of the electron was identical to an earlier suggestion by Langevin (see section IV), that the electron was deformable and that the deformation took place in such a way that the volume of the electron remained constant. Such an assumption leads to the prediction that the transverse mass of the electron is given by

$$m_o(1-v^2/c^2)^{1/3}$$

The theory was never a serious competitor and as such will not be treated further. For a description of the theory see, A.H. Bucherer, Mathematische Einfuhrung in die Elektonentheorie (Leipzig, 1904), pp. 57-60

72. Ibid., pp. 534-35. "...if these (the Lorentz and Einstein predictions) are considered as refuted, so also would the attempt to base the entire body of physics, including electrodynamics and optics under the principle of relative motion be labeled as failing...

We must remain with the assumption that physical appearances depend on the motion relative to a completely determined coordinate system that we designate as the absolutely resting ether."

73. Ibid., p. 535

As Holton has pointed out[74] this was the first response of any kind in the German literature to Einstein's theory, and the import of the response was that the theory was at odds with the data. And while Holton considers such a negative response or total silence to be characteristic of the reception of Einstein's work during the first few years,[75] the theory was not without a few persuasive and influential supporters. One of those was Max Planck.

At the 78th Naturforscherversamlung held at Stuttgart in the same year that Kaufmann published his results, Planck read a paper which analysed Kaufmann's data using techniques independent of those used by Kaufmann himself.[76] As we will see in detail in a later section, Planck was disposed toward the "Lorentz-Einstein" theory. In this paper Planck corroborated the results obtained by Kaufmann to the extent that he was able to say his results agreed with those of Kaufmann. However, Planck was not willing to say that this decided the issue. Planck reasoned that since the difference in the theoretical predictions of both Abraham and Lorentz-Einstein theories were smaller than the differences between the predictions of either theory, and the observed values, one could not conclude that the Lorentz-Einstein theory must be rejected. Planck also pointed out that since the experimental error was quite high and that since only slight changes in some of the parameters might make large

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74 Holton, "On the Origins..." p. 634.

75 Ibid.

76 Max Planck, "Die Kaufmanischen Messungen der Ablenkbarkeit der Strahlen in Ihren Bedeutung für die Dynamik der Elektronen" Phys Zs7:753-61, 1906.

differences in the results obtained, it would be premature to make any definitive statements.[77]

In the ensuing discussion of this paper, Kaufmann, Abraham, and Bucherer all questioned Planck's reasoning. For Kaufmann, the issue lay solely with the data. Abraham's theory was closer than Lorentz-Einstein, and that was that. Both Bucherer and Abraham, however, were more concerned with the fact that the Lorentz theory (Einstein was not considered at this point by either of them) required forces that were not electromagnetic.[78] While acknowledging the merit of the fact that Abraham's theory was purely electromagnetic, Planck pointed out that

> Wenn dies durchführbar wäre, wäre das wohl sehr schön, vorläufig ist es nur ein Postulat. Der Lorentz-Einsteinschen Theorie liegt auch ein Postulat zugrunde, nämlich, dass keine absolute Translation nachzuweisen ist. Beide Postulate lassen sich, wie es scheint, nicht vereinigen, und nun kommt es darauf an, welchem Postulat man den Vorzug gibt. Mir ist das Lorentzsche eigentlich sympatischer. Am besten wird es wohl so sein, wenn auf beiden Gebieten weiter gearbeitet wird und die Experimente schliesslich die Einscheidung geben.[78]

Clearly, unlike Kaufmann, the issues which concerned Planck and Abraham far transcended the data. In a later portion of this section it

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77 Ibid. , esp. pp. 757-759.

78 Ibid., "If this were feasible, it would be very pretty, meanwhile it is only a postulate. The Lorentz-Einstein theory is also based on a postulate, namely, that no absolute translation can be detected. It appears that both postulates cannot be combined and the question is which of them is superior. The Lorentz postulate is more sympathetic to my point of view. At best, it will be well if further work were done on both rules and the experiments finally made the distinction."

will become clear that Planck's early skepticism was founded on his insight that the theory of relativity provided an absoluteness to physical law which had heretofore been unavailable.

The issue of the veracity of Kaufmann's results did not end here. In the next two years both Planck and J. Stark presented evidence that there had probably been errors in the experiment. In particular, Kaufmann's calculation of the electric field in his apparatus was thought to have been in error since he had not taken into account the fact that the radiation from his radium source would ionize any residual gas in his evacuated apparatus.[79] Given the state of the art with regard to vacuum pumps, Kaufmann could not have been operating at pressures any less than 0.1 mm. Hg. And though Kaufmann objected, it was clear that by 1908, considerable suspicion had been cast on his results. In fact, by 1912, Lorentz could say that

> ...the vacuum (in Kaufmann's apparatus) was not high enough. In fact now and then a spark passed between the plates of the condenser, which shows that there was always some ionisation current left between these plates and that therefore the homogeneity of the electric field was not above doubt. In fine, no definite verdict can be based upon Kaufmann's experiment in favour of either theory.[80]

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C.M. Planck, "Nachtrag zu der Besprechung der Kaufmannschen Ablekungsmessungen." Verh d phys Ges 5: 301-05, 1907.
W. Kaufmann, "Bemerkungen zu Herrn Plancks: Nachtrag zu Besprechung der Kaufmannschen Ablekugsmessungen", Verh d phys Ges 5: 667-73, 1907.
J. Stark, "Bemerkung zu Herrn Kaufmann's Antwort auf einem Einwand von Herrn Planck", Verh d phys Ges 6: 14-16, 1908.
W. Kaufmann, "Erwiederung an Herrn Stark", Verh d phys Ges 6: 91-95, 1908.

80
Lorentz, Lectures on Theoretical Physics..., p. 274.

Significantly, nothing more was heard from Kaufmann on the meaning of his experiment. In fact, in two review articles on cathode rays and "roentgen rays" published in 1915, Kaufmann was mute on the experiment he had performed, or for that matter, on any other experiments on the value of e/m at velocities close to the speed of light.[81]

During this entire controversy, Einstein himself kept a significant silence. His only reference to Kaufmann's work or to the Abraham theory came in a review of the state of the theory of relativity which appeared in 1907.[82] Though the fate of Kaufmann's results were still being debated by others, Einstein remained above the details of the dispute. In fact, Einstein felt that in view of the difficulties inherent in the experiment, the agreement between Kaufmann's data and his own theory was satisfactory. As to the theories themselves Einstein remarked:

> Es ist noch zu erwähnen, dass die Theorien der Elektronenbewegung von Abraham...(and others) Kurven liefern die sich der beobachteten Kurve erheblich besser anschliessen als die aus der Relativitätstheorie ermittelte Kurve. Jenen Theorien kommt aber nach meiner Meinung eine ziemlich geringe Wahrscheinlichkeit zu, weil ihre die Masse des bewegten Elektrons betreffenden Grundanahmen nich nahe gelegt werden

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81 W. Kaufmann, "Die Kathodestrahlen", "Die Röntgenstrahlen", in E. Warburg (ed.), Physik (Leipzig, 1915), pp. 450-57, 467-77.

82 A. Einstein, "Über das Relativitätsprinzip und die aus demselben gezogene Folgerungen", Jahrb. d Radioaktivität u Elektronik 4: 411-62 1907. In this review, Abraham's name is mentioned just once. See footnote 83.

durch theoretische System, welch grossere Komplexe von Erscheinungen umfassen.[83]

Kaufmann's experiments were not the only early determinations of the change in mass of the moving electron. As has already been mentioned, A.H. Bucherer had developed his own theory of the mass of the moving electron. During the years 1907 and 1908 Bucherer had become involved in a polemical argument with an English theoretician Ebenezer Cunningham[84] over the viability of that theory.[85] The dispute centered on whether Bucherer had produced a theory independent of these of Lorentz and Einstein. Cunningham maintained that this was not so, that in any case, it was required to invoke the Lorentz transformations and hence the principle of relativity. Bucherer countered with the remark that he was "not aware that such a 'requirement' is necessary to explain any known fact of observation."[86] In his turn Cunningham showed that Bucherer's

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83
Ibid., p. 439. "It must also be mentioned that the theories of the motion of electrons of Abraham...(and others) yield considerably better curves than the curve produced by the theory of relativity when compared to the experimental curve. However, in my opinion other theories have a rather small probability because their fundamental assumptions concerning the mass of the moving electrons are not explainable in terms of theoretical systems which embrace a greater complex of phenomena." (This translation is in part from Gerald Holton, "Influences on and Reception of Einstein's Early Work in Relativity Theory", (mimeo, 1965).

84
For more of Cunningham's work see section V below.

85
A.H. Bucherer, "New Principle of Relativity in Electrodynamics", Phil Mag 13: 413-20, 1907.
E. Cunningham, "On the Electromagnetic Mass of a Moving Electron", Phil Mag 14: 538-47, 1907.

86
A.H. Bucherer," On the Principle of Relativity and the Electromagnetic Mass of the Electron. A Reply to Mr. Cunningham", Phil Mag 15: 316-18, 1908. p. 316.

theory could be derived using Maxwell's equations and the Lorentz transformations.[87] Bucherer's next reply was but two paragraphs long. The first paragraph stoutly defended the independence of his own theory. The second paragraph is reproduced here:

> Referring to my first paper on the subject in this Magazine, I had from the first recognized that the question, which of the various theories represented the law of nature was one for experiment to decide. I have completed the experiments (on the specific charge of swiftly moving electrons) forshadowed, and in contradiction to Kaufmann have verified the substantial accuracy of the Lorentz formula for the electromagnetic mass, and therefore also of the Lorentz-Einstein principle of relativity, since the only serious objection to its complete acceptance has been removed.[88]

Bucherer's tone in the public literature underwent a dramatic change from the time he reported the results of his experiments. Whereas he had previously treated Lorentz' 1904 paper rather lightly, he now described Lorentz as having led the way with that paper. Whereas he now cited Einstein as having most clearly enunciated the principle of relativity, hardly three months earlier he had claimed that the principle enunciated by Einstein did not lead to predictions in conformity with experiment. No longer did the fact that Kaufmann's data had indicated that the Lorentz and Einstein theories had been refuted disturb Bucherer now. He cast aside the Abraham theory as "inconclusive" and "ad-hoc." The only solution to the problem, Bucherer claimed, was the gathering of more precise data on the specific charge of rapidly moving electrons. It was

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87 E. Cunningham, "On the Principle of Relativity and the Electromagnetic Mass of the Electron. A Reply to Dr. A.H. Bucherer", Phil Mag 16: 423-28, 1908.

88 A.H. Bucherer, "On the Principle of Relativity. A Reply to Mr. Cunningham". Phil Mag 16: 939-40, 1908.

this which he claimed as the motivation of his research.[89]

Bucherer's experiment differed from Kaufmann's mainly in that he used crossed rather than parallel fields. As with Kaufmann's arrangement, Bucherer used a salt of radium as the source of his energetic electrons.

At the 80th Naturforscherversammlung at Köln in 1908, response to Bucherer's report of his results varied widely. On the one hand, Minkowski, whose theoretical researches had only themselves recently been published[90] was ecstatic:

> Ich will meiner Freude darüber Ausdruck geben, die experimentellen Ergebnisse zugunsten der Lorentzschen Theorie gegenüber der des starren Elektrons sprechen zu sehen. Dass dem eines Tages so seine wurde, konnte vom theoretischen Standpunkte aus gar nicht zwefelhaft sein. Dass starre Elektron is meiner Ansicht nach ein Monstrom in Gesellschaft der Maxwellschen Gleichungen, deren innerste Harmonie das Relativitätsprinzip ist. Wenn man mit der Idee des starren Elektrons an die Maxwellschen Gleichungen herangeht, so kommt mir das geradeso vor, wie wenn man in ein Konzert hineingeht und man hat sich die Ohren mit Watteropfen verstopt...Des starren Elektron is keine Arbeitshypothese, sondern ein Arbeitshindernis.[91]

89
A.H. Bucherer, "Messungen an Bequerelstrahlen", Verh d phys Ges 6: 688-99, 1909.

90
See below.

91
A.H. Bucherer, "Messungen an Bequerelstrahlen. Die Experimentelle Bestätigung der Lorentz-Einsteinschen Theorie." Phy Zs. 9: 755-62, 1908, p. 760.

"I would like to express my joy at seeing the experimental results in favor of the Lorentz theory as opposed to the theory of rigid electrons being expressed. That it would happen some day could be seen without doubt from a theoretical point of view. In my opinion, the rigid electron is like a monster in the presence of Maxwell's equations, whose central harmony is the principle of relativity. If one brings the idea of rigid electrons alongside Maxwell's equations, it seems to be analogous to a man who goes to a concert with his ears stuffed with cotton...The rigid electron is not a working hypothesis, but a working hindrance."

While this statement by Minkowski reveals a definite leaning toward

On the other hand, Bestelmeyer was skeptical. In fact, Bestelmeyer believed that the experimental error was so great that he doubted whether Bucherer could discriminate between theories with his data any better than Kaufmann.[92] These remarks by Bestelmeyer plunged Bucherer into yet another public debate.[93] While Bucherer himself revealed more and more understanding of the import of Einstein's theory and the distinctions between it and the Lorentz theory, Bestelmeyer's attack had proved effective in making others hesitant about the experiment. For example, in his review of the experimental basis of relativity in 1910, J. Laub felt that it was too difficult to come to a conclusion concerning Bucherer's results.[94] And several years later, in 1912, Lorentz dismissed all of these earlier

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the principle of relativity and "Lorentz theory" his own understanding of the relationship between the work of Einstein and Lorentz requires careful consideration. See below.

92 Ibid., p. 761.

93 Bucherer, "Die Experimentelle Bestätigung des Relativitätsprinzips", Ann d phys 28: 513-36, 1909.

A. Bestelmeyer, "Bemerkungen zu der Abhandlung Hrn. A.H. Bucherer's 'Die Experimentelle Bestätigung des Relativitätsprinzips', Ann d phys 30: 166-74, 1909.

A.H. Bucherer, "Antwort auf die Iritik des Herrn E. Bestelmeyer bezüglich meiner experimentellen Bestätigung des Relativitätsprinzips", Ann d phys 30: 974-86, 1909.

94 Jakob Laub, "Uber die Experimentellen Grundlagen des Relativitätsprinzip", Jahrb. für Radioaktivität, u Elektronik 7: 405-63, 1910. p. 455.

experiments as "not leading to a definite solution."[95]

As we have seen, the original impetus for doing e/m measurements for swiftly moving electrons after Abraham published his theory in 1902, was the confirmation of the predictions of that theory. After 1904 and the emergence of the fully developed Lorentz theory, Kaufmann's experiments were motivated by a desire to distinguish between the two theories. The review of Kaufmann's attempts and the attempts of others like Bucherer reveals that the experiments were unsuccessful in this goal. Indeed, one may say that at the end of the period we are considering, 1911, no definitive results had been obtained from determinations of the specific charge of the electron which could establish the correctness of the predictions of the competing theories. While protagonists like Bucherer and Kaufmann could cite some particular experimental result for support, there was no agreement, no clear-cut choice to be made. Only Einstein remained above the disputes over data. His confidence was based not on agreement with data, but on his sense of rightness over the form of his theory.

Of all the individuals involved, Kaufmann and Abraham alone seem to have been the only ones who felt that it was necessary to respond to

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95 Lorentz, Lectures on Theoretical Physics, Vol. I, p. 272. Several determinations of e/m which were not mentioned in the text were performed during this period. In particular, we may cite the work of J. Classen, "Eine Neubestimmung von e/m für Kathodenstrahlen", Phys Zs. 9: 762, 1908; E. Hupka, "Beitrag zur Kenntnis der trägen Masse bewegter Elektronen", Ann d Phys 31: 169-204, 1910. Classen's results for the value of e/m agreed well with those of Bucherer and Bestelmeyer. Hupka used cathode rays to determine e/m and concluded that his data decided for the theory of relativity. His data was examined by Heil (W. Heil, "Discussion der Versuch über die träge Masse bewegter Elektronen", Ann d Phys 31: 519-46, 1910) who argued that Hupka's data could not discriminate between the theories of Abraham and Einstein. Cf., Hupka, Ann d Phys 31: 400-02; Heil, Ann d Phys 33: 403-13, 1910.

Einstein's theory as opposed to Lorentz' theory. To the others, the eclectic adjective "Lorentz-Einstein" meant that both theories were the same. Bucherer, it should be noted, while at first viewing Einstein's work as making explicit what was implicit in Lorentz' theory, later came to see that the theories were essentially different. But for most of the researchers this distinction, if it came at all, came later rather than earlier. Certainly the different viewpoints expressed by Lorentz and Einstein could be submerged easily in the fact that both theories made identical predictions of the change in the specific charge of the electron with velocity.

But there is no other data. Since the only comparative prediction made by the Abraham theory was the variation in the mass of moving charge, and since the Lorentz and Einstein theories agree in every prediction, there could be no other experimental distinction between the theories.

Though it was not amenable to experimental check, another prediction made by both theories had to do with the length of bodies in motion. The "Lorentz contraction," a postulate of the Lorentz theory and outcome of still more fundamental postulates in Einstein's theory, had caught the fancy of many physicists. Again, it should be emphasized that both theories made identical predictions. The Abraham theory made no <u>general</u> pronouncements about the length of a moving body, however, the fact that it assumed an absolutely rigid electron could be and was interpreted to mean that in the Abraham theory, length was an absolutely fixed quantity, independent of the relative motion of the object and the observer.

At the very same time that the dispute over the meaning of the e/m measurements was occurring, a second dispute, stemming from the notion of length in the various theories was beginning to take shape. As we will try to show, this dispute was far more useful in the establishment of Einstein's theory over the others.

B. The Ehrenfest Paradox and the Length of A Moving Body

As we have seen, Abraham had shown that a non-spherical rigid electron could not maintain itself in inertial motion in all directions. Such a conclusion did not apply directly to the non-spherical Lorentz eletron, since that electron was deformable -- at least in the classical sense of rigidity. In 1907, Paul Ehrenfest[96] first raised the question of the applicability of the Abraham criticism to the Lorentz electron.[97] Stating that the "Lorentzian relativistic electrodynamics will become recognized rather generally as a closed system in the formulation by Hr. Einstein,"[98] Ehrenfest suggested that this formulation should be able to provide a deductive answer to the question of whether or not the Lorentz deformable electron could move inertially.

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96 Paul Ehrenfest (1880-1933) studied at Wien and Göttingen. He wrote his doctoral dissertation under Boltzmann at Wien and received the degree in 1904. He was appointed to the chair in Theoretical Physics at Leiden in 1912 succeeding Lorentz to the post. Ehrenfest's fame was more as a teacher than as an innovator; however, all during his life, he raised interesting and troublesome questions with regard to the new innovations in quantum mechanics and relativity. Professor Martin Klein is currently preparing a biography of Ehrenfest.

97 P. Ehrenfest, "Die Translation deformierbarer Elektronen und der Flächensatz", Ann d Phys 23: 204-05, 1907.

98 Ibid., p. 204.

If such a motion were not possible for the Lorentz electron, Ehrenfest pointed out that one would have an instrument for the determination of absolute rest. On the other hand if such a motion were possible, Ehrenfest challenged proponents of the Einsteinian system to provide a proof.

Einstein's response, which appeared in the pages of the Annalen der Physik immediately following Ehrenfest's paper[99] is one of the few examples we have of him making a direct answer to such a challenge. Einstein began by denying that he had provided a complete system:

> Das Relativitätsprinzip oder --genau ausgedrückt--das Relativitätsprinzip zusammen mit dem Prinzip von der Konstanz der Lichtgeschwindigkeit ist nicht als ein "abgeschlossenen System," ja überhaupt nicht als System aufzufassen, sonder lediglich als ein heuristisches Prinzip, welches für sich allein betrachtet nur Aussagen über starre Körper, Uhren und Lichtsignale enthält. Wieteres liefert die Relativitätstheorie nur dadurch, dass sie Beziehungen zwischen sonst voneinander unabhängig erscheinenden Gesetzmässigkeiten fordert.[100]

Einstein then proceeded to explain how the deformable electron was a kinematic consequence of the Lorentz transformations for electrons. He pointed out that to do anything more, for example, to attempt to express the laws of motion of electrons using dynamical assumptions, would require

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99 A. Einstein, "Bemerkungen zu der Notiz von Hrn. Paul Ehrenfest: 'Die Translation deformierbarer Elektronen und der Flächensatz", Ann d Phys 23: 206-08, 1907.

100 Ibid., p. 206. "The principle of relativity or--more precisely expressed--the principle of relativity together with the principle of the constancy of the velocity of light cannot be conceived of as a "complete system," indeed, it should not be conceived of as a system at all, but merely as a heuristic principle which regarded by itself, contains only assertions about rigid bodies, clocks and light signals. The theory of relativity delivers further information only by virtue of the fact that it requires relationships between regularities which otherwise appear to be independent of each other."

a number of additional hypotheses which did not seem justified on the basis of the little information available. The problem, Einstein remarked, could not be solved strictly within the framework of electrodynamics. One would also require a theory of rigid bodies.

This remark was consistent with Einstein's view that theory of relativity was a heuristic theory. In the next few years, several attempts were made to develop the necessary theory of rigidity. The majority of these attempts made use of the Minkowskian 4-dimensional representation of the theory of relativity.

On the other hand, many physicists did not see the need for a new theory of rigidity. We then turn to the Minkowskian Formulation and the theory of rigid bodies.

1. The Minkowski Formulation

It is a common view that Hermann Minkowski introduced the 4-dimensional representation as an alternative formulation to Einstein's. This view holds that Minkowski saw a way of making the theory more mathematically harmonious. It is difficult to find evidence for this in Minkowski's own work.

Minkowski's first presentation of his theory appeared in 1908, in a very lengthy paper entitled, "Die Grundgleichungen für die elektromagnetischen Vorgänge in bewegten Körpern."[101] According to Whittaker, this was a paper "of great importance to relativity theory."[102] As Whittaker points out, the purpose of the paper, according to Minkowski, was to show that the equations for bodies in motion could be derived from the equations which

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101 H. Minkowski, "Die Grundgleichungen für elektromagnetiscen Vorgänge in bewegten Körpern", Gött Nach Ges. Wiss (1908), pp. 53-111.

102 Whittaker, loc. cit., p. 64.

described the same system at rest.

Minkowski distinguished three classes of statements concerning relativity: the theorem of relativity, a mathematical fact; the postulate of relativity, assumed in dealing with yet untested laws; and the principle of relativity, a statement of experimental fact. The body of Minkowski's paper dealt, for the most part, with the first class of statements.[103] He was primarily concerned with the interactions of radiation and ponderable matter. Thus, considerable effort is devoted to obtaining values for the permittivity and permeability of matter in motion from first principles and comparing values thus obtained with those obtained by Lorentz and others.[104] The only reference to Einstein's work in this first paper is a statement to the effect that Einstein has most clearly stated the principle of relativity and shown that it is not an artificial hypothesis.[105] Minkowski's view that Einstein had simply expressed clearly the role of the time in relationship to the principle of relativity was a view that he repeated later in the same year, (1908) in his famous lecture at the 80th Naturforscherversammlung at Cologne:

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103 In a private communication, Professor Albert Bork of Reed College has said that in his view, Minkowski, a mathematician whose major interest had been the theory of quadratics, had recognized that the Lorentz theory represented a physical situation where the quadratic form could be imposed with little difficulty and that this was Minkowski's main interest. Bork further comments that, in fact, in his view, Minkowski's own interest in and contribution to the theory of relativity were quite limited.

104 Minkowski, loc. cit., pp. 72 ff.

105 Ibid., p. 55.

> ...the credit of first recognizing clearly that the time of one electron is just as good as that of the other...belongs to A. Einstein.[106]

Whether or not Minkowski would have ever changed his position on the contribution of Einstein will never be known since Minkowski's untimely death occurred in the following year, 1909. But regardless of Minkowski's position, his four-dimensional formalism was quickly adopted by several people for discussing problems of the electrodynamics of bodies in motion and for general relativistic problems.

For example, Max Born, a student of Minkowski who completed some of the work Minkowski had left uncompleted at his death[107] observed that there had been three theories which attempted to correct the errors in Hertz's theory of the electrodynamics of moving bodies. He associated these theories with Lorentz, Cohn, and Minkowski. Each of the theories made different predictions of possible experimental results. To Born's mind, the Minkowski theory was a counterpart to Hertz's resting, not on the classical principle of relativity as Hertz's theory had, but on the principle of relativity presented by Lorentz, FitzGerald, and Einstein.[108] Though he did not say so explicitly, the reason that Einstein's theory was not a consideration was that Einstein had not provided a theory of <u>material</u> bodies.

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106 H. Minkowski, "Raum und Zeit", <u>Phys Zs</u>. 104-111, 1909; tr. W. Perret and G.B. Jeffrey in Lorentz et al, <u>The Principle of Relativity</u>, pp. 75-91. The quotation is from the Perret and Jeffrey translation, p. 82.

107 M. Born, "Zur Elektrodynamik bewegter Körper," <u>Verh d phys Ges 12:</u> 457-67, 730, 1910.

Cf. M. Born, "Eine Ableitung der Grundgleichungen...Aus dem Nachlass von Hermann Minkowski Bearbeitung", <u>Math Ann 68</u>: 526-51, 1910.

108 Born, "Zur Elektrodynamik...", p. 458.

At this stage of his career, Born was most concerned about a dynamical theory of radiation and matter. In the pages of the Annalen der Physik in 1909, Born noted that on the one hand the Abraham theory of the rigid electron did not satisfy the principle of relativity, but on the other hand, the electrodynamics of Lorentz and Einstein, based on that principle, did not provide a satisfactory explanation of mass.[109] Without giving further physical insight into the physical significance of the terms of the equations, Born constructed a system of equations in which mass played the role of the Lagrangian multiplier.[110]

2. Born's definition of rigidity

It was against such a backdrop that later in 1909, Born introduced a new definition of rigidity.[111] The new definition was intended to be consistent with the principle of relativity, satisfying the Lorentz-FitzGerald contraction. Born's immediate purpose was to apply such a definition to the moving electron. The analysis was based on the Minkowski formalism.

It is not surprising to find in this initial paper and in the subsequent contributions which Born made to the problem of the rigid body that the emphasis is on the mathematical formulation as opposed to the physical consequence. As Phillip Franck has pointed out, both he and Born first became drawn to their interest in relativity theory by Minkowski's work.[112]

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109 M. Born, "Die Träge Masse und das Relativitätsprinzip", Ann d Phys 28: 571-84, 1909. p. 571.

110 Ibid.

111 M. Born, "Die Theory des Starren Elektrons in der Kinematik des Relativitätsprinzip", Ann d Phys 30: 1-56, 840, 1909.

112 Personal communication, December 7, 1964.

In a sense then, Born's investigation of the nature of the rigid body cannot properly be called a response to Einstein's theory as much as it can be considered a refinement and application of the new mathematical formulation of Minkowski.[113] However, as we point out in section I, one of the features of the German response and one we will find absent in the response in the other countries under consideration is that German physicists elaborated on the original theory. It was just this kind of elaboration which eventually overcame the early resistance to the theory.

As Born made perfectly clear at the beginning of his first paper on the definition of a rigid body, any new definition would not only have to satisfy the principle of relativity, it would also have to reduce to the classical notion of rigidity, namely that the length of a rigid body is independent of time and motion when the velocity of light is taken to be infinite, or, conversely, when the velocity of the moving object is small in comparison to the velocity of light.[114] While Born restricted himself to simple straightline motion and excluded rotary motion, he claimed that the analysis that he had provided was quite general. As to the practical value of the analysis, the new definition of rigidity would be immediately applicable to the theory of electrons.

As his teacher Minkowski, Born was repelled by the mathematical complications of the Abraham theory. However, unlike Minkowski, Born recognized that experiment had not decided the question. If he were going

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113 Ibid. Franck pointed out that Einstein himself was at first not interested in the four dimensional treatment of relativity. He found it very difficult to understand what Minkowski and Born were doing. He also expressed to Franck a distaste for such a treatment on the grounds that he did not consider Minkowski a very good expositor.

114 Born, "Die Theorie des Starren Elektron...", pp. 2-3.

to reject the Abraham electron, it would have to be on different criteria.[115] For one thing, the Abraham electron did not satisfy the principle of relativity. And while Born noted that the Lorentz electron satisfied the experimental evidence as well as the Abraham theory, heretofore, no one had given a satisfactory response to Abraham's criticism of the Lorentz electron; namely, that in certain directions a non-spherical electron would not move inertially.

Born's program may be summarized as follows: First, define the rigid body in such a way that the definition takes into account the Lorentz-FitzGerald contraction. Then apply this definition to the motion of electrons and demonstrate that both the Abraham and the Lorentz formulas for the dependence of mass on velocity predict equally well the actual physical situation. At the same time, using the new definition of rigidity, show that the Abraham criticism of the Lorentz electron is groundless. And finally, reject the Abraham formulation as being too complex and un-natural. It should again be emphasized that at this point in time, Born still held that Einstein had reformulated the principle of relativity of "Lorentz and FitzGerald." Born was not modest about the importance and significance of the new definition of rigidity:

> Meine Starrheitsdefinition erweist sich dem Systeme der Maxwellschen Elektrodynamik als durchaus in derselben Weise angemessen, wie die alte Definition der Starrheit dem Systeme der Galilei-Newtonschen Mechanik. Das in diesem Sinne starre Elektron stellt die dynamisch

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115 *Ibid.*, p.4,

einfachste Elektrizitätsbewegung dar. Man kann sogar so weit gehen, zu behaupten dass die Theorie deutliche Hinweise auf die atomistische Struktur der Elektrizität liefert, was in der Abrahamschen Theorie keineswegs der Fall ist.[116]

Born's definition of rigidity was based on a direct analogy to the definition in classical mechanics. Classically, a body is considered rigid if the distance between two points on the body given be the equation

$$r = [(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2]^{1/2} \quad \ldots . \; 1$$

is independent of the time, where r is the distance between the two points on the body and x,y, and z are the spatial coordinates in some three dimensional frame of reference.[117] Of course, there is a differential version of the above equation:

$$ds = (dx^2 + dy^2 + dz^2)^{1/2} \quad \ldots\ldots\ldots . \; 2$$

Using the Minkowskian representation, there are two alternative ways of expressing the classical definition of rigidity. The first states that the distance between two infinitesimally close world lines representing point on a rigid body remains constant. The second states that the time rate of change of the elements of the deformation matrix describing the body be identically equal to zero.

The relativistic formulation which Born gave to the definition of rigidity was strictly analogous to the classical definition. Relativistically,

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116 Ibid., pp.5-6. "My definition of rigidity is as precisely suitable to the system of Maxwellian electrodynamics as the old definition of rigidity is to the system of Galilean-Newtonian Mechanics. The electron which is rigid in this sense represents the dynamically simplest motion of electricity. One can even go so far as to assert that the theory produces a clear indication of the atomic structure of electricity, which is in no way the case in the Abraham theory.

117 Ibid., pp. 11-15.

distance by itself is not an invariant. However, the fundamental invariant

$$c^2t^2 - (x^2 + y^2 + z^2) = F$$

may be considered as analogous to (1). Again, as in the classical case, one can define the rigid body as one in which the distance between two infinitesimally close world lines representing the body remains constant. Though the language is the same for both the classical and relativistic cases, the physical meaning is very different. The 3X3 classical deformation matrix becomes, relativistically, a 4X4 deformation matrix and the definition of rigidity that the time rate of change of the deformation matrix is zero is understood, relativistically, to mean, the proper time.[117]

It was obvious that such a formulation could handle easily only the simplest kind of motion. Born restricted his considerations to straight line translations of bodies. Such a simplification led to the following equation describing the world lines of a rigid body:

$$c^2t^2 - x^2 = \mathcal{E}$$

the equation of a set of hyperbolae in the x,t plane. The asymptotes of these hyperbolae correspond to the velocity of light. As Born pointed out, using such a formualtion one could consider classical kinematics to have only one degree of freedom and the new formulation indeed reduced to the classical case when one took the velocity of light to be infinite.[118]

Born now applied this concept of hyperbolic motion to the case of the dynamics of moving electrons. Three of the criteria Born invoked in this

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[118] Ibid., pp. 23-25.

application of his definition were:

1. The equations of motion must be invariant under a Lorentz transformation.
2. Only quasi-stationary motion must be considered.
3. The rigid electron must move in such a way that the resultant of its own self excited field and the external field equal zero.[119]

Using the Minkowski formulation for the calculation of the field and applying the restrictions of hyperbolic motion described above, Born arrived in this way at the Lorentz formulation for the mass of the rigid electron.[120] Obviously, such an electron would not be rigid in the classical sense. Thus, Born felt that he had placed the dynamics of electrons on a purely electromagnetic basis in such a way as to satisfy the principle of relativity. While the motions that he had been able to consider were only of the simplest kind, one could feel confident in the calculations as long as the field did not change too rapidly.

Born reported the substance of this work at the Naturforscheversammlung at Saltzburg in 1909.[121] He again pointed out that the work of Lorentz was very narrowly based and though Einstein had talked about the inertial motion of electrons, he had done so without utilizing an electrodynamic basis. It was this which Born felt was a major advantage of his work. As to the notion of rigidity, Born justified the need for a new definition on the grounds that the old definition was inconsistent with the principle of relativity and that there was no invariant which corresponded to the classical conception of length.

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119 Ibid., p. 46.

120 Ibid., pp. 47-52.

121 M. Born, "Uber die Dynamik das Elektrons in der Kinematik des Relativitätsprinzip", Phy Zs. 10: 814-17, 1909.

The major comment on Born's paper at the Saltzurg meeting was by Arnold Sommerfeld. While expressing respect for the power of Born's analysis, Sommerfeld questioned whether the principle of relativity could say anything about cases involving accelerated motion.[122]

This remark by Sommerfeld foreshadowed the dispute which followed and which was begun in earnest by Paul Ehrenfest.[123] Ehrenfest correctly interpreted Born's definition of rigidity as follows: the definition which corresponds to the principle of relativity is based on the coordinate systems of a continuum of infinitesimal observers that travel with the points which are in motion. This being the case, Ehrenfest saw a paradox resulting. Consider a cylinder of radius R and height h which is taken to be rigid in the usual (classical) sense. Let it gradually attain a constant rotational motion about its axis. Let R' be the radius of the cylinder once it attains such a motion. Then, according to Ehrenfest, R' must satisfy two mutually contradictory conditions. To the observer in the laboratory frame of reference the cylinder must show a contration around the circumference since every element of the periphery moves tangentially with an instantaneous velocity R'w. On the other hand, if one considers any radius of the cylinder, that radius is moving in a direction perpendicular to its extention and therefore does not suffer a contraction. We thus have a cylinder whose radius is everywhere R but whose circumference is not $2\pi R$.

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122 *Ibid.*, p. 817.

123 P. Ehrenfest, "Gleichformige Rotation starrer Körper und Relativitätstheorie", *Phys Zs.* 10: 918, 1909.

But as Herglotz[124] pointed out, another interpretation was possible. Born's original definition only allowed for three degrees of freedom for the rigid body rather than the usual six. That being the case, the only time a rigid body could attain a state of uniform rotational motion was in the event that one of the points of the body was held fixed and was not allowed to translate. In particular, Herglotz interpreted Ehrenfest's hypothesis to mean that a body originally at rest cannot be placed in uniform circular motion under the Born definition of rigidity.[125]

Born readily acknowledged these shortcomings to his definition once they were pointed out by Herglotz and Ehrenfest; however, he was not willing therefore to immediately give up the definition. He still saw his definition as providing a means for a natural mechanics of electrons. It was in agreement with the principle of relativity and as there was no experimental evidence of the structure of the electron, one could treat it as a point mass which did not rotate.[126]

Even so, Born was willing to recognize that his definition of rigidity

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124
G. Herglotz, "Über den vom Standpunkt des Relativitätsprinzips aus als "starr" zu bezeichenden Körper", <u>Ann d Phys 31</u>: 393-415, 1910.
Cf. Herglotz, "Bewegung "starrer" Körper und Relativitätstheorie (Bemerkung zu den Arbeit von P. Ehrenfest)", <u>Phys Zs. 10</u>: 997, 1909.
F. Noether, "Zur Kinematik des starren Körpers in der Relativtheorie", <u>Ann d Phys 31</u>: 919-44, 1910.

125
Herglotz, Über den vom Standpunkt...", p. 393, fn. 2.

126
M. Born, "Über die Definition des starren Körpers in der Kinematik des Relativitätstheorie", <u>Phys Zs 11</u>: 233-34, 1910.

would not suffice for the mechanics of "ordinary rigid bodies."[127] Nevertheless, the work represents a characteristic which, during the period under investigation, is unique. It is only in Germany that one can point to investigations which more than evaluate Einstein's work, elaborate it.

From a modern point of view, Born's work could be criticized on several counts. In his later work Born himself came to realize that the quest for a natural definition of mass was not a real problem and that he had overemphasized the importance of a definition of rigidity.[128] In 1909-10, Born's concerns and approach were in fact quite similar to those of Abraham and Lorentz. He desired "natural definitions." He saw the possibility of a unified physics based on electrodynamics. Perhaps because of his youth and the fact that he had not already made well-defined and entrenched professional commitments, he was eventually able to overcome the preconceptions from which Abraham, Lorentz and other were not able to escape.

It remains the fact that Born's early work on rigidity did help to clarify the status of Einstein's theory with regard to changes in dimension and mass. For the matter did not rest with the exchange between Born, Ehrenfest, and Herglotz. In fact the dispute continued independent of Born's contributions. It will be profitable to pursue the matter a little further.

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127 It should be noted tha even though Born defended his definition of rigidity as useful to the dynamics of electrons, within several months of the publication of the article cited above (footnote 126) he had proposed a new definition of rigidity based on the velocity of infinitesimal points in a body. Cf. M. Born, "Zur Kinematik des starren Körpers im System des Relativitätsprinzips", Gött Nach Ges Wiss, (1910), pp. 161-79. The new definition allowed for six degrees of freedom.

128 Cf. M. Born, Einstein's Theory of Relativity, (New York: Dover Publications, Inc., 1962). pp. 207-14.

Max Planck suggested that the Ehrenfest paradox was the result of a misunderstanding. The heart of the problem, maintained Planck, was in the domain of the theory of elasticity, and since nothing had been said about the state variables of the body, nothing could be concluded.[129] Abraham, on the other hand, saw this paradox as another reason for doubting whether the theory of relativity could lead to a logical representation of mechanics. Taking note of Planck's belief that the solution to the Ehrenfest paradox lay in the theory of elasticity, Abraham said:

> Nun wäre es zwar zu wünschen, dass die Anhänger der Relativtheorie davon Rechnenschaft geben, wie ein elastischer Körper sich verhält wenn er etwa adiabatisch in Drehung versetz wird.[130]

Others shared Abraham's skepticism. W. Ignatowsky claimed that if one analysed the process of synchronizing clocks, the proper conclusion was that the limiting velocity was not the velocity of light, but rather the velocity given by c^2/v where v is the velocity of the frame of reference in which the measurement is made.[131] Sommerfeld[132] questioned this result stating that in his opinion Ignatowsky was not talking about signal velocities at all. When Ignatowsky replied that he arrived at the same result for the transmission of signals through rigid bodies, Sommerfeld countered with the reply that the whole notion of rigidity had to be changed in relativistic physics.

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129 M. Planck, "Gleichformige Rotation und Lorentz Kontraktion", Phys Zs 11: 294, 1910.

130 M. Abraham, "Die Bewegungsgleichungen eines Massenteilchen in der Relativtheorie", Phys Zs 11: 527-31, 1910. p. 531. "Now it would indeed be hoped, that the adherents to the theory of relativity would thereupon give an account of how an elastic body maintains itself when it is adiabatically placed in rotation."

131 W. von Ignatowsky, "Eine Allgemeine Bemerkungen zu Relativitätsprinzip", Phys Zs 11: 972-76, 1910.

132 Ibid.

Ignatowsky, however, maintained that his conclusion applied even to the Born definition of rigidity. Ignatowsky's result, which is based on an acceptance of the postulate of relativity and a rejection of the postulate of the constancy of the velocity of light contains a paradox of its own--a paradox which none of his commentors have noted. If the limiting velocity is given by c^2/v one has a criterion for determining absolute motion and absolute rest. A frame of reference in which the signal velocity through a rigid body is given by c^2 would be a system absolutely at rest. The fascinating aspect of Ignatowsky's work is that no one saw fit to criticize him on this point. Attention was focused rather on the nature of rigid bodies and Ignatowsky's analysis of the Ehrenfest paradox.[133]

According to Ignatowsky the whole problem was an illusion, The result would be completely explained if one took into account Ignatowsky's definition of synchronous measurement. Only when an object is at rest do we get a true picture of its form and dimensions.

In response, Ehrenfest questioned Ignatowsky's meaning of "synchronous measurement" and asked for clarification. At the same time, Ehrenfest requested that Ignatowsky elaborate on his statement that measurements of moving bodies give only apparent values. For example, Ehrenfest asked, if templates of the body in motion and at rest agree or not.[134]

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133 W. von Ignatowsky, "Der Starre Körper und das Relativitätsprinzip", Ann d Phys 33: 607-630. 1910.

134 P. Ehrenfest, "Zu Herrn Ignatowsky's Behandlung der Born"schen Starrheitsdefinition", Phys Zs 11: 1127-29, 1910.

While Ignatowsky never responded to Ehrenfest's inquiry,[135] V. Varicak[136] did. According to Varicak there are two ways for viewing the paradox. The Lorentz view maintained that the contraction was real and objective. This was Ehrenfest's point of view. The Einstein viewpoint was that the contraction was subjective. This was the position of Ignatowsky.[137]

To Varicak there could be no doubt about the solution to the problem. Two bodies whose lengths were equal when at rest with respect to each other would always be the same length regardless of appearances. Einstein was correct.[138] Einstein himself saw fit to reply to Varicak.[139] He rejected the distinction that Varicak had tried to develop between his theory and Lorentz's saying that the question was not whether or not the contraction really occurred. Einstein argued that the Lorentz contraction was experimentally detectable in exactly the same way that simultaneity was experimentally detectable. In his hands, relativity remained yet, a kinematical subject.

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135 Cf. Ignatowsky, "Zur Elastizitäts Theorie vom Standpunkte des Relativitätsprinzips", Phys Zs 12: 164-69, 1911.

136 V. Varicak, "Zum Ehrenfestschen Paradoxon", Phys Zs 12: 169-170, 1911.

137 W. von Ignatowsky, "Zum Ehrenfestschen Paradoxon", Phys Zs 12: 414. 1911. This was a specific response to Varicak and not to Ehrenfest.

138 Varicak, loc. cit.

139 A. Einstein, "Zum Ehrenfestschen Paradox", Phys Zs 12: 509-10, 1911.

While Einstein was not willing to take on the question of the nature of rigid bodies directly, Max Von Laue was.[140] Laue identified one common concern behind the entire discussion of the nature of a rigid body, namely that in contrast to a deformable body which has an infinitude of degrees of freedom, a rigid body can only have a finite number of such degrees classically. Laue now undertook to show that the principle of relativity excludes this kind of distinction and as a result, the type of investigations which had thus far been undertaken were hopeless and had no prospects.[141] The number of degrees of freedom, Laue showed, has no upper bound in relativistic mechanics, regardless of the degree of rigidity the body possesses.

Laue's observation that the argument over the correct number of degrees of freedom was pointless and without end was correct. The difficultly had developed partly as a result of Born's desire to make the relativistic definition of a rigid body analogous to the classical definition and partly on misconceptions of the basis of the theory by Ehrenfest, Ignatowsky, and Varicak. The latter two men desired to retain the classical concept of a rigid body as one which did not change its form. But the confusion on the question of the nature of the rigid body and the Ehrenfest paradox was perhaps exacerbated by the fact that the theory of relativity was not only a new theory but a new theory whose basic assumptions and fundamental conclusions violated the common sense of two hundred and fifty years of

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140 M. von Laue, "Zur Diskussion über den starren Körper in der Relativitätstheorie", Phys Zs 12: 85-87, 1911.

141 Ibid.

Newtonian physics. There is no better example of this confusion than the publication of the work of Ignatowsky in the Annalen der Physik. The Annalen was the most prestigous physics journal in the world and yet it published this paper whose conclusions, that the signal velocity was given by the ratio of the square of the speed of light to the velocity of the frame of reference, contradicted its premise, the principle of relativity.

It would seem likely that under normal circumstances, a fundamental and obvious error such as this would have been spotted by as perceptive an editor as Max Planck. That it was not suggests that perhaps during the period shortly after the introduction of a radically new theory like the theory of relativity, some criteria for judging the validity of the contributions of physicists may not be operative.

Planck himself had already made many contributions to the theory of relativity. On the one hand, he had defended the theory against the attack of Abraham and Kaufmann; and on the other hand, together with a graduate student, he had investigated the implications of the theory in the most general and specific grounds. This latter work represents still yet another elaboration of Einstein's theory. It is to an investigation of that work that we now turn.

C. General Dynamics and the Thermodynamics of Moving Cavities

Holton has pointed out that Max Planck was "Einstein's earliest patron in Scientific circles."[142] We have already seen a certain sense in which this is demonstrated when we discussed earlier Planck's defense of the theory of relativity against the attacks by Kaufmann, Abraham, and others. At this stage, in 1906, Planck was not a committed proponent. It was his judgment, however, that there were certain internal characteristics of the theory which were very important and very attractive. By this time, it should be noted, Planck was already a venerated figure in scholarly circles. He was Professor of Physics at Berlin, Editor of the Annalen der Physik, and author of the most revolutionary hypothesis the world of physics had seen for some time: the quantum of action.

Max Planck was unalterably opposed to any form of idealism with regard to scientific knowledge. He opposed both the schools of Ostwald and Mach.[143] He was an absolutist whose search was ever for the fundamental, unchanging, universal equations of the world. In that sense, he was a child more of the Enlightenment than of the nineteenth century. Planck believed that the laws of human reasoning coincide with the laws governing the sequences of the impressions we received from the world around us; and that is why, he held, that pure reasoning can tell us something of the world. In fact, Planck went

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142 Gerald Holton, "Ou est la realite? Le response d' Einstein", Science et synthese (Gallimard, 1967) pp. 97-140.

143 Ibid.

so far as to say that the most sublime scientific pursuit was the search for absolute laws.[144] To Planck the most important, fundamental and invariant law of the universe was the principle of least action.

In March 1906, just prior to his first public examination of the Kaufmann experiment, Planck read the first of several elaborative papers on the Einstein theory.[145] The paper reveals several attitudes of Planck toward the theory and toward physics in general which we will have to consider. These may be summarized as follows:

1. Planck's consistent view that Einstein's theory was a generalization of the Lorentz theory.
2. Planck's view that simplicity be an important consideration in judging scientific theories.
3. The importance of elaborating on the implications of an idea like relativity even if it might later prove to be at variance with experiment.

1. Einstein's work as a generalization of Lorentz

The very first sentence of Planck's 1906 paper on the principle of relativity and least action gives a clear indication of Planck's attitude toward the theory:

> Das vor kurzem von H.A. Lorentz und in noch allgemeinerer Fassung von A. Einstein eingeführte "Prinzip der Relativität"...[146]

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144 M. Planck, Wissenschafliche Selbstbiographie (Liepzig, J.A. Barth, 1948), pp. 7-8.

145 M. Planck, "Das Prinzip der Relativität und die Grundgleichungen der Mechanik", Verh d p Ges 4: 136-41, 1906.

146 Ibid., p. 136. "The principle of relativity that was recently introduced by Lorentz and in a more general form by Einstein..."

The view expressed here by Planck that Einstein's theory was a generalization of the Lorentz view was repeated over and over by him in many ways.[147]

Given that Planck was the earliest and most outspoken defender of Einstein's theory, it seems somewhat paradoxical that he would not distinguish between the theories of Lorentz and Einstein, and recognize the completely different epistemic basis of the two theories. The answer to this paradox lies in Planck's view of the importance of the theory and the connection which he saw between the problems both Lorentz and Einstein were dealing with and his recently developed "quantum resonator." The importance of the principle of relativity was to Planck intimately related to his view that simple and general theories, in a formal sense, were both important and necessary. We turn to that issue.

2. Simplicity as a criterion in judging scientific theories

Even at the time that Kaufmann's data still looked convincing, Planck held that since the theory offered such a simplified view of electrodynamics, it deserved careful consideration:

> ...Ein physikalischer Gedanke von der Einfachheit und allgemeinheit wie der in dem Relativitätsprinzip enthaltene, verdient es, auf mehr als eine einzige Art geprüft, und, wenn er unrichtig ist, ad absurdum geführt zu werden; und dass kann auf keine besere

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[147] Cf. M. Planck, "Zur Dynamik bewegter Systeme", Siz Ak Wiss Ber 29: 542-70, 1907, p. 546.
M. Planck, "Die Stellung der nerren Physik zu mechanischen Naturanschauung", Phys Zs 11: 922-32, 1910, p. 929.
M. Planck, Achte Vorlesung Uber Theoretische Physik (Leipzig: S. Hirzel, 1910), pp. 116-121.

Weise gescheen als durch Aufsuchung der Konsequenzen zu denen er führt.[148]

Several years later, when he had become convinced of the correctness of the relativistic formalism, Planck expressed the view that any new "truth" like the principle of relativity had many difficulties to contend with: the chief one being in the case of relativity, that it forced us to reconsider our concept of time.[149] But this, he argued, should not be a deterrent for,

> Der Masstab für die Bewertung einer neuen physikalischen Hypothese liegt nicht in ihrer Anschaulichkeit, sondern in ihrer Leistungfähigkeit.[150]

So for Planck there had been a number of factors related to his conception of simplicity which had attracted him to the theory of relativity. The theory gave the laws of physics a simple formulation, it proved useful if not intuitive, and as we will develop later, the theory gave an invariant formulation to fundamental laws. Why then, given the fact that Planck had studied the theory with some intensity, did he not make a more sharp distinction between Einstein and Lorentz? Why did he consider Einstein's work to be a generalization of Lorentz?

148
Planck, "Das Prinzip der Relativität...", p. 137. "...a physical thought of the simplicity and generalness such as is contained in the principle of relativity deserves more than a single test and when it is incorrect, it should be carried to absurdity, and that can be done in no better way than by investigating the consequences to which it leads."

149
Planck, "Die Stellung der neuen Physik...", p. 928.

150
Ibid. "The measure of the value of a new hypothesis in physics is not its obviousness but its utility."

The problem is a puzzling and complex one and we can only begin to unravel the many threads of which it is composed. There are two main forces in Planck's life which may be cited as controlling in this issue: First, his emphasis on simplicity of form, and second, his devotion to classical physics.[151]

Given that Planck was mainly interested in the form of physical laws, this might by itself account for his willingness to submerge the differences in metaphysics that lay behind the results of the two theories. Planck rarely referred to "the theory of relativity"; almost always he referred to the "principle of relativity."[152] It was, after all, this principle which endowed physical laws with their simplicity of form in different frames of reference. It was the demands imposed by this principle which led Lorentz to concoct his transformation equations and it was the logic of the interaction of the principle of relativity together with the principle of the constancy of the velocity of light which yielded the derivation of those very same transformation equations by Einstein. That both theories made the same predictions using those transformation equations meant that any physical law which was invariant in one theory was invariant in the other. However, Planck was not unmindful of metaphysical distinctions in theories. Something even more compelling convinced Planck that the theories were manifestations of the same metaphysics.

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151. Cf. Martin Klein, "Einstein and the Wave-Particle Duality", <u>The Natural Philosopher 3</u>: 1-50, 1964. p.5.

152. Cf. fn. 148.

Max Planck was not interested in revolutionizing physics. As Klein pointed out with regard to quantum mechanics, Planck made lifelong attempts to reconcile the quantum idea with classical physics.[153] He maintained a very close contact with Lorentz, chiefly through letters, right up to the time that Lorentz died. The letters during the period we are considering reveal that even while Planck was defending the theory of relativity in public, he was engaged in a dialogue with Lorentz on the possibilities of wedding classical physics with the new ideas both in quantum mechanics and relativity. This is not to suggest that Planck was leading a double existence. Rather the situation is better described by saying that what Planck only hinted at or implied in public, was made much more explicit and much more plain in the letters, where, perhaps, Planck might feel much more relaxed about speculation.

Among the topics that Planck discussed with Lorentz were the possibility of an ether drift experiment[154] and the possible interactions that ether might have with what Planck termed "my quantum resonators."[155] While such

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153 M. Klein, "Thermodynamics and Quanta in Planck's Work", Physics Today 19 (Nov.), 1966.
CF G. Holton, "Ou est la realtite? Le response d' Einstein", p. 15
While Holton does not make explicit Planck's attitude toward special relativity, he does point out that while Planck's support for the special theory of relativity was unstinting, he was not willing to support the general theory nor was he willing to go along with Einstein's early contributions to quantum theory.

154. Planck to Lorentz, October 19, 1907. I am indebted to Professor Steven Brush who kindly provided me with a microfilm copy of selected correspondence of Lorentz.

155. Cf. Planck to Lorentz, April 1, 1908; June 16, 1909; October 7, 1908.

thoughts were being expressed by Planck in his private letters, the same thoughts, in a much more diminutive fashion are present in his published writings. Thus in his defense of Einstein against the attack of Kaufmann, Planck invariably referred to "Vacuum (Aether)". Such remarks can be interpreted to mean that for Planck, aether and vacuum were the same, or rather, they played the same role. Something had to play the role of supporting electromagnetic vibrations and Planck was willing to call that something "ether" or "vacuum". Its role remained unchanged. In fact, in 1909, Planck believed that though there were two large divisions in physics, the physics of matter and the physics of ether, the separation was disappearing.[156]

In sum, Planck's attitude toward Einstein's innovation seemed to be this: He welcomed the principle of relativity and the formulation of the new transformation equations because he recognized in them, almost immediately, the kind of absoluteness and universality which he valued so much in the life of science. But, at the same time, he was unwilling and perhaps unable to sever his connections with the past and in particular, with the ether. He strove, as his letters clearly indicate, for a rapproachment between the new and the old. Though Einstein's special theory of relativity received his support, that support was limited to the formalism, not to the parsimonious spirit of Einstein's thesis. Perhaps it was in part in reference to his own experience that Planck once wrote:

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[156] Planck, "Die Einheit des physikalischen Weltbiddes", p. 64.

Eine neue wissenschaftliche Warheit pflegt sich nicht ein der Weise durchsetzen dass ihre Gegner überzeugt werden und sich als belehrt erklären, sondern wielmehr dadurch, dass die Gegner allmählich aussterben und dass die heranwachse der Generation von vornherein mit der Wahrheit vertraut gemacht.[157]

3. Planck's elaboration of Einstein's Theory

The fact remains that Planck understood perhaps as well as anyone the formal and physical implications of Einstein's theory. Though it may seem somewhat strange in view of his metaphysical inertia, the source of this quick and penetrating understanding seems to have been his desire for a universal and absolute physics.

In his first elaboration of Einstein's work[158] Planck derived the relativistic generalization of the Newtonian equations of motion. He then went on to show what the relativistic least action formulation of these laws must be, and gave the equations of motion in the form attributed to Hamilton. As Planck pointed out, these equations would be valid in frames of reference which were related to each other by the Lorentz transformations.

Thus early in 1906, barely six months after Einstein had published his original paper on special relativity, Planck had taken the simple equations of motion which Einstein had provided and generalized them and investigated most of their theoretical subtleties. To one like Planck, who considered least action to be the most fundamental formulation of physical problems, he had gone about as far as one could go with relativistic mechanics.

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157 Planck, Wissenschaftliche Selbstbiographie, p. 22. "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

158 Planck, "Das Prinzip der Relativität..."

The least action formulation of relativity was invariant under a Lorentz transformation.

Planck was now ready to turn to a field he was already familiar with--blackbody radiation--and investigate the application of the principle of relativity to the problem of moving black bodies. His first paper on the subject was not published until June 1907, over a year after the publication of the paper we have just examined. During that interval Planck had obviously been occupied by the problems of relativistic thermodynamics, for shortly before the publication of his own work on the subject, the fruits of a doctoral thesis by one of Planck's students, von Mosengeil, appeared in the Annalen der Physik. The subject of this work was stationary radiation in inertial cavities.[159]

The influence of Planck on Mosengeil is quite evident. Mosengeil's acceptance and understanding of the theory of relativity was much like that of his teacher:

> Alle versuche, einen Einfluss der Erdgeschwindigkeit auf die elektrodynamischen Erscheinungen festzustellen, haben ein negatives Resultat ergeben. Um dies zu erklären haben H.A. Lorentz und in noch allgemeinerer Fassung A. Einstein das "Prinzip der Relativität" eingeführt, nach welchem es prinzipiell unmöglich ist, einen derartigen Einfluss aufzufinden.[160]

159
Kurd von Mosengeil, "Theorie der stationären Strahlung in einem gleichförmig bewegten Ulhlraum", Ann d Physik 22: 867-904, 1907.

160
Ibid., p. 867. "All investigations to establish the influence of the velocity of the earth on electrodynamic observations have given a negative result. In order to explain this, H.A. Lorentz and in a more general conception, A. Einstein, have introduced the "principle of relativity," according to which it is impossible in principle to detect such an influence."

As Mosengeil pointed out, the topic he was investigating had been looked at earlier by Hasenöhrl, and it was Mosengeil's hope to avoid some of the errors that he detected in Hasenöhrl's work. Prior to 1905, Hasenöhrl had published a series of papers dealing with the problems of moving black bodies.[161] Hasenöhrl was interested in determining the relationships between thermodynamic variables in moving black-bodies, the effects of light pressure and the energy of the radiation in the black body. He realized that it would be insufficient to simply use the laws of thermodynamics because the value of the radiation pressure on a moving surface must be derived from a special hypothesis about the nature of radiant energy. In fact, Hasenöhrl used the analysis of light pressure already provided by Abraham on the basis of the Lorentz theory.[162] Hasenöhrl's commitment to an absolute frame of reference was central to his analysis.[163] Using these assumptions, Hasenöhrl came to two conclusions worth noting. First, he arrived at the result that the energy in a moving black body was proportional to the mass equivalent of the radiation.[164] In particular

$$E = 3/8\ mc^2$$

where c is the speed of light. The second conclusion was that one had

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161 F. Hasenöhrl, "Zur Theorie der Strahlung in bewegten Körpern", Ann d Phys 15: 344-70, 1904.

162 Ibid., p. 334.

163 Ibid., pp. 334-47.

164 Ibid., p. 363.

to consider the cavity to contract in the direction of motion if one did not want to encounter a violation of the second law of thermodynamics when one considered adiabatic processes in the cavity.[165] This entire analysis depended on the assumption of Lambert's cosine law for moving black bodies and the isotropy of the energy density. Abraham almost immediately pointed out that there was an error of calculation which altered the energy mass relationship to [166]

$$E = 3/4\ mc^2$$

Mosengeil claimed that Hasenöhrl had assumed too much by assuming the Lambert relationship; all one had to assume was that the radiation at any given angle was the same in the forward and backward directions. And without making any assumptions about the contraction of bodies in the direction of motion, but simply using the relationships derived by Einstein in his 1905 paper on relativity for light intensity, frequency, and angle, Mosengeil derived essentially all of the results derived by Hasenöhrl three years earlier.[167] It was Mosengeil's claim that he had completely undercut any meaning that the Hasenöhrl derivations might have had. Mosengeil withheld judgment on the validity of the theory of relativity for ponderable matter, but he was obviously committed to the formalism for the handling of problems of radiation.

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165 Ibid., pp. 364 ff.

166 Ibid., p. 363.

167 M. Planck, "Zur Dynamik bewegter Systeme", Sitz Ak. Wiss Ber 29: 542-70, 1907.

Mosengeil's work was followed shortly by another contribution by Planck which based itself in part on Mosengeil's conclusions.[167] Planck began by again asserting the primacy of the Principle of Least Action which he claimed remained the foundation of general dynamics, embracing mechanics, electrodynamics, and the two fundamental laws of thermodynamics. But, the Principle of Least Action is not enough for a complete dynamics since there is not substitute for a division of energy into translational and inner components. Planck's purpose was to combine the principle of least action with the theory of relativity to create a general dynamics.[168]

Planck devoted much space in this paper to the relationship between thermodynamic variables in inertial frames of reference and showed how volume and temperature were related by the Lorentz transformations; he showed that pressure and entropy were invariants and that in general, the action, $\int_1^2 H\,dt$ has the same value in all inertial frames of reference.[169] Finally, Planck derived the relationship $E = mc^2$ for a moving cavity, a relationship that had been derived earlier by Einstein for a very special case.[170]

Between them, Mosengeil and Planck had essentially finished the subject of the thermodynamics of bodies in relativity uniform motion with respect to each other. Their results may be read in summary form

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168 Ibid., p. 546.

169 Ibid., p. 560.

170 Ibid., p. 564.

in any standard textbook on the subject.[171] It was an impressive display of virtuosity, especially on the part of Planck, who, it must be remembered, was not completely committed to Einstein's formulation of the theory of relativity.

But at the time, matters were not so clear. Hasenöhrl, for example, could not accept the position of Planck and Mosengeil, even though he could now arrive at exactly the same results arrived at by both Planck and Mosengeil.[172] Hasenöhrl was unwilling to accept the assumptions of the theory of relativity. It was not that he did not understand them. For example, with regard to the change in volume undergone by a moving cavity, Planck had invoked the length contraction of the Einstein theory. Hasenöhrl recognized that the Lorentz contraction was equal in magnitude, however:

> Es stimmt dies mit der Kontraktionhypothese von H.A. Lorentz, sowie mit den Sätzen, die Herr Planck aus dem sogenannten Relativitätsprinzip abegleitet hat, überein.
>
> Während Herr Planck die Gültigkeit des Relativitätsprinzips von vornherein annimmt, sind wir gewissermassen zu einem Beweise der Kontraktionhypothese gelangt, in dem wir den Satz postulierten, dass eine gemeinsame Translationsbewegung für einen mitbewegten Beobachter nicht wahrnembar ist;...[173]

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171 Cf. R.C. Tolman, Relativity, Thermodynamics and Cosmology (Oxford: Oxford University Press, 1933).

172 F. Hasenöhrl, "Zur Thermodynamik bewegter System", Sitz Ak Wiss Wien 116: 1391-1405, 1907.

173 Ibid., p. 1405. "This agrees with the contraction hypothesis of H.A. Lorentz as well as with the theorems that Planck has derived from the so called principle of relativity.

While Planck assumed the validity of the principle of relativity, we arrive as it were at a knowledge of the contraction hypothesis in that we postulate the theorem that a uniform translational motion is not perceptable to the observer moving with the object;..."

Hasenöhrl also took issue with Mosengeil's claim that the meaning of his, Hasenöhrl's work, had been completely undercut because one need not assume the Lambert law. As Hasenöhrl correctly noted, all that meant was that there was more than one way at arriving at the same result.[174]

Hasenöhrl's insistence on using the Lorentz theory as opposed to the Einstein theory could not be shaken. For example, in 1909, he authored a review of the whole area of the Inertia of Energy.[175] While taking account once again of the fact that all of the results obtained by the theory of relativity were in agreement with those of the Lorentz theory, Hasenöhrl made a point of saying:

> Es soll demnach hier nur davon die Rede sein, wie man vom Standpunkt der eigentlichen Lorentzschen Theorie zum Begriffe der Trägheit der Energie gelangt. Wohl steht das Relativitätsprinzip mit seiner blendenden Eleganz jetz im Vordegrunde des Interesses der theoretischen Physik; doch soll darum gewiss nicht die Arbeit an der Theorie, die auf der Verstellung eines ruhenden Äthers beruht, vernachlässigt werden.[176]

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174 F. Hasenöhrl, "Zur Thermodynamik Bewegter Systeme", Sitz Ak Wiss Wien 117: 267-215, 1908.

175 F. Hasenöhrl, "Uber die Trägheit der Energie", Jahrbuch der Radiaaktivität u Elektronik 6: 485-502, 1909.

176 Ibid., p. 485. "Here we will only speak of how one can obtain a conception of the inertia of energy from the standpoint of the true Lorentz theory. No doubt the principle of relativity with its blinding elegance is now in the forefront of interest of theoretical physics; however surely the work based on the hypothesis of a resting ether cannot be ignored."

But for all that, it would be a misreading of Planck's own contribution to assume that because he utilized the principle of relativity, he had completely rejected the ether. With regard to the theory of relativity, Planck was always cautious and as we have indicated earlier, he himself never completely rejected the ether.

D. Summary

In the above pages, we have surveyed the reception of relativity in Germany with regard to three problems: the mass of swiftly moving electrons, the concept of a rigid body, and the investigation of general dynamics. It would be quite impossible to catalog other response in the detail we have thus far provided because of the magnitude of the response. Suffice it to say that the attack on the problems we have discussed is typical of the kind of response in Germany during the period 1905-1911. As far as we have gone in this study, it may be difficult to discern any pattern which might be characterized as a national response. For example, Planck, Born, Laue, might all be characterized as supporters each with his own particular point of view. Abraham, Kaufmann, Hasenöhrl might be characterized as opponents of the theory, each with his own point of view. Nevertheless, there are several features of the German response which are distinctive and which deserve some attention.

First, one must take note of the depth and breadth of the response in Germany. As will be developed in later sections, in no other country was there anything like the activity we have recorded in this section. In fact, in later sections, we will find almost no concern with the substantive problems that have been dealt with here.

Second, the German response can be characterized as serious. That is, regardless of their positions, German physicists took the theory seriously. Thus, Abraham, Kaufmann, and Hasenöhrl all rejected the theory. Nevertheless, their grasp of its implications was accurate. They chose not to accept it because of their commitments to each other and to the concept of absolute motion.

Third, the German response may be characterized as diversified. This is also unique to Germany. The superficial observation that there is no national German response because of the diversity of that response does not take into account the fact that it was only in Germany that there was such a diverse reaction.

The diversity in the reaction to Einstein's theory can only be understood in individual terms. One can cite for example, Planck's commitment to absolute truths as making him unwilling to reject a theory which held promise, to him, of such truths. One can cite Abraham's commitment to absolute space or his long involvement in the theory of radio transmission as pre-conditioning his unwillingness to accept a theory which did away with the ether--at one and the same time the benchmark of absolute space and the supporter of electromagnetic radiation.

But it is the diversity itself which is the characteristic which requires explanation. It is this which is distinctive about the German response. There was another feature of the German scene which was also distinctive. That feature was the structure of the German educational system. Only in Germany were there many centers of learning, most of them recognized for their greatness and all of them vying for the best men. Bucherer at Bonn, Bestelmeyer at Göttingen, Classen at Hamburg, Hasenöhrl at Wien, Herglotz at Leipzig, Kaufmann at Köningsberg, Laue at Berlin, Planck at Berlin, Stark at Aachen, Minkowski at Göttingen, Sommerfeld at München, Wien at Würtzberg, Born at Göttingen, are only a few of the individuals who can be cited as playing some role in the response to Einstein's theory. Each of the Universities cited (and the list is only partial) had doctoral programs enlisting many students. Each semester, the courses in physics offered at German speaking universities in Europe were listed in a catalog published in the <u>Physikalische Zeitschriftandas</u> is well known, it was not unusualy for a student studying at one institution to travel to another for a particular professor or course. But not only did students travel between institutions, instructors traveled as well:

> The migration of students as well as of eminent professors from one university to another is one of the most important features of German academic life....No one university has been allowed to retain for any length of time the supremacy in any single branch. The light has quickly been diffused all over the country, when once kindled at one point.[177]
>
> Wherever the progress of learning and science requires a large amount of detailed study inspired by a few leading ideas, or subservient to some common design and plan, the German universities and higher schools supply a well trained army of workers standing under the intellectual

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177 Merz, <u>loc. cit</u>., pp. 162-63, fn.

> generalship of a few great leading minds. Thus it is that no nation in modern times has so many schools of thought and learning as Germany...The university system, in one word, not only teaches knowledge, but above all it teaches research. This is its pride and the foundation of its fame.[178]

With the kind of intellectual competition and alertness that one would expect in such an environment, it should not be surprising that Einstein's theory received a variety of reaction from various men at various centers. In such an atmosphere, it would not be surprising either to find those, who like Planck, were attracted to the theory, elaborating the ideas contained within the theory in an effort to convince, counter, or confirm the criticisms and comments of peers at their own and other institutions.

It should not be concluded from this that the Theory was accepted readily in Germany, As we have seen, men like Minkowski, Planck, and Born were all attracted to the theory for different reasons, and as we have indicated, these reasons, for the most, had little to do with Einstein's own view of his work. But the theory was discussed and taken seriously and this in itself would almost insure the kind of elaboration necessary for eventual acceptance or rejection.

178. Ibid., p. 167. Emphasis in original.

IV. The French Response to Einstein's Theory of Relativity

In a recently published paper[179] we have alluded to the fact that there was a remarkable silence with regard to Einstein's theory in France during the years 1905-1911. In that paper, which is attached as Appendix I of this report, we have argued that contrary to the belief of some historians of science, Henri Poincaré did not anticipate Einstein's theory, but together with Lorentz was embarked on an entirely different program, the establishment of a comprehensive and unified explanation of the physical world, The Lorentz Theory of Electrons. We have also pointed out that there was a discrepancy between Poincaré's philosophical writings and his work in physics. On the one hand Poincaré was a philosophical conventionalist. On the other hand, when doing physics, he treated problems as a realist, with a faith in induction rivaling the faith of Francis Bacon.

Here we would like to extend the analysis provided in the earlier paper in two directions: First, to understand, if possible, Poincaré's lack of response, and second, to describe and account for the lack of response on the part of the rest of the French physics community.

A. The Silence of Poincaré

From the time of its publication in 1905 until the time of his death in 1912, Henri Poincaré never mentioned Einstein in connection

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[179] Stanley Goldberg, "Henri Poincare and Einstein's Theory of Relativity", Am. J. Phys. 35: 933-44, 1967.

with the theory of relativity. There are several ways that one could attempt to explain Poincare's silence. On the one hand, it might be held that personal animosity had developed somehow between them. Perhaps Poincaré felt that Einstein had received credit for work that he, Poincaré, had done. Perhaps it was jealousy generated by the realization on the part of Poincaré that Einstein had been able to put together the elements of a problem in a remarkable simple way---a problem that Poincaré had struggled with for ten years. In fact, there is almost no evidence to support either of these theories. And everything that we know about Poincaré's character belies motivations as petty as jealousy and/or a sense of being robbed. Consider the éloge of his friend Painleve:

> There remains one trait of his character that I cannot pass over in silence; that is his admirable intellectual sincerity.[180]

And Painlevé's characterization is not an isolated instance. Again and again, men who knew him as a colleague[181] or as a teacher,[182] or as a distant, yet great man[183] testified to his intellectual integrity, his generosity to others, and his humble lack of concern for matters

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[180] Painleve as quoted in Slossem, Major Profies of Today, p. 139.

[181] Gaston Darboux, "Éloge historique d'Henri Poincare", in Oeuvres de Henri Poincaré (11 Vol.; Paris: Gauthier-Villars, 1934-54) Vol 2, pp. vii-lxx. passim.

[182] Paul Langevin, In Volterra et al, Henri Poincaré, (Paris: Librairie Félix Alcan, 1914), passim.

[183] Tobias Dantzig, Henri Poincaré: Critic of Crisis (New York, Charles Scribner's Sons, 1954), Chapter 1.

of priority. In the absence of any correspondence and private papers,[184] these testimonies must be taken at face value.

But there is an explanation which may at one and the same time account for the behavior of Poincaré while maintaining a position consistent with what is known of his character. As we have developed in Appendix I, Poincaré was seeking a consistent theory which would account for the behavior of matter and radiation. In fact, Poincaré's reviews of his own work and the work of Lorentz suggest that he felt that the goal had been virtually attained. It was, perhaps, a high price to pay: Newton's laws would have to be modified somewhat, but still, Newtonian mechanics would be a valid approximation for most physical phenomena. Then too, one did not have to give up Newton's notions of absolute time and space, for one could take the position that it was only due to a compensation of effects that absolute space and time were not available to our experiments and measuring instruments. But the return that one got for the price was worthwhile: A theory which could subsume electricity and magnetism, optics, mechanics, heat, and sound -- all of experience-- under one set of assumptions and one explanatory mantle. And at the bottom of the whole apparatus were Lorentz's electrons.[185]

With such a theory in hand, why should Poincaré have bothered with Einstein's theory? After all, Einstein's paper only covers a very small

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184 Gerald Holton, Personal communication. Holton reports that he could find no personal papers left by Poincaré when he made a search for such material in Europe several years ago.

185 In one of his last articles, "L'hypothèse des quanta", Revue scientifique 17: 225-32, 1912, Poincaré gives a summary which supports this interpretation of his views.

portion of the phenomena dealt with in the Lorentz theory. It was not immediately obvious to Einstein himself that a theory, which he saw essentially as a theory of measurement, would have all of the physical ramifications that were, bit by bit, turned up. It is likely that Poincaré viewed Einstein's theory as trivial and incomplete---a small part of the larger work already done by Lorentz and himself under a much more reasonable metaphysic.

B. Other Response to Einstein's Theory in France

The total amount of literature on relativity, and in particular, on Einstein's theory in France during the years 1905-1911 is miniscule. Outside of Henri Poincaré, the only other physicist who published in any quantity in related areas was Paul Langevin. In a paper read at the St. Louis international exhibition in 1904, for example, Langevin discussed the problems which had recently been confronting physics in the area of electron dynamics.[186] Langevin's purpose was to show the solid basis, both experimental and theoretical, for the notion of "electron." And though he saw ether as being essentially different than matter it was

> le siège de deux formes distinctes de l'énergie la forme électrique et la forme magnétique...[187]

Prior to the publication of Einstein's 1905 paper, Langevin had made

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186 Langevin, "The Relations of the Physics of Electrons to other Branches of Science", Congress of Arts and Science, Universal Exposition St. Louis, 1904 4: 121-56, 1906. Reprinted in Langevin, La physique depuis vignt ans (Paris, 1923), p. 1. References will be made to the latter source.

187 Ibid., p. 4. "the seat of two distinct forms of energy, the electric form and the magnetic form..."

several contributions to the electron theory.[188] But after 1905, Langevin's publications in the area of relativity and electron dynamics stopped and nothing was heard from him again until 1910. In view of his interest and activity prior to 1906, and subsequent to 1910, this period of inactivity might at first seem strange and perhaps significant, however, a published survey of Langevin's writings in all fields of physics during this period reveals that between 1906 and 1911, Langevin published only two papers in any area of physics.[189] Whatever the reasons,[190] Langevin was "doing something else" during the five years immediately following the publication of Einstein's paper.

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188 Cf. Langevin, "Sur l'origine des radiations et l'inertie électromagnétique" (1905) Oeuvres, pp. 313-28.
Langevin, "Sur l'impossibilité physique de mettre en evidence le mouvement de la translation de la terre", Compte Rendus 140: 1171, 1905.

189 Langevin, Oeuvres, pp. 681 ff. One of these was on the measurement of ions in the atmosphere using an electrometer (1907) and the other related to Brownian movement (1908).

190 Langevin had a reputation as a superb teacher. During the period in question, he was assuming more and more responsibility for courses at the College de France and in addition, in 1905, he assumed the rank of Professor at the Ecole de Physique et Chimie. These responsibilities may in and of themselves account for this absence of publications.

In 1906, Langevin's position seemed quite similar to that of his teacher and friend, Poincaré. Though he had not embraced the principle of relativity with as much confidence as Poincaré had, he was more confident than Poincaré of the electromagnetic nature of the mass of the electron of the material existence of the ether.[191] After 1910, there was a noticeable shift in Langevin's point of view. First, he saw the principle of relativity as an inescapable conclusion, a conclusion forced on us by experiment:

> Il y a tout d' abord à sa base un fait expérimental, établi avec une précision depassant aujourd'hui la milliardienne, et qui trouve son expression dans le principe de relativite.[192]

Langevin continued with the observation that this principle is derived from the negative results of all the experiments designed to show the movement of the earth with respect to some absolute frame of reference. There can be little doubt then that Langevin's view of the principle of relativity was that its validity was based on induction from experience.

Langevin also made explicit the role he saw Einstein playing in the development of electromagnetic theory:

> Si l'on n'a en vue que la partie cinématique [of the Lorentz theory]... M. Einstein a montré le premier qu'on peut l'obtenir en utilisant une seul consequence des équations de la theorie électromagéntique, celle qui

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191 While we have argued that Poincaré had made the ether a necessary part of his schema, he did, formally, retain a conventionalist stance with regard to it. Langevin never did this, and given that he was a Marxist, one would never have expected him to. On his Marxism see, Paul Laberenne (ed), Paul Langevin La penseé et l' action (Paris: Editions Sociales, 1964).

192 P. Langevin, "L'evolution de l'espace et du temps", Revue de metaphysique et de maralales 19: 455-66, 1911.

est relative à l'existence d'une vitesse finie de propagation et qui traduit par la l'essentiel de l'idée primitive de Faraday sur la transmission des actions de proche en proche.[193]

This is just the position we have ascribed to Poincare with regard to Einstein's theory. Langevin modified this position only slightly later. In a series of lectures given in 1919 on the theory of relativity, he treated the invariance of the velocity of light as a consequence of the principle of relativity and at the same time as the conclusion which was forced on one as a result of the Michelson Morley experiment.[194]

On the other hand, Langevin's conception of the role of the ether changed significantly over the course of the years 1911-1919. Thus in 1911 he invoked the Poincare pressure to account for the contraction of electrons; and in 1919, though he no longer referred to Poincare pressure, he did in fact still retain the ether as the preferred, if unknowable, frame of reference in which electrodynamic phenomena proceeded.[195] It played no other role in the analysis.

It should be emphasized that Langevin's 1919 exposition of special relativity is formally impeccable. The picture that one gets, then, of

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193 Ibid., p. 457. "If one considers only the kinematic part (of the Lorentz theory)...Einstein was the first to demonstrate that one can obtain it by using a single consequence of the equations of the electromagnetic theory--that consequence which is relative to the existence of a finite velocity of propagation and which translates through this the essential part of the primitive idea of Faraday on the transmission of actions from place to place."

194 Langevin, Le principe de relativité (Paris: Etienne Chiron, 1922), p. 17.

195 Langevin, "L'intertie de l'énergie et ses conséquences", (1913) Oeuvres, pp. 397-426.

the development of Langevin's understanding of the theory of relativity is a gradual separation of electron dynamics for the philosophical questions of space and time and the problems of measurement. Even so, as late as 1919, he was willing to treat the Lorentz contraction as substantial and real, and the ether was still the backdrop against which the drama of the physical world unfolded.

Another French scientist who responded directly to Einstein's work was E.M. Lémeray. Lémeray had mixed feelings concerning the recent work that had been done on the electrodynamics of moving bodies. He noted that both Einstein and Lorentz had arrived at the same expression for "local" time from completely different points of view, and that they were arrived at in both cases independent of the mechanisms behind the phenomena and independent of the existence of the ether.[196]

Lemeray revealed in his writings an understanding of the logic of Einstein's work.[197] But he was unwilling to accept Einstein's conclusions:

> ...admettons l'existence de l'ether uniquement comme repere.
>
> La transformation entant-etablie, on peut parvernir, par voie deductive, a l'expression des forces absolutes entre corps en mouvement uniforme....[198]

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[196] Lémeray, "Sur la pression d'radiation", Journal de physique theorique et appliquee (1911), p. 564.

[197] Lémeray, "Le principe de relativité et les forces qui s'exercent entre corps en mouvement", Comptes Rendus 152: 1464-68, 1911.

[198] Ibid., pp. 1466-67. "...let's admit the existence of the ether solely as a benchmark.

The transformation being...established, one can arrive, by deductive means, at the expression of the absolute forces between bodies in uniform motion..."

So that in spite of his understanding of the dynamics of Einstein's contribution, and his realization of the distinctions between Lorentz's electron theory and a theory of relativity, Lémeray stubbornly maintained a position which included the ether and absolute space.[199]

Painlevé's position was even more firm. In 1904, while cognizant of the work of Lorentz, Poincaré, and Abraham, he insisted that the recent modifications introduced by those men, which had indeed modified certain principles such as the principle of action-reaction, the composition of forces and the constancy of the mass of an object, had left untouched the postulate of absolute motion, thus tending to confirm its objective value.[200] After the publication of Einstein's work, Painlevé remained unmoved. He was unwilling to accept anything less than "rational mechanics" as the necessary foundation for other sciences, for no other science exhibited such precision.[201]

C. Summary

This was essentially the extent of the French response to relativity between the years 1905-1911.[202] The one man who was most in a position

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199 By 1916, however, Lemeray had become a convert to relativity. See below.

200 Painlevé, "Les axiomes de la méchanique et le principe de causalité", Bulletin de la Societe Francaise de Philosopie 5: 27-72, 1905.

201 Cf. E. Nagel, The Structure of Science (Harcourt, Brace, and World, 1961), p. 154.

202 Not mentioned in the text are one or two experiments done during the period. Cf. Guye and Latnowsky, "Sur la variation de l'énergie de l'électron en fonction de la vitesse dans la rayons cathodiques et sur le principe de relativité", Comptes Rendus 150: 326-29, 1910.
G. Sagnac, "Limite superieure d'un effet tourbillonnaire optique du à un entrainment de l'éther lumineux au vois image de la terre," Le Radium 8: 1-8, 1911.
G. Sagnac, "Les systems optiques in mouvement et la translation de la terre", Comtes Rendus 152: 310-13, 1835-38; 153: 243-45, 1911.

to make some comment chose to ignore the theory, and few others even saw fit to mention Einstein as the creator of such a theory. One is impressed with the sense that the French, like the English,[203] almost to a man saw the principle of relativity as a simple induction from experience. One might have expected such a response in England, but the French tradition for axiomatic and rational physics was much more immediate and much more intense.[204]

A possible explanation presents itself immediately. In the eighteenth and nineteenth centuries the great French analysts, Laplace, Lagrange, Cauchy, Legendre, etc. had seen as their task the perfection and reformulation of Newtonian mechanics. The axioms or postulates that one started with were a matter of personal preference, now emphasizing one thing, energy for example, now another, momentum. As long as the results were in accord with fundamental Newtonian results or extended them in a natural way, it really did not matter what the axioms were. One always had the intuitive base of Newtonian mechanics as a check. But in this situation, with the extension of the principle of relativity and the attempt to subsume mechanics as an approximation within a granular theory of electromagnetics, Newtonian intuitions were no longer useful. Almost a century of experience intervened between the first edition of Newton's Principia and Laplace's Mechanique Celeste. Perhaps a requisite perculation time would be required for relativistic physics

203 See section V below.

204 Merz, loc.cit. , Chapter 1, passim.

to become "common sense" enough for the French to once again treat the problem as physique rationelle.

But the ignoral of Einstein is much more difficult to explain. Perhaps, like a bad dream, if the French waited long enough, he and his theory would go away. The influence of Poincaré in all of this cannot be ignored. The leading theoretician in all of France, probably the most eminent member of the Academie des Science, the expert in electrodynamics, Poincaré had opted for the Lorentz theory of electrons and never changed his mind. Could those who looked up to him do any less?--or more? In trying to understand this mystery, it proves useful to look at later French accounts of the development of electrodynamics during this period.

In 1906 Lucian Poincaré, a cousin of Henri, published a book entitled La physique modern son évolution.[205] The book went through a second edition in 1920, the only changes being in the pagination. A considerable portion of the book is devoted to the ether and the relation between ether and material. To L. Poincaré, whether or not the ether really existed was not a very valuable question--especially to the practicing physicist. For one did not need to know the answer to that question in order to make use of the ether. The idealization of the ether, with all of its fantastic properties, was a simple device which allowed the scientist to determine the form of the equations. That, and none of the

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[205] L. Poincaré, La physique moderne son évolution (Paris: Ernest Flammarion, 1906). At the time of the publication of this book, L. Poincaré was director of the French ministry of public education. A second edition of the book was published in 1920.

other metaphysical questions was essential.[206]

Even though the second edition of the work was not published until 1920, one finds L. Poincaré saying there that the experiments of Kaufmann had confirmed the predictions of the theory of Max Abraham. And the electron, according to L. Poincaré, was simply a determined volume, a point in the ether possessing special properties.[207] The significance of comments like these are not so much that they were made and essentially repeated 14 years later, but that they were made by the director of the ministry of public education and that in the second edition (though not in the first) they were "crowned" (couronne) by the Academie des Sciences.[208]

In many ways this work by L. Poincaré typifies the contributions of many French writers who chronicled the events in this field between 1900 and 1910. However, in all fairness, it must be pointed out that whereas many of the works were frankly hostile to Einstein, L. Poincaré's had the feature of <u>prima facia</u> neutrality. There was no mention of Einstein.

This was not the case with A. and R. Sartory.[209] The authors, intent on canvassing scientific opinion in France, asked, "What is the attitude of the world of scholarship before the edifice of relativity?" While

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206 <u>Ibid.</u>, p. 195. Specific references are to the second edition.

207 <u>Ibid.</u>, p. 296.

208 <u>Ibid.</u>, Title page.

209 A. Sartory and R. Sartory, <u>Vers le monde d'Einstein</u> (Strasbourg: Encyclopedie Illustreé Actualités Scientifiques, n.d.). This book was most probably published between 1920 and 1925.

admitting that opinion was divided. the Sartorys cited only one proponent, Langevin, who, they said, had made himself the apostle of the theory of relativity in France.[210] In opposition to the theory, they presented a host of statements from eminent men of French science including the mathematicians Painlevé and Picard, the physicists M. Brillouin, and E. Guillaume, and even the opinion of the German physicist Max Abraham.[211]

Another class of comments saw Einstein's work as a derivative from that of Lorentz and H. Poincaré. In his introduction to Poincaré's La dynamique de l'electron (1913) Pomey, Ingenier en chef des Postes et des Telegraphes, introduced the theory of the principle of relativity.[212] According to Pomey, Einstein axiomatized the work of Lorentz and Poincaré. Guillaume's introduction to the 1924 edition of Poincaré's La méchanique nouvelle was even more emphatic. Guillaume argued that the second postulate of relativity, the constancy of the velocity of light, was merely a generalization of the concept of "local time" created by Lorentz and Poincaré[213] and that the work of Poincaré in the "new mechanics" was the direct precursor of the ideas developed in 1908 by Minkowski.[214] Relativistic velocity addition was, according to Guillaume, a discovery which

210 Ibid., pp. 153-54.

211 Ibid.

212 H. Poincare, La dynamique de l'electron (1913), p. 14.

213 Poincare, La mechanique nouvelle (Paris: Gauthier-Villars et Cie, 1924), p. vi.

214 Ibid., p. vii.

Poincaré shared with Einstein.[215] There was no question to Charles Nordmann; the credit for relativity which Einstein had received should rightfully have gone to Poincaré.[216]

The theory of relativity did receive some unqualified support in France from others besides Langevin. Lémeray, whom we cited earlier as being unwilling to relinquish the concepts of absolute space and the ether, became a firm supporter of relativity by 1916. In that year, he wrote a short textbook on the theory.[217] And in 1922 he published another work on the subject entitled <u>L'éther actuel</u>.[218] The work contains a remarkable preface by LeCornu, an opponent of the theory of relativity. LeCornu expressed an unwillingness to accept the conclusions of the author that the ether did not exist, at least as a material medium, because it had proven to be useless and obstructive. LeCornu maintained that the ether had a reality which was manifested by the transmission of luminous phenomena; and indeed, the only real question was to determine the properties that it must possess to account for all of the facts. In effect, LeCornu argued, Lémeray was retreating from the forefront by denying the existence of a material-like ether on the grounds that it presented insoluable problems.[219]

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[215] <u>Ibid</u>.

[216] C. Nordmann, <u>Einstein et l'univers</u> (Paris, 1921), pp. 17-18.

[217] E. Lémeray, <u>Le principe de relativité</u> (Paris: Gautheir-Villars et Cie, 1916).

[218] E. Lémeray, <u>L'éther actuel</u> (Paris, 1922).

[219] <u>Ibid</u>., pp. vi-vii.

While LeCornu accurately reflects the attitude of Lémeray in this work, there is another issue raised by Lémeray and countered by LeCornu which should command our interest: the role of relativity in the education of French physicists. Lémeray lamented that textbooks in physics in France too closely resembled ancient treatises and that those ideas which had recently meant progress were not found there.[220]

The cause Lémeray felt was due to the fact that those responsible for the writing of such books did not make an effort to comprehend the new ideas. There was modification of the curriculum---but with what slowness!!, lamented Lémeray. Lémeray compared the lethergy in curriculum reform with regard to relativity to the difficulties encountered at the beginning of the nineteenth century with the introduction of the theory of combustion of Lavoisier and the relinquishment of the concept of phlogiston.[221]

In rebuttal, LeCornu urged that Newtonian mechanics continue to form the basis of scientific education in France. Noting that in conversations with Lémeray, he, Lémeray, had indicated that he intended his remarks to apply to advanced degrees, LeCornu was willing to endorse the idea. However, he urged that before complete adoption of such a curriculum be considered, time be allowed to ascertain whether or not the ideas associated with relativity would receive unqualified and universal

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220 Ibid., pp. 124-27.

221 Ibid.

endorsement beyond the shadow of a doubt.[222]

There is some evidence that the situation had changed little by 1955. According to Arzelies, writing in 1955:

> ...en France tout au moins, il éxiste peu d'exposés d'ensemble et la plupart s'adressent à un public restreint (niveau élementaire ou au contraire mathématiquement trop élévé).
>
> Par ailleurs, à des rares et très louables exceptions pres, les enseignements de base professés dans nos Faclutés ignorent jusqu'à l'éxistence des theories relativistes.
>
> Le certifcat de mécanique rationnelle, par exemple, ne contient, en general, pas une seul lecon sur la méchanique des grandes vitesses (et, bien sur, encore moins sur la méchanique quantique). Tout se passe comme si, au début de ce siècle, un mauvais génie avait pétrifié la méchanique francaise (celle qu'om enseigne) en statue de sel mathematique...[223]

In fact, even today, as was the case at the beginning of the century, the content of the curriculum at the universities in France is codified at the national level and the program is laid out with great specificity.[224] This kind of organization of the educational

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222 Ibid., p. 8.

223 H. Arzelies, La cinématique relativiste (Paris: Gauthier-Villars, 1955) p. vii.
"In France, at least, there exist few general expositions and most of these address themselves to a limited audience (elementary level or, at the opposite extreme, too mathematically sophisticated).
Elsewhere, save the rare and laudable exceptions, the basic instruction as taught in our Facultés ignores the relativistic theories to their very existence.
The Certificate in Rational Mechanics, for example, contains in general, not a single lesson on the mechanics of large velocities (and, of course, even less on quantum mechanics). Everything happens as if, at the beginning of this century, a wicked genie had petrified French mechanics (as it is taught) into a statue of mathematical salt..."

224 Unesco, A Survey of the Teaching of Physics at Universities (The Netherlands, 1966), p. 62 and Appendix III, document C4. It should be pointed out that special relativity is now a part of the regular curriculum.

system may in and of itself, explain the slowness with which the French responded to relativity. Given that those in control at the Academie des Science and those responsible for the details of the advanced curriculum in physics were disposed to look on the theory with disfavor, a new synthesis like special relativity would be kept out of the curriculum for a longer period than one would expect if individual teachers were responsible for the syllabus. To what extent and by whom such control was exerted has not been ascertained, but it is known that H. Poincaré was very active on educational councils and committees. He may well have been the "wicked genie" that petrified French physics in a statue of mathematical salt.

V The British Response to Einstein's Theory of Relativity

If there is a word to characterize English physics in the nineteenth century, it is the world, "ether." Beginning with Young's first paper on light as a wave phenomenon in 1800,[226] the English concern with the existence and nature of the ether grew steadily[227] until, at the end of the century, there were those who believe that the time was close at hand when the true structure of the etherial medium would be revealed to man.[228]

In the words of Eddington,

> The nineteenth century is littered with the debris of abortive aethers--elastic solids, jellies, froths, vortex networks.......229

Ether mechanics as we will term the investigation of the properties of the ether, was not a study restricted to England. However as will be shown in what follows, its effect on English science in the early twentieth century was far more profound than in any other country that we are dealing with in this study. In a sense it is misleading

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226. Thomas Young, "Bakerian Lecture of 1801", Phil Trans 92: 12-48, 387-98, 1802. Of course one can trace the concept of an ether in England at least as far back as Newton. Young himself, though challenging the authority of Newton looked to Newton for support for the wave theory- in particular to Newton's use of an ether.

227. Cf. Whittaker, Op. cit., passim.

Metz, op cit., Vol. 1, chap. 1; Vol 2, chap 6.

228. The most confident expression on the solution to the problem of the ether were those of Oliver Lodge of whom more will be said later.

229. A.S. Eddington, "Larmor, Sir Joseph" Obituary Notices of Fellows of the Royal Society 4: (#11), 1942

to speak of a reaction to the theory of relativity in England. More accurately the English were reacting to what they perceived to be an attack on the ether; for in fact many English scientists were quite ignorant of the details of Einstein's theory of relativity.

As late as 1923 N. R. Campbell could write:

> . . .It remains an indubitable fact that, in spite of the attempts to enlighten him, an average physicist- the man in the laboratory as I have ventured to call him- is still ignorant of Einstein's work and not very much interested in it. Physicists of great ability, who would be ashamed to admit that any other branch of physics is beyond their powers, will confess cheerfully to a complete inability to understand relativity 230

A Pervasiveness of the Concept of the Ether in England

At the BAAS meeting in 1907, Oliver Lodge read a paper on the motion of the ether. The contents of that paper have been described as follows:

> His conclusion is that every cubic millimeter of the universal ether of space must possess the equivalent of a thousand tons and every part must be squirming internally with the velocity of light....231

The Englishman concern with ether mechanics is no better illustrated than in the work of Lodge. The ether seems to have been more than a mathematical fiction to him. It is, in fact, not hard to find the sources of such a point of view in Lodge's professional work. His major research interest were, like those of Max Abraham, in the propagation of electromagnetic waves.[232]

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230. N.R. Cambell, Relativity (Cambridge: Cambridge University Press 1923), p.v.

231. Anon. "Mathematics and Physics at the British Association, 1907" Nature 76: 457-62, 1907, p. 457.

232. Allan Ferguson, "Oliver J. Lodge", DNB 1941-50.

It is not difficult to see how Lodge might want to cling with tenacity to "The Ether of Space" as he was fond of calling it,[233] the medium in which electromagnetic radiation was supposed to be propagated.

Late in his life, in the preface to a book in which he summed up his philosophical outlook he wrote:

> The Ether of Space has been my life study, and I have constantly urged its claims to attention. I have lived through the time of Lord Kelvin with his mechanical models of an ether down to the day when the universe by some physicists seems resolved in mathematics and the idea of an ether is by them considered superfluous if not contemptable. I always meant some day to write a scientific treåstise about the Ether of Space; but when in my old age I came to write this book, I found that the Ether pervaded all my ideas, both of this world and the next....234

But even earlier, his ideas on the ether had been well formed and had played an integral part in his work. Modern Views of Electricity, a widely use elementary treatis by Lodge, was first published in 1889. Two subsequent editions, published in 1892 and 1907 reveal that Lodge's conception of the problem of electricity were not subject to change, even though the last edition was published two years after Einstein's theory of relativity was made public.

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233. O. Lodge, The Ether of Space (New York: Harper and Brothers, 1909)

234. O. Lodge, My Philosophy: Reporting My Views on the Many Functions of the Aether of Space (London: Ernest Benn Ltd., 1933), Preface

The doctrine expounded in this book is the etherial theory of electricity. Crudely, one may say that as heat is a form of energy, or mode of motion, so electricity is a form of ether or mode of etherial manifestation.

. . . .

. . . .The existence of an ether can legitimately be denied in the same terms as the esistence of matter can be denied, but only so.
. . . .the evidence for ether is as strong and direct as the evidence for air. The eye may indeed be called an etherial sense-organ in the same sense as the ear can be called an aerial one 235

If metaphysics was to have any weight or validity for Lodge it had to provide an unconscious appeal to common sense.[236] For example, Lodge argued that the ether was a metaphysical necessity since action at a distance was "unthinkable" for gravitional, electric or magnetic interactions. As late as 1907 Lodge could say that the ether was accepted as a necessity by all modern physicists:

One continuous substance filling all space; which can vibrate as light; which can be sheared into positive and negative electricity which in whirls constitutes matter; and which transmits by continuity and not by impact, every action and reaction of which matter is capable. This is the modern view of the aether and its functions....237

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235. Lodge, Modern Views of Electricity(1st ed; London, Macmillan and Co., 1889) pp. vii, vii, These statements also appear in all later editions of the book.

236. Ibid., pp. 327-28.

237. Ibid., p. 358

In the 1907 edition of Modern Views of Electricity, Lodge added a chapter which reaffirmed his faith in an ether. It must be he said, continuous, frictionless, unresisting to motion while at the same time the ether had to be rigid, that is, resistant to shear, and it had to exhibit perfect elasticity, including gyrostatic elasticity of the kind described earlier by Lord Kelvin.[238] But Lodge felt that he was in a position now to go much further in specifying the physical features of the ether. He came to the conclusion for example, that it had to have a density of 10^{12}grams/cc. Lodge arrived at this result he said, following Kelvin, Heaviside, FitzGerald and Larmor in assuming that a magnetic field was a circulation of fluid ether. The direction of flow of the fluid was along the lines of magnetic induction. Lodge assumed that the kinetic energy of the flowing ether was equal to the energy in the magnetic field.[239]

But Lodge was not alone in these speculations on the nature of the ether. C.V. Burton held that the motion of absolute space was aphenomenal:

> There are some processes whose type is such that an observer with his surroundings may be the seat of them without any resulting phenomena being manifested to him; that is to say, the processes in question are without influence on the sense of the observer or upon any instrumental test or measurement which he can make. ...Thus absolute velocity in space, if admisable at all as a physical conception appears to be rigourously aphenomenal.[240]

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238. Whittaker, loc. cit., chap. 9.

239. O. Lodge, "Modern Views of the Aether", Nature 75:519-22, 1907 This article was included in the 1907 edition of Modern Views of Electricity.

240. C.V. Burton, "Notes on Aether and Electrons", Phil Mag 13: 693-708, 1907.

Burton objected to identification of the flow of ether with the direction of the magnetic field on the grounds that one should then be able to detect the effects of the flow in magnetization processes. He also resisted the identification of ether flow with the Poynting vector, since, he, said, for an object like the sun, one should expect a flow of ether outward in all directions for immense periods of time.[241]

Ebenezer Cunningham disagreed that there would be any problem with identification of the ether with either the magnetic field or the Poynting vector since a constant uniform drift in the aether as a whole would not be detectable. The both Burton and Cunningham agreed that the ether could not be detected. The ether, Cunningham state, "is in fact not a medium with an objective reality, but a mental image which is only unique under certain conditions.[242]

Cunningham's teacher, Sir Joseph Larmor also rejected a substantial view of the ether, though Larmor himself could not concur with any theory which gave a uniform velocity to the bulk of the ether. Larmor asserted that "there is no question of ascribing a uniform motion to the whole of the ether because there is no conceivable means of producing or altering such a motion.[243] Larmor could not understand the objection to identifying ether flow with the direction of the magnetic field and he concluded that

> an infintely extended aether postulates absolute motion as a fact, in the only real sense of that term, namely motion relative to the remote quiescent regions of the aether; since

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241. Burton, "The Structure of the Aether,Nature76:150-51, 1907

242. E. Cunningham,. The Principle of Relativity (Cambridge University Press, 1914), Chapter 15.

243. J. Larmor, "The Aether and Absolute Motion," Nature 76:269-70,1907

that determination is made, arguments from relativity of motion must lapse.[244]

Larmor, whose influence on English physics was great[245] expressed his views on this subject most forcefully in his book, Aether and Matter

> The basis of the present scientific procedure...rests on the view, derivable as a consequence of general philosophical ideas that the master key to a complete unraveling of the general dynamical and physical relations of matter lies in the fact that it is constituted as a discrete molecular aggregate existing in the aether. At the same time, all that is known (or perhaps need be known) of the aether itself may be formulated as a scheme of differential equations deriving the properties of a continum in space, which it would be gratuitious to further explain by a complication of structure....
>
> ...for the...analytical development, net of the aether scheme ...a concrete physical representation of the constitution of the aether is not required: The abstract relations and conditions...form a sufficient basis....[246]

Larmor's conservatism in scientific matters has already been referred to. This conservatism was similar to that of Lorentz and Planck. Like Planck, Larmor was convinced that least action was the supreme formulation of physical law. In fact, in 1924 Larmor claimed that he began to understand relativistic formulations when he realized

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244. Ibid.

245. Sir Joseph Larmor(1871-1942) attended St. John's College, Cambridge. He was first wrangler in 1880(J.J. Thompson being second wrangler in the same year). Larmor succeeded G.G. Stokes as Lucasian Professor of Natural Philosophy, a chair once held by Isaac Newton. "Larmor was decidedly conservative in his scientific views. It seems strange to say this of the man who must be counted the harbinger in England of the new ideas which mark the present century."(Eddington, Loc. cit. Cf. "Sir Joseph Larmor" DNB 1941-50.

246. Larmor, Aether and Matter: A Development of the Dynamical Relations of the Aether to Material Systems on the Basis of the Atomic Constitution of Matter(Cambridge: Cambridge University Press, 1900),pp. 78,164 This book which was based on several earlier papers won the Adams Prize in 1900.

that "its all least action."[247] On the other hand, like Lorentz, he maintained for the entirety of his life that the ether was the equations which defined it. He seemed, Eddington has said,

> A man whose heart was in the nineteenth century with the names of Faraday, Maxwell, Kelvin, Hamilton, Stokes ever on his lips--as though he mentally consulted their judgement on all modern problems that arose.[248]

Larmor, Lodge, Burton and Cunningham were not the only English physicists interested in preserving the ether. J.J. Thomson, like Lodge was an experimentalist and his views on the ether were similar to those of Lodge. He expressed these views most clearly in the Adamson lecture at the University of Manchester in 1907[249]. His concern was with the degree of generality of Newton's third law. To Thomson, the third law was

> one of the foundations of Mechanics .....A system in which this principle did not hold would be one whose behavior could not imitated by any mechanical model. The study of electricity ... makes us acquainted with cases where action is apparently not equal to reaction...This would mean giving up the hope of regarding electrical phenomena as arising from the properties of matter in motion. Fortunately, however, it is not necessary...we may suppose another system which though invisible possesses mass...and is able therefore to store up momentum....[250]

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247. Eddington, loc. cit.

248. Ibid.

249. J.J. Thomson, On the Light Thrown by Recent Investigations on Electricity and on the Relation between Matter and Ether: The Adamson Lecture Delivered at the University on Nov. 4, 1907. (Manchester University Press, 1908.)

250. Ibid. pp. 7-8

The invisible universe which Thomson made reference to is clearly the ether. Thomson's presidential address to the BAAS at Winnipeg in 1909 also contained references to the invisible universe:

> The aether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe.
>
> ....
>
> The aether must be the seat of electrical and magnetic forces.
>
> ....
>
> We may regard the aether as a bank in which we may deposit energy and withdraw it at our convenience...the fluctuations in mass [of the aether] are, however, so small that they cannot be detected by any means at present at our disposal.[251]

In England through 1909 one can find almost no reference to Einstein or his theory. The question in England was not whether or not to give up the ether, but what status to ascribe to it. Larmor, Cunningham and Burton represent examples of physicists who felt that the ether must be defined in such a way as to be undetectable in principle. Of the three, Burton seemed to have been most committed to an ether of real substantiation. Lodge and Thomson are examples of physicists who were profoundly committed to eventual detection of the etherial medium. Even those English physicists who, in one way or another, became aware of the existence of the theory of relativity seemed to have difficulty in understanding even the principles on which the theory was based. For example in 1909, F.C. Searle, the British astronomer wrote to Einstein thanking him for a paper on relativity sent to Searle by Einstein at the request of A.H. Bucherer:

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251. Thomson, "Presidential Address to the British Association at Winnipeg, 1909." Electrician 63: 776-79, 1909. pp. 7-8

> I have not been able so far to gain any really clear idea as to the principles involved or as to their meaning and those to whom I have spoken in England about the subject seem to have the same feeling.[252]

In understanding this lack of response to Einstein's theory one must turn to the characteristics of English physics in the nineteenth century. Merz for example, has commented on the fact that English physics in the nineteenth century was characterized as being peculiarly interested in the practical over the theoretical, in the model rather than the pure idea.[253]

The differences between English physics and physics on the continent has been pungently described by Pierre Duhem:

> In the treatises on physics published in England, there is always one element which greatly astonishes the French student; that element which invariably accompanies the exposition of a theory is the model.

For example, Duhem continued, faced with a problem of explaining the interaction of two electrical charges, the German or French physicist

> will be an act of thought postulate in the space outside these bodies that abstraction called a material point and associate with it that other abstraction called electric charge. He then tries to calculate a third abstraction: the force to which the material point is subjected...
> The French or German physicist conceives in the space... abstract lines of force having no thickness or real existence; the English physicists materializes these lines and thickens them to the dimensions of a tube which he will fill with vulcanized rubber...Here is a book [Lodge's Modern Views of Electricity] intended to expound the modern theories of electricity...In it there is nothing but strings which move around pulleys, which roll around drums, which go through pearl beads, which carry weights; and tubes which pump water while others swell and contract; toothed wheels which are geared to one another and engage

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252. Letter from F.C. Searle to Albert Einstein, May 20, 1909.

253. Merz, loc cit., Chap. 3

> hooks. We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory...254

To Duhem the English physicist never felt constrained by logical consistency; his only aim was to create a visible and palpable model of abstract laws. He used no methaphysics.

Much of the material we have thus far presented supports Duhem's contention about the English penchant for model building. We must however, disagree with him on two counts. First we would argue that English physicists did for the most part operate under a metaphysics and second, with some exceptions, the English physicist was consistent in his use of models.

Model building itself can be construed as representing a metaphysics about the way the English physicist saw the relationship between the world around him and his physics. The metaphysics of Oliver Lodge is quite plain. He could not imagine a world which allowed for action at a distance. While J.J. Thomson is paying homage to Newton, underlying this verneration is a desire to preserve the conservation of momentum in all physical interaction. This is certainly a metaphysics. And while it is true that there was inconsistency in the models that some English physicists built for different problems,[255] this was certainly not true for all English physicists.

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254. P. Duhem, The Aim and Structure of Physical Theory(New York: Anthenum, 1962) pp. 69-72

255. The chief offender, and Duhem's prime example was Lord Kelvin. Cf. Whittaker, loc. cit. Vol. i. chaps, 7-10 for a discussion of the variety of models produced by Kelvin.

The case of J.J. Thomson and the "vortex-atom" is a counter-example to Duhem's claim, Thomson begain in 1882 with his Adam's prize essay by postulating that matter-Atoms-were aetherial vortices.[256] In 1907 he still maintained that the vortex theory as "fundamental".[257] The vortex atom as a devise for transforming the ether of space into the atom of space was still very much a part of Thomson's thoughts in 1936.[258]

Men like Lodge and Thomson eventually accepted the results of the theory of relativity; but they did not accept its spirit. For example, eventually, Lodge expressed his admiration for the theory of relativity in these terms:

> For myself, though I am lost in admiration at the brilliant achievements of this modern school, I cannot think that their philosophical outlook will be found ultimately satisfying.
>
> ....
>
> The [first] postulate [of relativity] says that a certain experiment is impossible. There are many cases where people have said that an experiment was impossible, and helt to it until the experiment was actually performed. I for one am, and then must be many who still are, hopeful that absolute motion will one day be determined.259

And according to Thomson,

> ...It is reasonable to regard Maxwell's equation as the fundamental principle rather than that of relativity, and also to regard the ether as the seat of mass, momentum, and

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256. Thomson, <u>Recollections and Reflections</u> (London: Belland Sons Ltd, 1936) pp. 94-95

257. Thomson, <u>The Corpuscular Theory of Matter</u> (London: Archibard Constable and Co. Ltd., 1907) pp. 1-2.

258. Thomson, <u>Recollections and Reflections</u>. 432-43.

259. Lodge, <u>My Philosophy</u>, pp. 109-11, 115.

> energy of matter, i.e. of protons and electrons: lines of force being the bonds which bind ether to matter. In Einstein's theory there is no mention of ether, but a great deal about space: now space f it is to be of any use in physics must have much the same properties as we ascribe to the ether; for example as Descartes pointed out long ago, space cannot be a void.[260]

With the failure of attempts by Lorentz, Larmor and others to provide a satisfactory mechanical explanation of electromagnetic phenomena, English physicists welcomed the further attempts of theoretical physicists to provide an electromagnetic basis to all physics. For example Lorentz' attempts at first and second order theories of moving bodies were widely intepreted, with justification, as assuming an electrical view of matter and to most English physicists, Lorentz had been successful. To them Lorentz's theory was not ad-hoc. Rather it represented an activity that they were quite familar with: the tailoring of a model to fit the data.

B. The Theory of Relativity in England 1907-1911.

For almost all English physicists, the acceptability of relativity hinged on the ability to fit the theory into the framework of some model of the ether. This included several steps: First to implicitly deny relativity the status of a theory; and second to make it a subordinate principle, modifying the electron theory of matter and third to re-define the ether so as to render it undetectable in principle, but nevertheless, to be described by equations which were in

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260. Thomson, Recollections and Reflections pp. 432-33. It is interesting to note that in invoking the authority of Descartes, English physicists has swung full circle from the time when Newton had pitted himself against Descrates' own conception of vortices. On this point see Merz, loc. cit. p. 61, fn.

concert with (the theory of) relativity. We have already seen how Cunningham and Larmor had begun to redefine the ether. We now turn to the other aspects of this transformation process.

Oliver Lodge observed, in 1907, that new theories were supplementary to the electron theory rather than revolutionary. He urged that we "remain with or go back" to Newton.[261] This suggestion provided the tepping stone for the introduction of Relativity into England. The first serious consideration given to the theory of relativity in England was by Cunningham who challenged the claims of Bucherer. We have already delt with that issue in section III. His eclectic attitude toward the theory of relativity can be illustrated by two papers published in 1909. One of them, represented an effort in pure ether mechanics.[262] The task that Cunningham set himself was to bridge the gap between Maxwell's ether-stress and Newton's third law. In the second paper[263] Cunningham came back to the problem of relativity. He expressed the view that experiment had force the principle of relativity upon us and that the motion of the ether had been fully accounted for by Lorentz and Einstein if we allowed the hypothesis of the eletromagnetic basis of matter.

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261. O. Lodge, Continuity: The Presidential Address to the British Association at Birmingham, 1913.(London: J.M. Dant and Sons Ltd., nd.) pp. 13-15

262. Cunningham, "The Motional Effects of the Maxwell Aether-Stress, "London Proc. Roy. Soc. 83: 109-19, 1909.

263. Cunningham, "The Principle of Relativity in Electrodynamics and an Extension Thereof" London Proc. Math. Soc. 8: 77-98, 1909.

In 1911 Cunningham was asked by the Cambridge University Press to write a book on the theory of relativity.[264] The book was published in 1914 and in it Cunningham gave his view of the place of the theory of relativity in then current physics.

> The controversial note which has been characteristic of discussion in respect of the Principle of Relativity has prevented the significance of the principle from being seen in its proper porportions and in relation to general physical theory. On the one hand, there have been those who have magnified its importance, and assigned to it and unduly revolutionary power, while on the other hand, there are those who have scoffed at it as fantastic and reared on the most slender physical bases....[However], there is a real place for it as a hypothesis supplementary to and independent of electrical theory owing to the limitations to which that theory is subject....If we speak of a "fixed aether" as the background of electrical activity it is the hypothesis that the <u>velocity of any piece of matter relative to the aether is unknowable</u>...
>
> ....It is an <u>empirical</u> principle, suggested by an observed group of the facts, namely the failure of experimental devices for determining the velocity of the earth relative to the luminferous aether, and would make it a <u>criterion</u> of theories of matter that they should give an account of this failure, and it suggests modifications where the theory is insufficient to do so. But like all physical principles it is to be probed by further experience.[265]

In one fell swoop, Cunningham had reduced the theory of relativity to the first postulate, "the principle of relativity". He had made it supplementary to the electron theory of matter and he had reasserted his position on the undetectibility of the ether. In fact he went so far as to say that the principle of relativity required there to be an infinitude of ethers: one for each inertial frame of reference. There was no doubt

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264. Cunningham, <u>The Principle of Relativity</u>(Cambridge: Cambridge University Press, 1914). The fact that Cunningham was asked to write the book by the publisher was communicated to me by Cunningham himself.

265. <u>Ibid</u>. p. v, 7-8.

in Cunningham's mind that the search for what he called an "objective aether" had failed. But he did not want to abandon the ether as a concept. He recognized that any such ether would have to be "subject to the Einstein transformation of space and time when the frame of reference is altered.[266] But while Cunningham was cautious about the ether, he devoted a whole chapter of the book to "Relativity and an Objective Aether" in which he proposed his revised conception of the ether in which the ether was a-priori undetectable in principle.[267]

Despite the efforts of Cunningham and some other to make a reconcilliation between the theory of relativity and the ether, most English physicists between the years 1907 and 1911 simply ignored relativity and the work of Einstein. In the entire period only one voice was raised to question the validity of the ether. That was the voice of N.R. Campbell who attacked the problem in a direct and challenging manner in a series of three papers beginning in 1910.[268]

The concept of the ether, he said, seem unsatisfactory in modern physics.[269] Campbell noted that one of the founders of the atomic theory of radiation, J.J. Thomson had devoted his entire Presidential

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266. Ibid., p. 52

267. Ibid., chap. 15.

268. N.R. Campbell, "The Aether," Phil Mag 19: 181-91, 1910
N.R. Cambell, "The Common Sense of Relativity," Phil Mag 21: 502-17, 1911.
N.R. Campbell, "Relativity and the Conservation of Mass," Phil Mag 21: 626-30, 1911

269. Campbell, "The Aether," p. 181.

Address to the BAAS to the properties of the ether. Campbell attacked Cunningham's attempt to redefine the ether as undetectable in principle:

> It is probable that the future historian of physics will be astounded that the vast majority of physicists should accept a system of such bewildering complexity and precarious validity rather than abanden ideas which seem to have their sole origin in the use of the word "aether" and reject those to which so many lines of thought point insistently.
> A demonstration. . .that the case for the aether is ludicrously weak, where it was thought to be strongest, that the concept has never been the source of anything but fallacy and confusion of thought, may serve to expidite its relegation to the dust-heap where "phlogiston" and "caloric" are now mouldering.[270]

Campbell's position, that the ether was a meaningless concept, was unique in England at this time. But Campbell was not against the use of physical models. In fact, like his English peers, he felt it absolutely necessary to have a model. For example, speaking to the objection to the theory of relativity that it had no physical meaning and that it "destroyed utterly the old theory of light based on an elastic aether and put nothing in its place,"[271] Campbell maintained that a physical theory of light could be produced which was consistent with the principle of relativity.[272] So while N.R. Campbell, alone, took issue with the ether as a meaningful concept, he maintained a firm conviction with regard to the use of the model in the understanding of physical phenomena.

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270. Ibid., pp. 189-90

271. As is well known, Campbell and Duhem were life-long protagonists on the use of the model. See M. Hesse, Models and Analogies in Science (London: Sheed and Ward, 1963)

272. Campbell, "The Common Sense of Relativity," p. 515. cf. Campbell, Physics, The Elements, repr as, Foundations of Science(New York Dover Publications Inc. 1957) p. 130

C. Conclusions

Taken all for all, there was almost no work in England with regard to the theory of Relativity between the years 1905 and 1911. There was a great deal of work on ether mechanics and attempts to save the concept of an absolute frame of reference. Cunningham has noted that he "was surprized to be asked to speak to the British Association in 1910" on the subject of relativity. His surprize was caused by the fact that he "did not think that there was much interest in the matter at that time."[273] In 1911 the British Association for the Advancement of Science sponsored a panel discussion on the principle of relativity as part of their yearly meeting. The discussants included G.N. Lewis, W.F.G. Swann, and Zeeman, and centered first on the scope of the "principle" and second on the mathematical aspects of the "principle."[274] The audience response to the discussion, however, revealed little in the way of a new attitude:

> Dr. C.V. Burton, after expressing his satisfaction that no one had confessed a disbelief in the aether urged the importance of the search for residual phenomena not falling within the electromagnetic scheme. Conceivably gravitation is such a phenomenon. There is the further question as to whether neighboring electrically neutral masses exert forces upon one another in virute of their motion through the aether.[275]

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273. Personal communication.

274. Anon. "Mathematics and Physics at the British Association, 1911," Nature 87: 498-502, p. 500.

275. Ibid.

Many physicists shared Oliver Lodge's inability to conceive of a theory which did not account for "apparent" action at a distance in terms of pushes and pulls.[276] Still others confessed to simply being unable to understand one aspect or another of the foundations of relativity.[277]

For three hundred years, English physicists had speculated and experimented on the nature and structure of the ether. In the middle of the nineteenth century a new hero, Kelvin, had come to the front ranks to lead the search. But even the great Kelvin had failed. At the very time however, that Kelvin was declaring his failure to understand the nature of the substance underpinning electromagnetic phenomena, J.J. Thomson, Rutherford and others were making assults on the constitution and configuration of the atom. It was to this activity that English physicists turned. If it was impossible to determine the structure of the ether, it was possible to determine the structure of the atoms themselves which were in the view of many Englishmen, the products of the ether. The King, Kelvin was dead; long live the King, Rutherford. In a sense the English turned from concerns of the ether to concerns of the nuclear atom, sidestepping the whole question of the theory of relativity.

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276. Alex McAuley, "Spontaneous Generation of Electrons in an Elastic Solid Aether" Phil Mag 19: 129-52, 1910. p. 135

277. Besides those already cited, see, A.A. Robb, A Theory of Time and Space (Cambridge: Cambridge University Press, 1914) p. 2.

The uniform behavior exhibited by British physicists with regard to the theory of relativity becomes a little more understandable when one looks at the manner in which English theoretical physicists were trained. In fact, one can say that a theoretical physicist in England interested in Electrodynamics was trained to do ether mechanics. It was what he had to learn and it was what he knew best.

Most physicists, experimental and theoretical in England were trained at Cambridge University.[278]The Mathematical Tripos examination at Cambridge was the most competitive arena for mathematicians and mathematical physicists in England until 1910.[279] Consider the following question taken from the 1901 examination:

> Obtain the energy function of an isotropic elastic medium and assuming that waves of dialation are propagated through the medium with an indefinately great velocity and that the difference between different media is one of density only, find the intensities of the reflected and refracted waves when plane waves are incident on a plane interface separating two media.
> Waves of light are incident on a face of a uniaxial crystal cut perpendicularly to its axis, find on MacCullagh's theory the intensities of reflected and refracted waves (1) when they are polarized in the plane of incidence, (2) when they are polarized perpendicularly to it.[280]

Presumably one would have had to have committed MacCullagh's theory to memory. This theory on which students of mathematical physics were being queried in 1901 had been created in 1839 and required an elastic ether exhibiting the property of "rotation elasticity," a property

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278. Merz, loc. cit. Vol 1, chap. 3 passim.

279. S.P. Thompson, Life of Kelvin (2 Vol.; London: Macmillan and Co., 1910) Vol 1, passim. J.J. Thompson, Reflections and Recollections. pp. 35ff.

280. Mathematical Tripos, Part II Thursday May 30, 1901, 9-12, Divisions V, VI

which no elastic solid that anyone knew of had exhibited. As a result there had been considerable skepticism at the time if its introduction.[281] Whittaker, himself trained at Cambridge in the late nineteenth century, displayed enthusiasm for Macullagh's ether as late as 1951:

> ...There can be no doubt that MacCullagh really solved the problem of devising a medium whose vibrations, calculated in accordance with the correct laws of dynamics, should have the same properties as the vibrations of light.
>
> The hesitation which was felt in accepting the rotationally elastic aether arose mainly from the want of any readily conceived example of a body endowed with such a property. This difficulty was removed in 1889 by Sir William Thomson (Lord Kelvin), who designed mechanical models possessed of rotational elasticity.[282]

Macullagh's ether was not the only theory required of the students taking the 1901 Mathematical Tripos, The examination included detailed questions on such things as the vortex theories of Helmholtz and Kelvin and, on the theories of Stokes and Maxwell.

What was it like to take a Tripos examination? J.J. Thomson was given us a full description:

> The examination...when I sat for it in January 1880, was an arduous, anxious and very uncomfortable experience....The exami-

281. Whittaker, loc. cit. Vol 1, p. 142

282. Ibid. pp. 144-145. It is interesting to consider the description given by Whittaker of Kelvin's model: "Suppose for example, that a structure is formed of spheres, each sphere being the centre of the tetrahedron formed by its four nearest neighbors. Let each sphere be joined to these four neighbours by rigid bars, which have spherical caps at their ends so as to slide freely on the spheres. Such a structure would, for small deformations, behave like an incompressible perfect fluid. Now attach to each bar a pair of gyroscopically mounted flywheels, rotating with equal and opposite angular velocities and having their axes in the line of the bar; a bar thus equipped will require a couple to hold it at rest in any position inclined to its original position, and the structure as a whole will possess the kind of quasi-elasticity which was first imagined by MacCullagh. (Ibid. p. 145)

nation was divided into two periods: the first lasted four days....At the end of the fourth day those who had acquitted themselves so as to deserve mathematical honors....could take the second part of the Tripos which lasted five days.

....

A...quality which played a great part was concentration on the question at hand and ability to get quickly into stride for another question as soon as one had finished the old... [we had to] gallop all the way to have a chance of winning.[283]

Of course the exam was taken without the aid of text material.

How did one prepare? One allowed three years and a term for preparation. Thomson studied under Routh, a tutor who saw many men successfully through the Tripos.

Routh's system certainly succeeded in the object for which it was designed, that of training ment to take high places in the tripos; for in the thirty three years from 1855 to 1888 in which it was in force, he had 27 Senior Wranglers and he taught 24 in 24 consecutive years....

....

...Routh...was Senior Wrangler in the year when Clerk Maxwell was second. Perhaps no other man has ever exerted so much influence on the teaching of mathematics; for about half a century the vast majority of professors of mathematics in English, Scotch, Welsh, and Colonial universities..had been pupils of his and to a very large extent adopted his methods..

....

...Routh like Maxwell studied mathematics under Hopkins, the great "coach" at that time who had taught Stokes, and William Thomson [Lord Kelvin] and scored 17 Senor Wranglers before he retired.[284]

There existed a natural filter for processing of mathematicians and mathematical physicists in England. It is understanable then why so many of the people we have considered acted with such uniformity when confronted with the theory of relativity. They had studied under Hopkins or one of his pupils. The had been trained to master the partial differential equations of waves moving through "froths, jellies and vortices."

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283. J.J. Thomson, Reflections and Recollections, pp. 56-58.

284. Ibid, pp. 38-40

VI The American Response to Relativity

There can be no question of the nature of the reception of the theory of relativity in this country. As we will show in the following pages, at first the theory went unnoticed. Not until 1908 does one find reference to the theory.[285] Instead one finds a resistance to any suggestion which would suggest dismissal of a theory based on some kind of ether, or skeptical renunciation of those Europeans who were seen as undermining the foundations of classical theory. Even when relativity was supported it was because the physicists in this country who found it acceptable had convinced themselves that the theory had been proved experimentally. Whereas in Europe, much time was devoted to thrashing out theoretical questions, there was very little concern with such questions in America. Time and time again, the Michelson-Morley experiment was repeated: in cellars an on mountains, the instrument was constructed of iron, or wood, or sandstone; the source of light was varied from sodium to mercury vapor to artificial white light to sunlight; and always the results were the same; not, that there was no ether drift, but rather that such a drift could not be measured.

A. The Search for an Ether Drift.

According to the Lorentz theory one should expect a fringe shift in the Michelson-Morley experiment of about one-third of a fringe. Michelson and Morley had expressed confidence that the apparatus

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285. A.D. Cole, "Physics at the American Association, 1908," Science 29: 467-78, 1909, p. 470

was capable of detecting a shift of a hundreth of a fringe.[286] The apparatus had far more accuracy that had been required. Yet Brace, in a review of such experiments in 1905, concluded that more accuracy was needed before a definitive judgement on ether drift experiments could be made.[287]

Later in the year, Brace followed his own advice. He repeated Fizeau's experiment on the change in rotation of a polarized beam of light which should result from the movement of the earth through the ether. He claimed to have improved the experimental accuracy to a point where he should have been able to detect one thousandth of the expected rotation. There was no effect detectable.

Morley and Miller performed the Michelson-Morley experiment several times in 1904 and 1905 and could state with confidence that the "FitzGerald-Lorentz" contraction was the same for wood as it was for iron and that both materials responded in an identical fashion to sandstone.[288]Since the contraction prediction is independent of the material of which the interferometer is made, it is difficult to know how Morley and Miller had come to do such an experiment. Their paper does not develop such a rationale for doing the experiment. Since theory had been carefully tailored to remove the possibility of such

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286. A.Michelson and E. Morley, "The Relative Motion of the Earth and the Luminiferous Aether" American Journal of Science 34: 333, 1887.

287. D.B. Brace "A Repetition of Fizeau's Experiment on the Change Produced by the Earth's Motion in the Rotation of a Refracted Ray", Phil Mag 10: 591-99, 1905.

288. E. W. Morley and D.C. Miller, "Report on an Experiment to Detect the FitzGerald Lorentz Contraction", Phil Mag 9: 680-85, 1905

an effect this raises the question of just how current American physicists were with regard to work being done on the continent.

There is something desparate in Brace's plea for more accuracy and in Miller and Morley's attempt to find different contractions in different materials. Perhaps in spite of theoretical predictions, the sensible reality of the ether was an overpowering thought to them. Such an attitude would explain why Miller and Morley responded positively to opinion which suggested that the null result of the Michelson-Morley experiment was due to its having been done in the basement of a brick building. Presumably, the ether, capable of steaming through the entire earth, was incapable of penetrating the relatively porous structure of the brick. They removed the apparatus to a high hill in Cleveland, surrounded it with glass in the open and only awaited good weather to carry out the experiment.[289] A year later they reported the results had been null.[290] Reports of such experimental programs all but vanished from the public view for the next six years.

But concern with the properties of the ether persisted. Lodge's ether evoked a great deal of comment and his papers on the subject was reprinted and abstracted in both scientific and popular literature.[291] Carl Barus, Professor of Physics at Brown University, saw for reaching implications to Lodge's model of the ether. Given the difference in

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289. Anon., "Physics Section of the AAAS, 1905", Science 23:415-21,1906

290. D.C. Miller, "Physics Section of the AAAS, 1906" Science25: 521-35, 1906. p. 525

291. Cf. O. Lodge, "Density of the Aether" Sci Amer. Supp. 64: 212-13, 1907.

O. Lodge, "Density of the Aether" Science 26: 482-83, 1907.

density between ether and the earth, one should expect, on hydrodynamic principles, that the earth spin in the ether. This might be a way of accounting for gravitational attraction. The fact that no such rotation in the ether is detected in the Michelson-Morley experiment meant to Barus not that such an effect was non-existent, but that the "electronist gets around this [null result] by the principle of relativity".[292] Presumably Barus was referring to theoretical predictions made on the basis of an electron theory which invoke the principle of relativity.

These kinds of remarks are not examples of aberrant viewpoints in the United States. Although the amount of relevant written work in America is small in comparison to the production of German or even British scientists, almost all of it was concerned with ether mechanics, ether structure and detection of the earth's absolute motion.

The situation in England and the United States in this regard is not dissimilar. The English emphasis however had been on an ideal--an ether which, if necessary, was nothing more than the equations which describe it. The American ether on the other hand, was conceived, far more universally than in England, as something substantial, something which one way or another, practical experience would make manifest.

B. In Defense of Ether

By 1907, papers began to appear in the American literature which defended the idea of an ether and absolute space. The shift in emphasis, though slight, was significant. D. F. Comstock said that he

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292. C. Barus, "Lodge's Aether and Huygens Gravitation" Science 26: 875, 1907.

detected a distrustful attitude among scientists who were not physicists respecting some of the physicists' contentions. This, he felt, was especially true of the ether. They read of the always unsuccessful attempts to measure the "aether wind" and begin:

> to feel that the builders of physical theory are perhaps unreasonably tenacious of an idea which could, perhaps, be dispensed with.[293]

But in Comstock's view, which was to change very soon, the idea could not be dispensed with. The most important reason was the independence of the velocity of light from the velocity of the source of light. This surely had to be true otherwise, the "orbits of binary stars would be distorted "[294] and they are not. Then too, Comstock expected that one could detect effects, for example alteration in the force on two charges, as a result of the absolute motion through space.

In response to Comstock, Heyl felt that there was still a more important reason for believing in an ether; namely, the non-instantaneous transmission of light and energy which "precluded action at a distance. The aether stands or falls with the principle of the conservation of energy."[295]

Comstock had not referred to Einstein's solution to the problem of absolute space or double star paradox. In turn, Heyl was unwilling to consider the electromagnetic field in space as the locus of electromagnetic energy. Something more substantial--something closer to the

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293. D.F. Comstock, "Reasons for Believing in an Aether" Science 25: 432-33, 1907

294. Ibid.

295. P.R. Heyl, "Reasons for Believing in an Aether" Science 25: 870, 1907

material of experience must be involved, namely the ether. There is a very simple reason why Comstock made no reference to Einstein. He didn't know about Einstein's theory in 1907 and he was not to become aware of Einstein's theory of relativity until 1910.[296]

The evidence of the public literature suggests that this may have been true of many of the American scientists who have thus far been cited. In Comstock's words "we didn't know anything about Einstein or Relativity. Nothing ever exploded in 1905."[297] As it turns out, Comstock was in a somewhat different position than his American colleagues. In 1907, Comstock was a young student spending the year on leave from graduate study at MIT, working at the Cavendish laboratory in England. At the end of that year, Comstock published a paper which independently arrived at the same result that Hasenöhrl had obtained in 1904 and 1905 with regard to the relation between energy and mass, $E = 3/4mc^2$. The derivation made use of the Poynting vector, the stress tensor and Maxwell's equations in much the same way that Hasenöhrl had used these concepts. Like Hasenöhrl Comstock identified this mass as the "electromagnetic part of the mass."[298] Comstock expressed some hesitation in concluding that all mass was electromagnetic, though he thought that Kaufmann's experimental work suggested that that might be the case for single electrons. He felt

296. Personal Communication, Dec. 10, 1964

297. Ibid.

298. D.F. Comstock, "The Relation of Mass to Energy" Phil Mag 15: 1-21, 1908

that there would be "psychological resistance" to accepting such a concept. However he felt that there was no choice but to accept

> the simplest theory which adequately represents the phenomena,-- we must decide in favor of the complete electromagnetic explanation, which involves only the aether and its properties.[299]

Other Americans were not using simplicity as a criterion in judging scientific theories. It is not quite clear what criteria they were using, but in another work, I have suggested that American's were completely immersed in the practical applications of research and the use of science in the making of money.[300] Prior to the introduction of relativity, outside of lay analyses of the aether, the activity we have described above comes closest to being a response to the theory of relativity in the United States between 1905 and 1907.

C. The Introduction of the Theory of Relativity

In 1908 G.N. Lewis, then a physical chemist at MIT, published a remarkable paper which, though not concerned directly with the theory of relativity itself, led him directly to the theory.[301] The paper was motivated, he said, by the recent experiments which suggested "a review of Newtonian mechanics", in particular the experiments of Kaufmann, Bucherer, etc., on the specific charge of rapidly moving electrons. Lewis's purpose was to build a mechanics in which energy is conserved,

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299. Ibid.

300. Stanley Goldberg, "Basic Research and Industrial Growth" in press, Cf. I.B. Cohen, Science and Technology in the First Century of the Republic (Columbus: Ohio State University, 1960)

301. G.N. Lewis, "A Revision of the Fundamental Laws of Matter and Energy", Phil Mag 16:705-17, 1908

mass is conserved and momentum is conserved at every instant for every process. He began like Comstock before him with Maxwell's expression for light pressure:

$$f = \frac{1}{c}\, dE/dt$$

where dE/dt is the rate at which the body received energy, f is the force on the body and c is the velocity of light. If the body is acquiring momentum then some other system must be losing momentum, that is if we are going to preserve the conservation of momentum. And according to Poynting, a beam of light not only carries energy, but it carries momentum as well. If m is the momentum, then for the beam of light we should be able to write:

$$dM/dt = f$$

and combining these two equations:

$$dE/dM = c; \; E/M = c$$

Contrary to the "prevailing point of view"[302] Lewis used this derivation to adopt the following standpoint: "In such a beam, <u>something</u> possessing mass moves with the velocity of light and therefore has momentum and energy."[303] This led Lewis directly to the relationship, $E = mc^2$

Lewis's paper is fraught with difficulties. He never justified his treatment of the velocity of light as a constant. He never gave physical interpretations to many of his results. Though Lewis said that he was

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302. <u>Ibid.</u>, p. 707

303. <u>Ibid.</u>

emboldened to publish this paper by the work of Comstock and Einstein;[304] Comstock, a friend of Lewis's, relates that at the time Lewis wrote the paper he, Lewis, was not aware of Einstein's work.[305]

Lewis did attempt to give his work a physical interpretation. He denied, without elaboration that his logical result implied that light is corpuscular. However since any body with finite rest mass will have infinite mass at the speed of light

> ...That which is a beam of light has mass, momentum, and energy and is traveling with the velocity of light would have no energy momentum or mass if it were at rest, or indeed if it were moving with a velocity even by the smallest fraction less than light. 306

Lewis had swept aside many problems in his rush to get general results. However, his goal of conserving mass, energy and momentum, at whatever cost to physical meaning, had been accomplished. In closing, he could not help but make some speculation about the nature of the "something" that moved with the velocity of light. Identifying it with the ether, he cautiously suggested that absolute space might be identifiable.

> If we assume an ether pervading space and assume that this ether possesses no mass, except when it moves with the velocity of light, it is obvious that an ether drift could in no way affect a beam of radiation nor could it be detected by any mechanical means. If we are to assume such an ether we may as well assume it to be at rest. A body is absolutely at rest when any motion imparted to it increases its mass or when a certain force will give it the same acceleration in any direction. 307

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304. Ibid., p. 705

305. Personal communication

306. Lewis, loc. cit., p. 716. Emphasis in original.

307. Ibid. p. 717

Lewis's program then was a method, if only in principle, for measuring absolute motion. Within a year, under the influence of the theory of relativity he was to withdraw that claim:

> ...I should like to modify one of the statements in my previous paper. It was there intimated that the equations of non-Newtonian mechanics offered a means of determing absolute motion through space. In a recent paper by Mr. Tolman and myself, it is shown on the other hand, that these equations maintain their full validity no matter what point is arbitrarily chosen as a point of reference.[308]

The response to Lewis's original paper were quite mixed. On the one hand, several authors felt that paper required elaboration before one could tell what requirements Lewis's work would place on the ether.[309] On the other hand some, like L.T. More, Professor of Physics at the University of Cincinnati saw some very dangerous trends in Lewis's work.[310] More felt that Lewis's effort typified the lack of logic in some work being done in physics; but Lewis's effort could be profitably used as an example because his work was "without the complexity which usually obscures, in such attempts, the real issues." In More's view, Lewis's chief blunder was in ascribing to light, not only momentum and energy, but also mass, traveling with the velocity of light. The blunder, More continued, was further compounded by asserting that since a body which absorbs radiation also absorbs energy, acquiring

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308. G.N. Lewis, "A Revision of the Fundamental Laws of Matter and Energy", Science 30:84-86, 1909

309. F.W. Very, "The Conversation of Mass and the Passing of Matter" Science 30:491-96, 1909.
O.C. Lester, "Recent Advances in the Science of Physics", Proc. Colo. Sci. Soc9: 217-33, 1909

310. L.T. More, "On Theories of Matter and Mass" Phil Mag 18: 17-26, 1909

momentum, it must increase in mass. But, More rejoined, consider a ball hurtling toward a man. He can stop the ball with his hand which evidently absorbs the energy, yet no one would say that the mass of the man's hand had increased.[311] More chastised Lewis for failing to realize that the velocity of light depends on the medium in which the light is propagated. That is, the velocity of light cannot be a constant.[312] Finally, More wondered how, if Lewis were correct, the sun could lose 1.2×10^{17} grams/year without that loss having an effect on "cosmic problems".

As to the special character of the velocity of light, Lewis responded in a way that was to become typical for those Americans who supported the theory of relativity: the constancy of the velocity of light was an empirical fact.[313] For example H.A. Bumstead, Professor of Physics at Yale, remarked that Einstein had considered the Lorentz-FitzGerald contraction from an "interesting and instructive" point of view. In fact, it was Bumstead's belief that the principle of relativity might become one of the fundamental empirical laws of physics "occupying a position analogous to the second law of thermodynamics."[314]

Lewis's proposed revision of Newtonian mechanics was actually the first step toward the introduction of the theory of relativity into the physics literature in this country. In Dec. 1908, he and

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311. Ibid. p. 19.

312. Ibid., p. 20

313. Lewis, "A Revision.......", Science30:

314. H.A. Bumstead, "Applications of the Lorentz-FitzGerald Hypothesis to Dynamical and Gravitational Problems," American Journ.Sci26:493-508,1908

R.C. Tolman read a paper at the American Physical Society on Einstein's theory. The paper later appeared in the Philosophical Magazine[315]. It is interesting to note that neither Lewis nor Tolman were trained as physicists. Lewis was a professor of physical chemistry and Tolman was then a graduate student in chemical engineering at MIT.

In laying the groundwork for their exposition Lewis and Tolman paid particular attention to the second postulate of relativity, emphasing over and over again its radical nature. But to Lewis and Tolman both postulates of relativity had been established on a firm experimental basis.[316] Tolman later reiterated this claim by deriving the second postulate as a consequence of the results of Bucherer's experiments on the inertia of electrons.[317]

It is clear from the exposition of relativity by Lewis and Tolman that they were doing something other than what Einstein had had in mind when he called his theory a theory about rigid rods, clocks, and light signals:

> Let us emphasize once more, that these changes in units of time and length, as well as the changes in the units of mass, force, and energy which we are about to discuss possess in a certain sense a purely fictitious significance ....[318]

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315. G.N. Lewis and R.C. Tolman, "The Principle of Relativity and Non-Newtonian Mechanics" Phil Mag 18: 510-23, 1909.

316. Ibid., pp. 512-13.

317. R.C. Tolman, "The Second Postulate of Relativity" Phys Rev. 31: 26-40, 1910

318. Lewis and Tolman, loc. cit. pp. 516-17.

Though little more support for the theory of relativity was evident in the American literature in the years immediately following the publication of the exposition by Lewis and Tolman, interest was growing. Comstock now held that the burden of proof was on those who object to the theory since the theory was in harmony with so many experimental phenomena.[319] C.M. Sparrow and W.S. Franklin both wrote brief, popular expostions of the theory.[320] And Tolman continued to publish papers in the implications of the theory.

As we have already mentioned, Tolman viewed the second postulate of relativity, the constancy of the velocity of light as having been demonstrated experimentally. Stewart agreed with Tolman that the first postulate had been established by experiment, but as for the second postulate, if it were true, according to Stewart, it was only because one could assume a luminiferous ether. In other words Stewart saw a conflict between the two postulates of relativity: The first postualte excluded the possibility of an ether, the second postualte demanded it; hence there was a conflict. The only way out, Stewart saw, was to assume a ballistic theory--a theory in which the velocity of light depended on the velocity of the source.[321]

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319. Comstock, "Principle of Relativity" Science 31: 762-72, 1910

320. C.M. Sparrow, "On the Fundamental Ideas of the Theory of Relativity" Johns Hopkins University Circulars(NS)29: (#2) 45-53, 1910

321. O.M. Stewart, "The Seoond Postulate of Relativity and the Electromagnetic Emission of Light", Phys Rev 32: 418-28, 1911

L.T. More's reaction to the theory exposited by Lewis and Tolman was that they had confused physics with metaphysics and the only solution was to cleanse physics of the smother influence of metaphysics.[322] And while there was other support for the theory of relativity in America between 1909 and 1911[323] the climate seems to have been summed up by W.F. Magie in his Presidential Address to the Physics secition of the AAAS in 1911:

> I do not believe that there is any man now living who can assert with truth that he can conceive of time which is a function of velocity or is willing to go to the stake for the conviction that his "now" is another man's "future" or still another man's "past."[324]

D. Conclusions

The theory of relativity was hardly "received" in the United States. On the other hand, there are palpable differences between the reception of the theory in America and France and England. Unlike the almost unbroken silence which the theory confronted in France, response, once it came in America, was impassioned, both for and against the theory. But unlike England, the theory was judged on the degree to which it conformed to experience and the degree to which the elements of the theory, including the postulates of the theory, had been tested.

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322. L.T. More, "Recent Theories of Electricity", Phil Mag 21: 196-218, 1911.

323. Cf. H. Crew, "Debt of Physics to Metaphysics" Science 31: 521-32, 1910.
A.G. Webster, "Past and Present Status of the Aether", Pop. Sci. Mon. 77:105-13, 1910

324. W.F. Magie, "The Primary Concepts of Physics", Science 25: 281-292, 1912, pp. 292

Granting that the response in the United States was much more muted than the German response, and granted that the empirical bent which had long been a part of American science was plainly operating, there are some similarities in the American response to relativity to the response in Germany. There was support and that support was serious support. Furthermore, while a common thread of affront to commonsense ran through the oppostion to the theory, that opposition was in its own way as varied as that in Germany. Thus More objected on the grounds that the theory of relativity was metaphysical, Stewart on the grounds that the theory was internally contradictory, and Magie on the grounds that the cost of accepting the theory was too dear a price to pay for a working metaphysics.

When one turns to the structure of the American educational system, one can see the beginnings of the elements which went into making Germany such a power in science. Graduate education in America was less than 20 years old in 1900. But already by the time Einstein published his first paper on relativity, Harvard, Yale, MIT and a few other schools had joined Johns Hopkins in offering advanced degrees in physics. The variety of programs available, while still small was not insignificant especially in comparison to England and France. That variety was to grow into the amazing complex of graduate programs we have today. Is it any wonder that physics in the United States has, since the second world war eclipsed the physics of most other nations in terms of contention, variety, and heuristicity.

VII Conclusions

We began this study with an observation by Merz that a uniform scientific spirit had pervaded science on the continent by the turn of this century. Our purpose has been to show that in fact the evidence with regard to the theory of relativity suggests that the scientific spirit was not as uniform as Merz would have us to believe. Indeed, I would venture to say that even today, ' public view of change in science is colored by the belief that science as a social institution is a uniform, monolithic block. This view holds that change in science is uniform and transcends the pettiness of national boundries. No doubt the language of scientific discourse has become far more universal in the last sixty years. But underneath this uniformity, ethnic and national interpretation persists today just as surely as it persists underneath the common and trival agreement that musical conductors have with regard to the notes in a musical score.

Modes of creation in science, like modes of creation in any human endeavor are subject to the restraints of style. Similarly modes of acceptable behavior in science like modes of acceptable behavior in any human endeavor are subject to the restraints of style. Small wonder then that when revolution occurs in science, like any other revolution, one finds continuity between the old and new--the continuity of style. To the degree that a multitude of styles are prevalent in a society, a multitude of responses can be expected.

It is important, I feel, to take seriously the correlation between the structure of the educational systems in the countries we have studied and the kind of response the theory of relativity received in each of these countries. One might predict for example, that the imposition of a national curriculum in science or any other field might have the effect of damping and muting the kind of contention that seem to be necessary for the eventual acceptance of a radical innovation like relativity. For this contention eventually forced scientists in Germany to elaborate the theory of relativity, if for no other purpose than to show how absurd it would prove to be. And it is in the process of elaboration, we believe, that the value and worth of a new idea is revealed.

APPENDIX I

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HENRI POINCARE AND EINSTEIN'S THEORY OF RELATIVITY

Stanley Goldberg

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