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out, as illustrating what the theory of induction has to show how to deal with, is that this proposition of Newton's serves for him in an extremely straightforward way as a premise to a further inductive conclusion (of undoubted empirical value). In his argument for Proposition IV, Newton can assert that a piece of the moon, if brought to the earth, would experience a force increasing as the inverse square of the distance, for his astronomical evidence supports this. He cannot, at this stage, assert the symmetrical proposition: that a terrestrial body, raised to the height of the moon, would diminish in weight in that same ratio; for he has no evidence at all to support this. Once Proposition IV is established, however, and the acceleration field of Galileo has been identified with the acceleration field of Kepler, this new proposition can be asserted. Of course it has now become possible—three centuries later—to put both propositions to experimental test (in the opposite order): we have lifted terrestrial bodies to the moon, and have brought lunar bodies down to earth; and in doing so have confirmed both propositions directly.

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Outlines of a Logic of Comparative Theory Evaluation with Special Attention to Pre- and Post-Relativistic Electrodynamics

I. Introduction

It would be false to say that case studies drawn from the history of science have had no influence on the philosophy of science in the past ten years. The contributions of the late N. R. Hanson (1958), and S. Toulmin (1961), P. K. Feyerabend (1962, 1965), and T. S. Kuhn (1962) utilize examples drawn from the science of Aristotle, Buridan and Oresme, Galileo, Newton, Lavoisier, Dalton, Maxwell, and Einstein. On the basis of such examples these current authors develop their views of the nature of scientific thought, and though they by no means agree in all particulars with one another, their general “historical” approach has raised some perplexing questions for philosophers of science who have based their views on the more “logical” analyses of, say, Carnap, Hempel, and Nagel.1

By focusing attention on the richness and adaptability of historically discarded scientific theories, and on the many instances of theory competition that exist in the historical record, these “historical” philosophers of science have called into serious question many of the central doctrines of earlier philosophers of science.2 In varying ways they have suggested that:

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1 The distinction between the “logical” and the “historical” approaches to the philosophy of science is made in Shapere (1965). For representative selections of the former approach see Carnap (1956), Hempel (1965), and Nagel (1961).

2 These central doctrines had not of course gone uncriticized before the last decade. In fact some of the work of the earlier critics of the logical empiricist approach, such
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1. Experiments in science are not theory neutral but have interpretations placed on them which are theory dependent. This claim immediately gives rise to the question whether a “crucial experiment” which would choose between conflicting theories is ever possible, as the experiments are, on this view, interpreted in radically different ways within the different competing theories.

2. There is no common “observation language” as many of the logical empiricists believed there was for different theories applying to the same domain of inquiry. Rather, observational terms do not possess a meaning per se, but are only meaningful in connection with a theory. The traditional position has accordingly been neatly inverted: theories are understandable per se, observational terms only admit of a partial interpretation through theories.3

3. Recalcitrant experiments, or experiments which prima facie falsify a theory and resist reinterpretation which might save the theory, do not cause a theory’s rejection (or, it is implied, even its modification—cf. Kuhn, 1962). All theories are considered at all times to be in some difficulty. When conditions are right a theory is rejected in toto and replaced, apparently for irrational reasons, by an alternative theory which is either inconsistent or “incommensurable” with the former. Scientific change is viewed as fundamentally cataclysmic and discontinuous, as opposed to the traditional account of an uneven but cumulative progress of science. Though one of the conditions for change is that an alternative theory be available which in some notoriously obscure sense is “about” or “associated” with the “same” experimental domain as its less fit predecessor, it is clear that this claim is in conflict with the comments on the discontinuity and incommensurability of theories and observations cited above.

In spite of the difficulties of unsatisfactory formulation, however, this claim does contain plausible arguments against any general “falsifiability” approach to science.4

4. The historical school, if I may be permitted to call it a “school,” has also suggested that one has to give the standard term “theory” a new and broader meaning so as to be faithful to the way it functions in the history as Wittgenstein (1953) and Popper (1934, but hereafter cited as 1959), served as the source and stimulants of the historical school’s critiques.

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of science. Feyerabend (1962) and Hanson (1958) have respectively proposed that a theory be understood as “a way of looking at the world” and “what makes it possible to observe phenomena as being of a certain sort . . .” Toulmin (1961) has introduced the phrase “Ideal of Natural Order” to articulate how he understands a scientific theory, and Kuhn (1962) has proposed the term “paradigm” to stand for that complex of theory, methodology, standards, and metaphysic which a scientist works with, implying it is a good deal more than a theory that is tested by an experiment. The logical empiricist analysis of a theory is treated as irrelevant, if not incorrect, for an adequate philosophy of science. Such a view of “theory,” when taken in conjunction with the three earlier theses, indicates the extent to which the reaction of the “historical” school to logical empiricism has led them in the direction of a new “idealism.”

Such theses as the four cited above present a very different picture of science from what most of us hold or once held. The cumulative view of science and science’s “objectivity” seem to disappear. The control over speculation exercised by observation and experiment appears severely weakened, and the rationality of science and the progress of science are denied.5

Though the history of science has, then, exerted a significant influence on the philosophy of science, the claim of a contrary influence is easy to deny. With several exceptions, such as Joseph Clark’s attempt to consider the implications of a logical empiricist’s conception of a scientific theory for the history of science,6 historians of science do not avail themselves of the contributions of the philosophy of science. Some historians do, of course, like L. Pearce Williams,7 read Kant and Schelling—but this is hardly contemporary philosophy of science.

Nevertheless historians do face what can legitimately be called “philosophical” problems. When they are forced to assign a date to the discovery of oxygen, they must involve themselves in conceptual analysis to determine whether “dephlogisticated air” is to count.8 When a historian is asked to determine whether it was Lorentz and Poincaré, or rather Einstein, who first articulated the special theory of relativity, certain questions about the nature of scientific theory and of alternative interpretations of

3. See Scheffler (1967) and Shapere (1964, 1966) for general comments and critiques of these and other implications of the historical school.


6. See Kuhn (1962), pp. 54ff on this problem.
mathematical formalism must be considered. When a historian wishes to give an account of the relevant circumstances involved in the replacement of one scientific theory by another, he must know something about the logic of theory selection as it affects the behavior of the practicing scientist.

This paper is an attempt to construct a logic of comparative theory evaluation which will resolve some of the paradoxes associated with science’s apparent rationality which were raised by the historical school of philosophy of science. It is also hoped that it will assist in solving some of the disputes that have arisen in historical circles over the priority of the discovery of the special theory of relativity. By absorbing certain aspects of the major theses of the historical school as outlined above, and by re-presenting some of the logical empiricists’ conceptual tools in a new form, this paper will attempt to articulate a characterization of science which permits the elaboration of a logic of comparative theory evaluation.

II. An Analysis of Scientific Theory and Experiment

Before I begin an inquiry into the logic of comparative theory evaluation, it will be necessary to digress slightly in order to discuss some basic ideas which will occur in my analysis.

A. Antecedent Theoretical Meaning

The term “scientific theory” will be used in this paper to refer to a set of sentences of universal form. These sentences will contain a class of “primitive” nonlogical terms which are given what I call antecedent theoretical meaning by drawing on antecedently understood domains of discourse.° These domains are not intended to provide “models”—in any of the traditional philosophical senses—for the theory. An entity term, such as “gas molecule,” has its meaning created by sentences providing an appropriate analogical description, usually drawn from several diverse domains including branches of mathematics. Such meaning-creating sentences, which are to be found in the text of scientific articles and monographs, are usually characterized as analytic. The antecedently meaningful terms are then interrelated in sentences which have the character of hypotheses, that is, which are usually considered to be synthetic.²°

° See my (1969b) for more details on this notion of antecedent theoretical meaning.
²° The distinction between analytic and synthetic sentences becomes somewhat relative in this analysis and will depend on the extent to which certain properties are considered definitional. There are also numerous instances in the history of science in which definitions change. See Putnam (1962) for a further discussion of relevant examples and issues.

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James Clerk Maxwell (1867) gave an example of this procedure in connection with the term “gas molecule” when he wrote in one of his fundamental papers on kinetic theory: “In the present paper I propose to consider the molecules of a gas not as elastic spheres of definite radius, but as small bodies or groups of smaller molecules repelling one another with a force whose direction always passes very nearly through the centers of gravity of the molecules, and whose magnitude is represented very nearly by some function of the distance of the centers of gravity.” Maxwell continued, further elaborating his notion of a “molecule,” and also proposing a law or specific hypothesis for the interaction of these entities: “If we suppose the molecules hard elastic bodies, the numbers of collisions of a given kind will be proportional to the velocity, but if we suppose them centers of force, the angle of deflection will be smaller when the velocity is greater; and if the force is inversely as the fifth power of the distance, the number of deflections of a given kind will be independent of the velocity. Hence I have adopted this law in making my calculations.”

We see in the quotations above how the meaning of the term “gas molecule” is carefully created, and how it is possible to postulate modes of behavior of the entity. Such meaning as is conferred on the term, of course, is not forever fixed, and could change if a different theory utilizing, say, a different notion of “gas molecule” were formulated. It should also be mentioned that insofar as the effects of the actions of the gas molecules come to be analytically associated with the notion of “gas molecule,” the adjunction of certain types of correspondence rules to a theory about gas molecules can extend and modify the meaning of the term “gas molecule.” I shall have more comments to make concerning correspondence rules below.

B. Correspondence Rules and Theory Interdependence

In order to provide experimental control and tests, as well as to enable theories to account for observationally accessible states of affairs, a theory must also have associated with it an additional set of sentences, which I shall term C-sentences; these state how the entities and/or processes described by the theory’s axioms ultimately affect our sense organs. Let us call O-sentences those sentences which describe an intersubjectively testable experience, such as the “seeing of alternating light and dark bands or fringes through a telescope” or the “hearing of a tone.” We shall also characterize as O-sentences those sentences which name an entity that is ob-
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C. Theory and Experiment

On the view to be defended here, which is in many respects very much like both Popper's and the "historical" school's position, any experiment takes place within a theoretical context. A theory (or perhaps theories) is applied to a relatively specific situation and yields, in accordance with the antecedently meaningful sentences and associated C-sentences, O-sentences as described in the previous section. This means that it is the theory, by virtue of its antecedent theoretical meaning and its antecedently understandable hypotheses, and the associated theories which indicate which experiments are relevant to its claims about the world. This is the case whether the experiments have been done earlier, even in connection with a discarded theory (unless an important parameter is now claimed to have been overlooked in the earlier version of the experiment), or are yet to be performed.

We can see the features of this analysis exhibited in the Michelson-Morley interferometer experiment. Lorentz's (and also the relativists') analysis of the Michelson-Morley experiment will be discussed later, so suffice it for now to say that if the ether is assumed stationary in the universe, and if the Michelson interferometer is rotated, then there will be a slight displacement of the fringes viewed through the telescope of the interferometer. There were associated well-confirmed optical and cosmological theories which were taken to be beyond question in the context of this experiment: it was not doubted that a wave disturbance of light would travel in a straight line at a velocity approximately equal to \(1.86 \times 10^8\) miles per second and obey the laws of reflection and refraction, nor was the "hypothesis" that the earth was moving about the sun at a velocity of 18 miles per second questioned.

The observation reports or O-sentences which are the output of this experiment are representative of what a careful observer, whether he be a proponent of Lorentz's theory, Stokes's theory, or Einstein's theory, would see through the interferometer's telescope as the interferometer is rotated. (How and why such agreement can occur will be discussed below in the "experimental adequacy" section.) The language of the O-sentences is, it is true, mathematical, for it mentions fringe shifts of so many tenths of a centimeter. Nevertheless the theoretical component of the O-sentences is clearly minimal, as is supported by the willingness of observers of different theoretical commitments to make the same observations. Furthermore, and this is most important, the O-sentences need not agree with the ex-

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11 I have discussed this analysis of correspondence rules more extensively in my (1969b), and have also applied it in the context of the reduction of biology to physics and chemistry in my (1969c).

12 Cf. Popper (1959), especially chapter 5.
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expectations of the theorist, as the results of the Michelson-Morley experiment did not agree with the expectations of Lorentz, nor may it be possible for the theorist to easily and nontrivially modify his theory so as to accommodate the unexpected O-sentence. Accordingly, experiments can exert some control over theory, even though the results of the experiments, the O-sentences, may have theoretical elements infused in them. There are, of course, greater difficulties of control if the theoretician can easily modify his theory to accommodate the O-sentence(s), but this is an issue which will be taken up in the section in which ad hocness and simplicity are analyzed, where it will be argued that such considerations restrict the theoretician from making facile accommodating moves.

This analytical interlude which I have now completed was necessary to set the stage for later accounts of the interaction between theory and experiment that will be developed in the next two sections. It is hoped that the rather schematic aspects of this logical analysis will become more distinct and complete in the context of the specific examples to be discussed later.

III. Hertz on the Foundations of Mechanics and the Logic of Comparative Theory Evaluation

In the long introduction to his posthumously published (1894) monograph, *The Principles of Mechanics*, the distinguished physicist Heinrich Hertz attempted to outline the reasons why he found both the old Newtonian account of mechanics and the newer energeticist formulation of mechanics inadequate. Hertz understood each formulation of mechanics to be an “image,” or *Bild*, constructed from its own basic ideas or primitive terms by combining those ideas or terms into fundamental principles or propositions or axioms. Each set of ideas and principles had to be adequate for deriving the whole of mechanics, but the ideas and principles could vary: some primitive terms in one image being defined terms in others, some principles of one image becoming theorems in a different image.

In particular, Hertz considered three different images of mechanics: (1) the Newtonian-Lagrangian image which Hertz took to be based on the

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13 See my (1969a) and also chapter 6 of my (1970) for a detailed discussion of Lorentz’s difficulties with the results of the Michelson-Morley experiment.

14 This analysis, which permits disagreement between the consequences of a theory and experimental outcomes, partially accords with Feyerabend’s account discussed in his (1965), pp. 214–215, but is, I think, a more intelligible analysis of the process representing the prelude to falsification.

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ideas of space, time, mass, and force, and to employ as principles either Newton’s laws of motion or Lagrange’s generalization of d’Alembert’s principle, (2) the energeticist image, which founded mechanics on the ideas of space, time, mass, and energy, and which Hertz understood to use Hamilton’s integral principle of least action, and (3) Hertz’s own image of mechanics, which had as its basic ideas only space, time, and mass, and which utilized a principle of minimal curvature of path of a mechanical system in motion.

It is not my intention to consider Hertz’s mechanical investigations any further. What I wish to do, rather, is to examine the metascientific ideas which he constructed in order to assess the relative merits of these three competing images.

Hertz introduced three metascientific concepts: *Zulässigkeit* or permis-
sibility, *Richtigkeit* or correctness, and *Zweckmässigkeit* or appropriateness. Hertz believed that only if we could obtain a clear conception of what properties were to be ascribed to the images for the sake of permis-
sibility, correctness, and appropriateness could we “attain the possibility of modifying and improving our images.”

The concept of permisibility reflects a type of Kantian bias on Hertz’s part, for it involves an a priori and immutable assessment of an image or a scientific theory. In connection with permisibility Hertz wrote:

> We should at once denote as inadmissible all images which implicitly contradict the laws of our thought. Hence we postulate all our images shall be logically permissible [i.e., self-consistent].

What enters into the images in order that they be permissible is given by the nature of our mind. To the question whether an image is permissible or not, we can without ambiguity answer yes or no; and our decision will hold good for all time.

Correctness on the other hand is associated with experiments and observations:

> We shall denote as incorrect any permissible image, if their essential relations contradict the relations of external things.

What enters into the images for the sake of correctness is contained in the results of experience, from which the images are built up. Without ambiguity we can decide whether an image is correct or not; but only according to the state of our present experience, and permitting an appeal to later and riper experience.

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15 All quotations from Hertz in these pages are from the Introduction to his (1894) monograph.
Appropriateness is defined in the following terms:

Of two images of the same object that is the more appropriate which pictures more of the essential relations of the object—the one which we may call the more distinct. Of two images of equal distinctness the more appropriate is the one which contains in addition to the essential characteristics, the smaller number of superfluous or empty relations—the simpler of the two.

What is ascribed to the images for the sake of appropriateness is contained in the notations, definitions, abbreviations, and, in short, all that we can arbitrarily add or take away.

It is most difficult to determine the appropriateness of an image, as Hertz noted when he wrote: "We cannot decide without ambiguity whether an image is appropriate or not; as to this differences of opinion may arise."

IV. A Generalization of Hertz’s Categories of Comparative Theory Evaluation

Though Hertz’s three categories in terms of which to compare competing theories are suggestive, and were used by him with excellent results in the domain of mechanics, I feel that they must be somewhat generalized if they are to accommodate the advances made by twentieth-century physicists, historians, and philosophers.

A. Theoretical Context Sufficiency

The notion of “permissibility,” understood in the sense given above in the quotations from Hertz, lacks the flexibility that is needed to describe the scientific revolutions which physics has gone through in this century, as, for example, the revolution in our views of geometry and space associated with Einstein’s general theory of relativity. There is, happily, a suggestion in Hertz which can be followed up in order to attain a more adequate characterization of this dimension of theory comparison.

In what was almost an aside before he commenced his critique of the permissibility of Hamilton’s principle of least action, Hertz remarked: “In order that an image of certain external things may in our sense be permissible, not only must its characteristics be consistent amongst themselves, but they must not contradict the characteristics of other images already established in our knowledge.” Now this expansion of the view held earlier indicates that one of the significant aspects of permissibility is the amount of concordance between a theory to be assessed and the corpus of accepted scientific knowledge. If I may give this assessment category, as so generalized, a name, I shall label it theoretical context sufficiency, and shall accordingly be discussing the historical theoretical context within which a theory is to be judged.17

But we must do more for this dimension of theory comparison than simply broaden it to include the theoretical context of the time. It must also be stressed that a decision made in terms of the “theoretical context sufficiency” of a theory is not good for all time, for it may well turn out that a theory which is inconsistent with currently accepted theories is in fact correct, and that it is the various accepted theories constituting the theoretical context which require revision. This relativization of judgments made of the “theoretical context sufficiency” will not necessarily result in the type of relativity suggested by the historical school of philosophers of science, because of the joint manner in which the other categories of comparative theory evaluation function. We shall return to this difficulty in the context of a specific example below.

It must also be stressed that an assessment of the theoretical context sufficiency will vary from scientist to scientist, depending on his perspective and knowledge. Any philosophy of science must permit these “subjective or individual fluctuations” from the consensus of science at the time, though I think that it is a mistake to turn such fluctuations into a normative element of a philosophy of science as both Kuhn and Feyerabend seem to have done. We shall have an occasion to examine such “subjective fluctuations” in connection with Einstein’s view of Lorentz’s theory in 1905 and Lorentz’s view of Einstein’s in 1909.

B. Experimental Adequacy

Hertz’s notion of “correctness,” though it already has some flexibility and dynamism built into it, as it provides for revision in the assessment of correctness based on new and later experiments, is not yet adequate for our purposes. Recall the earlier account which I gave of the relation between theory and experiment. In that account experimental results or observation reports were admitted to be infused with theoretical meaning. Observation reports were, analogous to Popper’s basic statements, taken to be observationally accessible provisional stopping points captured.

17 See Sommerfeld (1952) for a critique, though, of Hertz’s account of the Newtonian-Lagrangian image.
in some language that is, by convention, admitted to be descriptive of those points.

Disagreement between a theory and an experimental outcome, then, is on this view a conflict between some complex of elements drawn from the corpus of accepted scientific knowledge, the theory under test, and sentences describing the interpreted sense experiences associated with the experiment. This claim is nothing new, for it was pointed out by Duhem (1906) early in this century. Nevertheless there are difficulties which are raised both by the Duhemian thesis and by my acceptance of some of the historical school’s claims regarding the infusion of theoretical meaning into observational sentences, and it would be worthwhile to indicate how the view being developed here (and in section II above) can be fitted with a thesis of experimental control over the acceptance of a theory. Accordingly what I wish to do now is to consider how, given the apparent flexibility of theories and their relations with experiment, a category of anything like “correctness” can continue to function.

The difficulty of introducing a modified form of Hertz’s category of correctness, a category which I shall term “experimental adequacy” inasmuch as “correctness” has unfortunately restrictive connotations, is twofold. First, as was discussed briefly above, the meaning of the O-sentence, since it is admitted to contain “theoretical elements,” might well change from theory to theory, thus ruling out an intertheoretic common experimental base to which competing theories must conform. This difficulty was sidestepped earlier, when I simply noted that certain O-sentences, such as are the result of the Michelson-Morley experiment, seem to have a common element from any of the competing theories’ points of view. But simply pointing out the prima facie common element is not sufficient; for we have a right to ask why there is this common element so as to be able to guard against the illusion of O-sentence meaning change. In other words, some general philosophical support is required for the apparent overlap of O-sentence meaning.

Second, we must also resolve a different difficulty which is connected with the issue of experimental control over theory acceptance. This is the issue of the relevance of an experiment (or observation report): does an experiment which is closely associated with theory \( T_1 \) in domain \( D \) also have to be associated with theory \( T_2 \) also applied in domain \( D \)?

Of these two difficulties, Feyerabend (1962, 1965) stresses the first, and Kuhn (1962) the second, though neither would disagree with either.

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We shall attempt to make use of the distinctions developed in section II above to resolve these difficulties.

Suppose that we are given two competing theories, say a Newtonian-like corpuscular optics and a Young-Fresnel wave optics. Further, suppose that \( O_1 \) represents an O-sentence describing a fringe pattern on a certain field of view, say on a screen, a, located behind two narrow parallel slits illuminated by a monochromatic light source. I shall assume that such a sentence contains (1) terms which are “ostensively” definable (to provide the “primary sense” noted below), (2) logical terms which serve as connectors, and (3) mathematical terms which are theory neutral. Our difficulties, then, will concern the meanings of the first set of terms, which we may call O-terms or O-predicates.

We now attempt to distinguish two general types of senses ascribable to the O-predicate in this situation. The first or primary sense is that associated with the referent, the bandlike appearance one experiences in learning the meaning of “optical fringe phenomena.” The second sense is that which is associated with the O-term by virtue of the term’s being embedded or hooked into a theory through correspondence rules or C-sentences such as were discussed earlier. Such embedment is achieved if the O-term appears in an O-sentence which is the terminal element of a C-sentence chain initiating in a theory. Only in the second sense do we talk of seeing the fringes as evidence for wave optics (or for corpuscular optics).

The primary sense of the O-term then is common to both the Newtonian theorist’s use of scientific language and the wave theorist’s use. Furthermore, it is common not by virtue of being part of some “higher-level background theory,” as Feyerabend (1965) would suggest, but rather because both our higher-level theories of wave optics and corpuscular optics contain low-level theories (supposing such O-sentences to be theoretical) which are connected with them by C-sentences. This low-level theory uses the theoretically uninteresting language of spots, fringes, colors, and the like, in as precise a way as possible by measuring the fringes with scales and attempting to construct intensity scales of colors and brightness. More overlap than this, however, is not necessary, for overlap only need occur at the points where both competing theories make contact with experience. The terms, in their primary observational sense, simply do not change meaning with the ease which Feyerabend suggests they do. Feyerabend
can only maintain his thesis by conflating the two senses of the O-terms which I have distinguished.

Such a thesis, then, contends that a “withdrawal” or “retrenchment” is possible when scientists of different and conflicting theoretical orientations are analyzing the output of an experiment. If disagreement on the meaning of the O-sentence, in its primary sense, is present, it is possible, such a thesis would maintain, that the sentence could be further analyzed so as to reach a common level. This new common level would then be the primary sense of the O-sentence vis-à-vis this theoretical conflict. Thus the distinction between primary and secondary senses is relative, but relative to theoretical comparison, and not to theories in isolation. Though it is logically possible that there be no common basis no matter how far the primary sense is sought for, this seems most unlikely, for it is not supported by any of the examples in the literature designed to exemplify differences in the meaning of observation terms relative to different theories. Though this thesis of withdrawal or retrenchment in the search for a common empirical base is perhaps put to new ends here, it is to be found in its essentials in Popper’s (1959) discussion of the “relativity of basic statements.”

An answer to the second difficulty mentioned above, that of the change in the relevance of an experiment due to a change in theory, is more complex and requires to a certain extent a more pragmatic argument. Again I refer to Popper (1959) who asserted: “... a theory which has been well corroborated can only be superseded by one of a higher level of universality; that is, by a theory which is better testable and which in addition, contains the old, well corroborated theory—or at least a good approximation to it.”

Though it is precisely this and similar theses which are effectually attacked by Feyerabend (1962, 1965) and Kuhn (1962), there is still some merit to such an inclusionist claim, though it can be overstated and misinterpreted. The merit accrues to the claim because the purpose of science is both to introduce system into our knowledge and to enable us to anticipate the future course of events. Consequently, if a new theory has significantly less breadth than the presently accepted theory, it is not likely to be considered as a serious candidate to replace the older theory. The body of experimental evidence associated with and tied together by the old theory accordingly constitutes a body of knowledge which the new theory must either accommodate or give good reasons why it cannot and should not do so.¹⁹ Such “good reasons” can be strictly internal to the new theory.

On the other hand, as I pointed out in section II, it is the new theory, by virtue of its antecedent theoretical meaning and its antecedently understandable hypothesis, and its associated theories, which will inform us whether a body of experimental knowledge should be relevant to its claims. It is this aspect of the theory which permits it to be extended into new domains, as well as to allow it to accommodate old knowledge in new ways. Thus there is the possibility of a tension between what “experimental” knowledge the old theory informs us should be accommodated by its potential replacement, and that to which the new theory says it should apply. Thus Newton’s optics indicated that any new optical theory should relate and explicate the rectilinear propagation of light rays, reflection, refraction, and “Newton’s rings.” Fresnel’s optics agreed, and also contended that it should also account for diffraction patterns.

In order for two theories to compete, as Lorentz’s and Einstein’s theories do, and as Lorentz’s and Mendel’s genetic theory do not, the competing theories must overlap in the sense of providing alternative accounts of the same experiments and observations. I indicated above how and why this was possible. But when the theories diverge, say as Lorentz’s and Einstein’s do inasmuch as Lorentz’s theory gives an explanation of the Zeeman effect, which Einstein’s does not, and Einstein’s theory accounts for the inertial energy, i.e., \( E/c^2 = m \), whereas Lorentz’s theory does not, the external constraint on the relevance of an experiment disappears, and only the (internal) antecedent theoretical meaning can determine the relevance of experiments. Nevertheless, there is still the possibility of conflict between the assertions of the theory which are testable by experiment and the O-sentences which are the outcome of the experiments. Accordingly, even where there is divergence between successive theories, the category of “experimental adequacy” may still be employed, though in less straightforward a manner than in cases where there is overlap between competing theories and a more obvious determination of the relative success of the competing theories can be made on a completely common field of battle.

¹⁹ Shapere (1970) has formulated the technical notion of a “domain” in order to introduce in an extra-theoretical manner similar constraints to those which I cite here. Shapere’s notion, however, is directed not so much at the problem of theory acceptance as at the problem of theory creation and modification.
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C. Simplicity

Hertz’s notion of appropriateness is the most difficult to specify as he himself admitted. What is wanted here is something like the minimal number of concepts and principles necessary to account for a collection of experimental and theoretical results constituting a branch of science. As such, appropriateness is very much like sparseness or “simplicity,” and I shall be using this latter term in place of Hertz’s term “appropriateness.” It will turn out, though, that there are several different senses of “simplicity,” which it is wise to distinguish, and even though some of them have been so distinguished in recent writings on simplicity by philosophers of science,²⁹ not all of the senses of the term, as applied in Hertz’s analysis, have been characterized.

Before I turn to an analysis of simplicity, however, it would be well to pause and to note one difficulty which was raised in the previous section on experimental adequacy and which is relevant here. This was the claim that different competing theories might diverge in their areas of application. A consequence of this possible divergence is the difficulty of giving any extra-theoretical characterization of what it is that a theory should account for, i.e., there may be a difference in the “collections” cited in the immediately preceding paragraph for two competing theories. This fact might be thought to lead to considerable difficulties if a broader theory were more complex, for one would be faced with estimating the relative merits of a simple narrow theory and those of a broad complex theory. Happily this difficulty does not seem to arise, insofar as simplicity considerations are normally outweighed by considerations of experimental adequacy in such cases of category assessment conflict. In general, then, a broader and more complex theory will be acceptable over a simpler narrower theory. (To a certain extent a case like this occurred when Maxwell’s electromagnetic theory of light became, in 1865 in England, favored over a conjunction of the simpler Weberian theory of electrodynamics and the simpler optical ether theories. This occurred before Hertz’s important experiments.)

My first task as regards a generalization of Hertz’s category of appropriateness is to convey a clearer idea of his notion, or rather of his notions of appropriateness, as there are several intersecting ideas grouped under this term. Unfortunately the only way to do this is to examine the manner

²⁹ For example see Rudner’s (1961) typology of simplicity, and also the collection of articles on simplicity in Foster and Martin’s (1966).

in which Hertz applied the notion in its various senses in the context of his analysis of the relative merits of the different images of mechanics. We can then generalize from these examples.

In connection with the Newtonian-Lagrangian image of mechanics, Hertz noted that there were many situations permitted and characterizable by that image for which there was no experimental evidence. Hertz (1894) wrote:

All the motions of which the fundamental laws [of the Newtonian-Lagrangian image] admit, and which are treated of in mechanics as mathematical exercises do not occur in nature. Of natural motions, forces, and fixed connections, we can predicate more than the accepted fundamental laws do. Since the middle of this century we have been firmly convinced that no forces actually exist in nature which would involve a violation of the principle of the conservation of energy. . . . In short, then, so far as the forces, as well as the fixed relations, are concerned, our system of principles embraces all the natural motions; but it also includes very many motions which are not natural. A system which excludes the latter, or even a part of them, would picture more of the actual relations of things to each other, and would therefore in this sense be more appropriate.

I shall call this sense of appropriateness “fitness,” for want of a better term. It corresponds to Hertz’s earlier use of the term “distinctness” which does not seem apt. There is, as far as I am aware, no sense of simplicity in the current literature which touches on this issue in exactly the Hertzian sense, though some explications of “inductive simplicity” approximate it if generalized from the excessively formalistic “curve-fitting” orientation which infuses such explications. (Cf. Rudner, 1961.)

It should be noted that the determination of “fitness” will usually require some time lapse after the initial enunciation of a scientific theory. Since fitness is related to a minimization of the ultimately non-testable, and usually less easily initially checkable, experimental consequences of a scientific theory, it cannot be easily determined when a theory is first broached. Nevertheless it is sometimes fairly easy to make a relative or comparative determination of the fitness of two competing theories within a previously well-researched area of application, if the new theory does not go, or is not thought to go, extensively beyond the scope of the old theory.

Hertz also considered another dimension of appropriateness. He wrote in the same context as above:

We are next bound to inquire as to the appropriateness of our image in a second direction. Is our image simple? Is it sparing in unessential characteristics—ones added by ourselves, permissibly and yet arbitrarily, to the
essential and natural ones? In answering this question our thoughts turn again to the idea of force. It cannot be denied that in very many cases the forces which are used in mechanics for treating physical problems are simply sleeping partners, which keep out of the business altogether when actual facts have to be represented. . . . [Now] we have felt sure from the beginning that unessential relations could not be altogether avoided in our images. All that we can ask is that these relations should, as far as possible, be restricted, and that a wise discretion should be observed in their use. But has physics always been sparing in the use of such relations? Has it not rather been compelled to fill the world to overflowing with forces of the most various kinds—with forces which never appeared in the phenomena, even with forces which only came into action in exceptional cases? . . . Now if we place these conceptions before some unprejudiced persons who will believe us? Whom shall we convince that we are speaking of actual things, not images of a riotous imagination? . . . Whether complications can be entirely avoided is questionable; but there can be no question that a system of mechanics which does avoid or exclude them is simpler, and in this sense more appropriate, than the one here considered; for the latter not only permits such conceptions, but directly obtrudes them upon us.

In these passages we can distinguish, though Hertz does not do this explicitly, two further senses of appropriateness or simplicity: one is a terminological and/or ontological simplicity; secondly there is a simplicity of system. The conjunction-disjunction of terminological ontological is used because for Hertz, as for myself, the fundamental terms which appear in a scientific theory are to be taken in a realistic sense. The notion of simplicity of system refers to the comparative simplicity of two axiom systems, in which it is assumed that each axiom represents a further irreducible physical effect. This restriction is imposed to eliminate the trivialization of this aspect of simplicity which would occur if all the axioms could be conjoined into one axiom.21

It is obvious that by making a virtue of simplicity we make a vice of complexity. One particularly odious form of complexity is that which accrues to a theory which has many ad hoc hypotheses associated with it. Such complexity not only is contrary to our desire for sparseness and elegance in our scientific theories, but also permits a theory to outflank the controls imposed on theoretical speculation by the restrictions cited in connection with the category of “experimental adequacy.” Therefore, from the point of view of the logic of comparative theory evaluation being sketched here, theories containing ad hoc hypotheses are doubly suspect.

The notion of an ad hoc hypothesis has been succinctly characterized by Grünbaum (1964) as a hypothesis which is added to a theory solely to enable it to outflank some unexpected and embarrassing result, an E-result, and which hypothesis (plus the theory under test) has no further additional testable consequences which differ from the E-result in an interesting and significant way. Though the use of the terms “interesting” and “significant” introduces pragmatic elements into the characterization, they are not necessarily subjective ones. This sense of ad hocness can be termed intrasystemic ad hocness, to distinguish it from a different sense of ad hocness of an intersystemic type. In this form, a hypothesis H is ad hoc if it is conjoined to a theory T₁, to obviate the necessity of accepting a new theory T₂, which accounts for the E-result without H. H, as in the previous sense, is not supposed to entail, in conjunction with T₁, any additional “interesting and significant” results.

I believe that I can accept either or both of these senses of ad hocness within the logic of comparative theory evaluation which is being proposed here. In both cases, if an ad hoc H is accepted, there will be an increase in the system complexity, and possibly also an increase in the terminological/ontological complexity. A decrease in fitness is also likely in such circumstances.

It now remains only to introduce some comments on the way in which a judgment of theoretical context sufficiency may outweigh and overpower the type of judgment made in connection with the relative simplicity of two competing theories.

The judgment whether one ontology, say, is simpler, in any useful sense, than another is contingent on the ability of the scientist to seriously consider ontological shifts. Thus a particle physics ontology based on “quarks” is generally thought to be simpler than one without the still observationally inaccessible particles. Suppose, however, that physicists could not accept the existence of quarks because their existence would violate some
fundamental physical principle, say the conservation of energy or Lorentz covariance. If this were the case, it would seem that the “ontological simplicity” to be gained by the acceptance of a quark-based particle physics would be of questionable value. Later I shall consider a case quite similar to this which actually occurred in the history of science. The implication is that the theoretical context considerations may overwhelm any simplicity assessment.

The theoretical context also, and perhaps more directly, influences simplicity assessments in yet another way. Given strong theoretical reasons for wishing to retain or maintain some entity or principle, an additional hypothesis, which is conjoined to a theory to enable the theory to survive a prima facie falsifying experiment, will not appear as odiously ad hoc as it would if such strong reasons were lacking. Accordingly, the assessment of system simplicity vis-à-vis the problem of ad hoc modifications of the system is also potentially influenceable by determinations made in the first assessment category.

It should be clear that such a logic of comparative theory assessment as I have been outlining will not employ formal or “effective” concepts. It does not constitute a type of easily applicable schema which can result in an automatic decision for the person thinking in terms of its categories. Nevertheless I do think that the categories of theoretical context sufficiency, experimental adequacy, and relative simplicity do accurately characterize the process of comparative theory evaluation as it is practiced by scientists making the history of science. In order to show how the categories work in practice and in order to delineate the features of the categories more adequately I now turn to a specific and somewhat controversial case in the history of recent physics.

V. The Electrodynamics of Moving Bodies in the Early Twentieth Century

An examination of scientific articles and textbooks written in the years 1900–5 yields the unmistakable conclusion that the electron theory of H. A. Lorentz enjoyed supremacy among the various electromagnetic theories of moving bodies. In a long monograph published in 1901 which was based on lectures given in 1899 at the Sorbonne, Henri Poincaré compared the theories of Maxwell, Hertz, Larmor, and Lorentz on the electrodynamics of moving bodies. Although he had certain reservations having to do with an apparent failure of Newton’s third law in Lorentz’s theory, Poincaré concluded that the Lorentz theory offered the greatest promise of all the then current theories.

P. Drude’s important Lehrbuch der Optik which was published in Germany in 1900 and in English translation in 1902 and which became a widely used optics text, presented Lorentz’s 1895 theory in a section on the optics of moving bodies. In the next few years, studies on the properties of the “empirical electron,” and Lorentz’s development of a more adequate second-order theory of the electrodynamics of moving bodies, only served to increase the influence of the Lorentz electron theory. A new world picture based on the electron theory and conceiving of all inertial mass as due to electromagnetic interactions was being developed by Wien and Abraham. Lorentz’s theory fit quite well into this new Weltbild which suggested that traditional mechanics might be in need of revision if it was to conform to the new picture of the world as basically electromagnetic in nature.

In September of 1905 Einstein published a brief paper on the electrodynamics of moving bodies which obtained what appeared to be many of the same results as had Lorentz, though Einstein reasoned to them in such a way as to throw a very different perspective on the nature of space and time and the laws of nature. By 1909–10, in only about four or five years’ time, most of the German physicists had been converted to Einstein’s theory from the Lorentz approach. In other countries the pace of conversion was slower, but in general was not delayed beyond the middle teens.

Einstein’s theory of the electrodynamics of moving bodies—the special theory of relativity—constituted in many ways a very abrupt departure from traditional ways of thought—a true scientific revolution. This, together with the fact that there are many diverse points at which the theory obtains experimental support and the fact that it appears so different from the Lorentz theory as regards its prima facie “simplicity,” suggests that the study of the transition from pre- to post-relativistic electrodynamics might afford an excellent test area in which to apply the logic of comparative theory evaluation sketched out above.

33 See Jäger (1961), McCormmach (unpublished), and Goldberg (1970b) for discussions of the electromagnetic view of nature.
34 See Goldberg (1969a) and also his (1970a) for indications of the difference in the rate of acceptance of relativity by country.
A. The Foundations of the Electron Theory

The genesis and development of H. A. Lorentz’s theory of the electrodynamics of moving bodies cannot be examined in any significant detail in these pages. What I propose to do is to outline the structure of the Lorentz electron theory, largely insofar as it applies to the electrodynamics of moving bodies, in its mature 1904-9 form. Then I shall do the same for Einstein’s theory, and conclude with a relative assessment of the two theories based on the modified tricategorical Hertzian analysis developed above.

Lorentz’s electron theory has its roots in Maxwell’s electromagnetic field theory. Lorentz proposed, as was expected for any field theory at the end of the nineteenth and in the beginning of the twentieth centuries, the existence of an ether or a medium which pervaded all space and, for Lorentz, even ponderable bodies. There were certain “states” of this medium which gave rise to electrical and magnetic forces acting on electrified bodies and magnetic poles. Contrary to Maxwell, though like Hertz, Lorentz refused to speculate more on the state of the medium which might account for these forces. It was sufficient to define the electrical and magnetic fields in terms of these “forces” and to relate the forces in partial differential equations. For Lorentz the ether was essentially motionless; bodies could not drag any ether with them as they moved through it and though the ether could act on electrified and magnetized bodies, these bodies could not react on the ether, as this would require setting it in motion. Lorentz does introduce, as did Maxwell, a “dielectric displacement” though we are not told what, if anything, is being displaced, and in the free ether, anyway, the dielectric displacement and the electric force have the same magnitude and direction.

The basic equations which hold for the electromagnetic field in the (charge) free ether are:

(1) \[ \text{div} \, d = 0 \]
(2) \[ \text{div} \, h = 0 \]
(3) \[ \text{curl} \, h = \frac{1}{c} \frac{\partial d}{\partial t} \]

where \( d \) is the dielectric displacement, \( h \) is the magnetic force, and \( c \) is a constant depending on the properties of the ether (\( c \), of course, will turn out to be the velocity of a disturbance propagated through the ether which, for a short range of frequencies, is visible light).

Lorentz also introduced the notion of extremely small charged particles which he at first termed “ions” but later, after the analysis of cathode and \( \beta \) rays had been carried out and Stony’s term adopted, called them “electrons.” These electrons have a specific structure for Lorentz. They are essentially spherical but he also noted: “We shall consider the volume density \( \rho \) [of the electron’s charge] as a continuous function of the coordinates so that the charged particle has no sharp boundary, but is surrounded by a thin layer in which the density gradually sinks from the value it has within the electron to zero.” Some of these electrons are free to move, especially electrons found in the interior of conductors; others in dielectrics, for example, are bound loosely to atoms in the ponderable bodies and can vibrate.

The equations for the state of the ether within the electrons require only the slightest modification of the equations already given for the (charge) free ether. In place of (1) we write:

\[ \text{div} \, d = \rho \]

and in place of (3) we substitute:

\[ \text{curl} \, h = \frac{1}{c} \left( \frac{\partial d}{\partial t} + \rho \nu \right) \]

where \( \nu \) is the absolute velocity of the charge. Lorentz also adds a fifth equation:

\[ f = d + \frac{1}{c} (\nu \times h) \]

which represents the dynamical force per unit charge produced by the ether on a charge. This equation can be derived from the others by adding vectorially the two forces experienced in a field by a moving charge.

From the above the reader should be able to see how the antecedent theoretical meaning of the theory of electrons is developed.


*This analysis is based on Lorentz’s (1904b, 1909) works.

*Lorentz subsequently relaxed his position on this point by admitting the existence of an "electromagnetic momentum" in the ether. This also resolved difficulties over the apparent failure of Newton’s third law in his electron theory. See Lorentz (1904a, b, and 1909) for discussion of electromagnetic momentum.

*Lorentz (1909), p. 11.
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By applying these equations to free electrons moving in metals Lorentz obtained causal explanations for many empirically established formulas, such as had been established by the experimental work of Drude and others. Lorentz also developed an explanation of the splitting of spectral lines in a magnetic field—the well-known Zeeman effect—by utilizing Newton's laws of motion together with his own equations and applying them to the loosely bound electrons in atoms. It is in this and similar accounts that we see the exemplification of what I referred to as the establishment of correspondence rules or C-sentences: connections between theoretical processes and experimental or observationally accessible states of affairs.

B. Lorentz's Electron Theory Applied to Moving Bodies

From the birth of his electron theory in 1892 on, Lorentz had been concerned to give explanations of various experiments made on systems of moving light waves. The well-known Fresnel partial dragging coefficient,\[ \left(1 - \frac{1}{n^2}\right),\]

where \(n\) is the index of refraction of a transparent body, which is needed to account for many experiments in the field of the optics of moving bodies, had been derived by Lorentz in 1892. The Michelson-Morley experiment received a sort of ad hoc explanation when Lorentz suggested that objects moving through the ether contracted just enough to compensate for the difference in the path of the light waves in the moving interferometer. In 1895 Lorentz proved a general theorem from his equations—known as his theorem of corresponding states—which showed that if a new time variable, a “local time,” could be substituted for the universal time in the moving system, no first-order effect of the earth’s motion on an electromagnetic system would be detectable. It is most important to realize, however, that the “local time” for Lorentz was, as he later termed it, “no more than an auxiliary mathematical quantity.” Pauli (1958) explicates the sense of this by noting that such a “local time” means that “the origin of \(t'\) was taken to be a linear function of the space coordinates, while the time scale was assumed to be unchanged.” Lorentz did not invest his “local time” with any physical significance, other than to associate it with the ether wind, and in particular he did not identify it with the time of the moving system, until after he had read Einstein’s papers on relativity.

Lorentz’s theory of 1895, then, gave a good account of experiments in the domain of the electrodynamics of moving bodies known at that time. But the field was not static, and by 1904 new second-order experiments had been performed by Trouton and Noble and by Rayleigh and Brace which raised additional problems for Lorentz, and which caused him to reformulate his theory of moving bodies and to generalize it to second- and higher-order experiments.

Lorentz, in developing his 1904 theory, began with the fundamental equations of his electron theory given above as (1), (2), (3), (4), and (5). He then applied a “Galilean velocity transformation,” \(x' = x - vt\), to these fundamental equations, and then superadded another change of variables:

\[
\begin{align*}
(6) & \quad x' = \beta lx_r \\
(7) & \quad y' = ly_r \\
(8) & \quad z' = lz_r \\
(9) & \quad t' = \frac{1}{\beta} t - \beta \frac{v}{c^2} x_r
\end{align*}
\]

where

\[\beta = \sqrt{1 - \frac{v^2}{c^2}}\]

and \(l\) is an as yet unknown function of \(v\). Equations (6)–(9) are the well-known Lorentz transformation equations, but it would be well to pause for a moment and recall their meaning for Lorentz.

Equation (6) represents the transformation of length that captures the Lorentz-Fitzgerald contraction. This is a shrinkage caused by the motion of bodies through the ether with an absolute velocity equal to the \(v\) appearing in the transformation. The \(t'\) in equation (9) represents the latest form of the “local time” which holds in moving electromagnetic systems.

\[\text{Cf. Lorentz (1909), pp. 62ff.}\]
\[\text{See Schaffner (1969a) and especially (1970), chapters 3 and 6, for a discussion of the Fresnel partial dragging or convection coefficient.}\]
\[\text{See same references as cited in note 31 for a discussion of the Michelson-Morley experiment}\]

\[\text{The partial quotation from Lorentz is from his (1915), p. 321, the Pauli quote is from his (1958; originally published in 1921), p. 1.}\]
\[\text{The difference in meaning between Lorentz’s and Einstein’s transformation equation has been discussed by d'Abro (1927), Grünbaum (1961, 1963), Popper (1966), and myself (1969a, 1970).}\]
A Logic of Comparative Theory Evaluation

It is still to be distinguished from the “true” Newtonian time holding in bodies at rest in the ether.

Transformations for \( d \) and \( h \) were introduced by postulation, but they vary only slightly from what would have been expected from a standard referral of a system of charges to a moving coordinate system. Lorentz also postulated transformations for charge density:

\[
\rho' = \rho/\beta^3
\]

and he also introduced a modified velocity addition formula, claiming that if an electron had a velocity \( u \) in addition to the velocity \( v \) of the system of charges to which it belonged, the velocity in the primed system of coordinates would be

\[
u_x' = u_x \beta^2 \quad u_y' = \beta u_y \quad u_z' = \beta u_z
\]

In a later section of his (1904b) paper, Lorentz also introduced a new vector representing electric moment (due to polarization), symbolized by \( \mathbf{P} \), and also postulated its transformation properties.

From his fundamental equations plus the transformations which he had simply postulated in addition to them, Lorentz was able to derive expressions for the dielectric displacement and Lorentz force acting on electrons as referred to the moving system. He was also able to calculate the electromagnetic momentum of such a system, something he had to do to obtain results that would enable him to account for the outcome of the Trouton-Noble experiment.

This rather complex system of equations, however, had to be complicated even more, before Lorentz could attain to his goal of proving a theorem of corresponding states for many electromagnetic phenomena to all orders of \( v/c \). He also found it necessary to add: (1) a special hypothesis postulating that the electron contracted in accordance with the Lorentz-Fitzgerald contraction and (2) still another hypothesis concerning the transformation properties of any forces holding between particles and (3) yet another hypothesis proposing that all the electron’s mass be taken as electromagnetic mass. The last hypothesis had some experimental evidence in its favor, however, and was then a current belief of all those who were working out electromagnetic Weltbilder.

By applying these various hypotheses to the motion of electrons, and, in addition, by making use of Newton’s second law of motion, Lorentz was able to obtain the value of the undetermined \( \lambda \) function which appeared in his transformation equations; it becomes equal to unity.

From here Lorentz went on to prove a generalized theorem of corresponding states. Lorentz wrote (1904b):

We may sum up by saying: If in the system without translation, there is a state of motion in which, at a definite place, the components of \( \mathbf{P}, \mathbf{d}, \) and \( \mathbf{h} \) are certain functions of the time, then the same system after it has been put in motion (and thereby deformed) can be the state of a state of motion in which, at the corresponding place, the components of \( \mathbf{P}', \mathbf{d}' \) and \( \mathbf{h}' \) are the same functions of the local time.

Lorentz concluded that these new time variables, contractions, mass transformations, and the like, so interacted that many experiments, and in particular the Michelson-Morley, Trouton-Noble, and Rayleigh-Brace experiments, performed in optics and electromagnetic theory would produce null results.

Lorentz’s theory does not constitute a theory of relativity nor does the theorem of corresponding states constitute a principle of relativity. In Lorentz’s own words, he was attempting to show that “many electromagnetic actions are entirely independent of the motion of the [moving] system” (my emphasis).

I do not have the space in this paper to comment on Poincaré’s extension of Lorentz’s approach, and his publication, independently of Einstein, of a principle of relativity which he, Poincaré, actually used to modify Lorentz’s charge density transformations, and the velocity transformation, so as to obtain complete invariance of the fundamental equations.

VII. Einstein’s Special Theory of Relativity

Einstein’s special theory of relativity constituted a significant departure from the Lorentz theory. Einstein’s assessment of the theoretical context sufficiency of Lorentz’s 1895 theory was somewhat different from Lorentz’s assessment which motivated him to produce his 1904 version. By early 1905 Einstein had already satisfied himself as to the microscopic inadequacy of the Maxwell and the Lorentz theories in his paper on light quanta and the photoelectric effect. Einstein also felt that Lorentz’s theory, with its ether rest frame, implied the existence of asymmetries in

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optical and electromagnetic phenomena for which there was no experimental foundation.

In Einstein's assessment of the adequacy of the Lorentz theory we see an example of a "subjective fluctuation" from the consensus of science. Part of Einstein's distrust of the Lorentz theory was shared by others, in particular Poincaré, but part was uniquely his own. This is especially true of Einstein's rejection of the Lorentz "constructive" approach because of his own work on light quanta. Perusal of Professor Stuewer's paper in this volume will indicate how alone Einstein stood on this ground.

Though Einstein was not able to formulate what he would call a "constructive" type of theory of electrodynamics, basically because of the difficulties introduced by the light quantum hypothesis, he did discover that a theory of principle—on the analogy with the second law of thermodynamics, and as distinguished from a "constructive" type of theory such as statistical mechanics—could be formulated. In order to do so, however, a new kinematics would be necessary: only with the aid of new conceptions of space and time could a simple and consistent theory of the electrodynamics of moving bodies be developed.

Einstein's theory is essentially a new kinematics based on the principles of the constancy of the velocity of light and the principle of relativity. The first principle is the heritage of the wave theory of light and asserts only, contrary to the beliefs of some students of relativity, that the velocity of an emitting body has no effect on the velocity with which the light is propagated. This, when conjoined with the principle of relativity, that is, that "the laws by which states of physical systems undergo change are not affected whether these changes of state be referred to the one or the other of two systems of coordinates in uniform translating motion," resulted in a paradox. The paradox was resolved by Einstein by a brilliant reanalysis of the notion of time or of simultaneous measurements in which he exposed the conventional character of some of the fundamental notions of physics.

Einstein's kinematics led to what formally appeared to be the same space and time transformations as Lorentz had postulated. There were, however, several significant differences. Central to the antecedent theoretical meaning of the special theory of relativity is the thesis of the equivalence of all unaccelerated reference systems and of the relativity of velocities which appear in the transformation equations. For Lorentz the v term appearing in his equations represented absolute velocity, and the transformations between moving and rest frames are not reciprocal, for in shifting from a moving frame of reference to one at rest in the ether one obtains an "expansion" effect which is built into Lorentz's formalism. For Einstein there were reciprocal contractions which are due to reciprocal failures of the identity of simultaneity and not to alterations in the molecular forces due to the translation of a body through the ether.

It should also be pointed out that Einstein's derivation of the transformations transcended the electron theoretical foundations: it was a "kinematical" derivation based only on the two principles cited above, and such generality immediately suggested that the new kinematics might well apply in other areas outside of electrodynamics.

Einstein's principle of relativity and his new kinematics were, in the second part of his 1905 paper, applied to electrodynamics, i.e., to the Maxwell-Hertz equations for bodies at rest and also to the fundamental equations of the Lorentz electron theory. Einstein obtained the same transformations for electric force (or dielectric displacement) and magnetic force as Lorentz was forced to postulate, and Einstein also derived a different charge density transformation, which, taken together with the Einsteinian velocity addition formula proven in the kinematical part of the paper, yielded complete invariance of the Lorentz equations under the Lorentz space and time transformation equations. Einstein also obtained, deductively, the Lorentz expressions for the velocity dependence of the mass of an electron. There are other important results developed in this second part of Einstein's paper, but they do not concern our present task.

VIII. A Comparison of Lorentz's and Einstein's Theories from the Point of View of the Logic of Comparative Theory Evaluation
A. Theoretical Context Sufficiency

The theoretical context in which the Einstein special theory of relativity first appeared was still dominated by Newtonian mechanics and its later reformulations, though the mechanical approach was considerably weaker than it had been, say, in the middle years of the nineteenth century. Maxwell's electromagnetic theory, owing to the experimental work of Hertz, had, about seventeen years previously, triumphed over various action-at-a-distance theories of electromagnetism, and had in the 1890's begun to

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88 Einstein (1905b).
89 See Reichenbach (1957) and Grünbaum (1963) for in-depth analyses of the philosophical aspects of Einstein's reanalysis of simultaneity.

See my (1969a) for more details.
replace the older theories of the mechanical optical ether. Maxwell's theory was still conceived of as ultimately explicable by mechanics,\textsuperscript{41} though in the early years of the twentieth century several physicists, building on the considerable successes of Lorentz's electron theory, began developing a new electromagnetic view of nature which would serve to compete, as regards fundamentality, with the Newtonian mechanical view.\textsuperscript{42} The success of thermodynamics as a nonmechanical theory had engendered a vigorous, if rather polemical, energetic school, and Ernst Mach's important philosophical-historical critiques fanned the flames of the anti-mechanist approach to physics. Max Planck had, in 1900, discovered the quantum of action, but was still attempting to accommodate it within classical physics.\textsuperscript{43}

In spite of the weakening of the classical Newtonian ideal, Max Born, in commenting on the scientific atmosphere at the time he was a student of physics during 1901–7, indicated that the influence of the Newtonian ideas was still significant. Born (1956) wrote: “Newton's mechanics still dominated the field completely, in spite of the revolutionary discoveries made during the preceding decade, X-rays, radioactivity, the electron, the radiation formula and the quantum of energy, etc. The student was still taught—and I think not only in Germany, but everywhere—that the aim of physics was to reduce all phenomena to the motion of particles according to Newton's laws, and to doubt these laws was heresy never attempted.”

I think that this statement of Born's is a bit misleading, for even if students were being taught that Newton was unchallengeable, their teachers were not so dissuaded from questioning the Newtonian laws of dynamics. Lorentz (1895) had shrugged off the failure of Newton's third law in his theory, though this difficulty was later resolved in the early twentieth century.\textsuperscript{44} Poincaré had speculated in the early 1900's about a “new mechanics” which might have the speed of light as a limiting velocity, and the proponents of an electromagnetic view of nature were questioning the validity of the Newtonian scheme. In his (1909) work, Poincaré considered in a fairly systematic manner some of the implications of the Lorentz theory as amended by his own extensions (Poincaré, 1906). Though Poincaré did think that the Lorentz theory would entail revisions in the traditional mechanics, much as Einstein's theory would, such revisions are less revolutionary than Einstein's revisions, because they are based on extensions of accepted electromagnetic theory, e.g., forces are taken to be (and to transform as) electrical forces, and mass is conceived of as velocity dependent because of the self-induction of the electron. Such modifications are accordingly alterations in dynamics.

Even though traditional dynamics was being questioned, however, traditional kinematics, the science of space and time, was not under fire. Furthermore, there was little criticism of the notion of the electrodynamic ether, though physicists had begun to find it more convenient to work with Maxwell's and Lorentz's equations, per se, and not worry about further mechanical ethers underlying these field theories.

It was into this theoretical context that Einstein's special theory of relativity intruded, eliminating the ether, revolutionizing the basic concepts of space and time, and implying that Newton was, at best, only approximately right. Einstein's radical analysis of simultaneity and the concept of time was the reason why his theory was so novel and exciting, but also the reason why it was so suspect and difficult to accept. Born (1956) wrote concerning this point:

In 1907 . . . I returned to my home city Breslau, and there at last I heard the name of Einstein and read his papers. I was working at that time on a relativistic problem and [a friend] . . . directed my attention to Einstein's articles . . . . Although I was quite familiar with the relativistic idea [apparently in the Lorentz sense] and the Lorentz transformations, Einstein's reasoning was a revelation to me. . . . For me—and many others—the exciting feature of the paper [Einstein, 1905b] was not so much its simplicity and completeness, but the audacity of challenging Isaac Newton's established philosophy, the traditional concepts of space and time, that distinguishes Einstein's work from his predecessors.\textsuperscript{45}

Lorentz also found Einstein's theory quite different from his own, and in 1909 characterized it as “very interesting” and remarked at its “fascinating boldness” and at its “simplicity.” Nevertheless, he could not yet accept it. In 1915, however, he wrote in the second edition of The Theory of Electrons:

If I had to write the last chapter ["On Optical Phenomena in Moving Bodies"] now, I should certainly have given a more prominent place to Einstein's theory of relativity . . . by which the theory of electromag-

\textsuperscript{41} The relation of Maxwell's theory to mechanics is discussed extensively in my (1970), chapters 4 and 5.

\textsuperscript{42} See Jammer (1961), McCormmach (unpublished), and Goldberg (1970b).

\textsuperscript{43} Important discussions of the theoretical context at this time are given by Holton (1967–8), by Klein (1967) and, of course, in the most important autobiographical notes of Einstein (1949).

\textsuperscript{44} See note 28, and especially see Lorentz (1909), pp. 30–32.