

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.

## *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.<sup>1</sup>

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.<sup>2</sup> In varying ways they have suggested that:

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<sup>1</sup>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).

<sup>2</sup>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

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.<sup>3</sup>

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.<sup>4</sup>

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.

<sup>3</sup> Cf. Feyerabend (1965).

<sup>4</sup> One clear exception to this criticism of a "falsifiability" approach is the recent work of I. Lakatos (1968, 1970), who has proposed a "sophisticated falsification" approach which is to bridge the gap, as it were, between Popper and Kuhn.

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 metaphysics 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.<sup>5</sup>

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,<sup>6</sup> historians of science do not avail themselves of the contributions of the philosophy of science. Some historians do, of course, like L. Pearce Williams,<sup>7</sup> 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.<sup>8</sup> 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

<sup>5</sup> See Scheffler (1967) and Shapere (1964, 1966) for general comments and critiques of these and other implications of the historical school.

<sup>6</sup> Cf. Clark (1962).

<sup>7</sup> Cf. Williams (1965, 1966).

<sup>8</sup> 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.<sup>9</sup> 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.<sup>10</sup>

<sup>9</sup> See my (1969b) for more details on this notion of antecedent theoretical meaning.

<sup>10</sup> 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.

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-

servationally accessible without the immediate use of artificial instruments. The second class of O-sentences covers such things as photographs with curved lines on them (bubble chamber *photographs*) and the numerical total, i.e., the readable number sign, on a particle counter.

The C-sentences then connect theoretical terms with "observational" terms, and are thus strongly analogous to what traditionally have been termed "correspondence rules." They differ from the usual analysis of correspondence rules in that these C-sentences are further analyzable into chains of sentences, each element of which characterizes a state of affairs, and which causally or nomically implies the next element in the chain. The causal or nomological implications are warranted by well-confirmed or well-corroborated auxiliary theories of science. Thus in the C-sentence of the Bohr atomic theory connecting the "theoretical" notion of an "electron transition between orbits" with the "observational" notion of a "spectral line," the theory of physical optics is appealed to in order to account for the behavior of light in prisms and telescopes which makes the "spectral line" associable with the "electron transition." Accordingly, the Bohr theory employs, as an auxiliary theory, the theory of physical optics. A view of the theoretical interdependence of science, not dissimilar to Duhem's (1906), is a consequence of such an account of C-sentences.<sup>11</sup>

The O-sentences mentioned above are not meant to be necessarily atheoretical. In many cases they may involve universal concepts, such as numbers, and are best understood as Popper understands his "basic statements"—as provisional but conventionally accepted reports of intersensually testable events.<sup>12</sup>

The concept of an O-sentence introduced here is thus characterized by several definitia which are possibly epistemologically distinct, e.g., such sentences were noted as "describing an intersubjectively testable experience," as "naming an entity which is observationally accessible," and as "reports of intersensually testable events . . ." Though I believe that these various notions could well profit by more detailed analysis, it seems that the characterization of O-sentences provided here is sufficiently clear so as to provide the loose type of experimental constraint discussed in the later philosophical and historical sections of this paper.

<sup>11</sup> 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).

<sup>12</sup> Cf. Popper (1959), especially chapter 5.

### 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-

pectations of the theorist, as the results of the Michelson-Morley experiment did not agree with the expectations of Lorentz,<sup>13</sup> 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.<sup>14</sup> 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 theory and 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

<sup>13</sup> 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.

<sup>14</sup> 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.

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 permissibility, *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 permissibility, correctness, and appropriateness could we "attain the possibility of modifying and improving our images."

The concept of permissibility 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 permissibility Hertz wrote:<sup>15</sup>

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.

<sup>15</sup> 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,<sup>16</sup> 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

<sup>16</sup> See Sommerfeld (1952) for a critique, though, of Hertz’s account of the Newtonian-Lagrangian image.

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.<sup>17</sup>

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

<sup>17</sup> This “theoretical context” is similar to what other philosophers have termed the “background knowledge.” Cf., for example, Shapere (1970).

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  $D$ ?<sup>18</sup>

<sup>18</sup> Of these two difficulties, Feyerabend (1962, 1965) stresses the first, and Kuhn (1962) the second, though neither would disagree with either.

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  $O_a$  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 effectively 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.<sup>19</sup> 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 inertia of 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.

<sup>19</sup> 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.



### 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,<sup>20</sup> 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 1885 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

<sup>20</sup> 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 nontestable, 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.<sup>21</sup>

It is obvious that by making a virtue of simplicity we make a vice of

<sup>21</sup> It could happen that the second and third senses of simplicity distinguished here, i.e., the terminological/ontological and the simplicity of system, collapse into one basic sense. This would be Goodman's (1966) position. I believe, however, that such a reduction to the simplicity of predicate bases would not be very helpful. This belief is partly based on Goodman's approach, which assumes replaceability as a precondition for simplicity orderings. This is what our problem is all about. Secondly, Goodman's criticism, that the simplicity of a system cannot be determined by comparing the number of hypotheses because all axioms can be conjoined into one axiom, has been eliminated by the stipulation that each hypothesis represent an "irreducible physical effect" in the sense that two "irreducible effects" not be known to be derivable from one such effect, nor to be derivable by the detachment of one of the conjuncts from a conjunction. Such a restriction is perhaps ad hoc in the context of logic, but it represents a quite suitable restriction in the context of physics.

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  $T_1$ , to obviate the necessity of accepting a new theory  $T_2$ , which accounts for the E-result *without* H. H, as in the previous sense, is not supposed to entail, in conjunction with  $T_1$ , 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.<sup>22</sup> 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 electrody-

<sup>22</sup> See Schaffner (1969a, 1970), Goldberg (1969b), and McCormach (1970 and unpublished) for support for this claim.

namics 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.<sup>23</sup>

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.<sup>24</sup>

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.<sup>25</sup> 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.

<sup>23</sup> See Poincaré (1901), p. ii.

<sup>24</sup> See Jammer (1961), McCormach (unpublished), and Goldberg (1970b) for discussions of the electromagnetic view of nature.

<sup>25</sup> See Goldberg (1969a) and also his (1970a) for indications of the difference in the rate of acceptance of relativity by country.

## VI. The Lorentz "Absolute" Theory

## 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.<sup>26</sup> 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.<sup>27</sup> 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.<sup>28</sup> 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:

$$\begin{aligned} (1) \quad & \text{div } \mathbf{d} = 0 \\ (2) \quad & \text{div } \mathbf{h} = 0 \\ (3) \quad & \text{curl } \mathbf{h} = \frac{1}{c} \frac{\partial \mathbf{d}}{\partial t} \end{aligned}$$

<sup>26</sup> See Schaffner (1969a, 1970), Goldberg (1969b), Hirose (1962, 1966), and McCormach (1970 and unpublished) for more details on the genesis of the electron theory of Lorentz.

<sup>27</sup> This analysis is based on Lorentz's (1904b, 1909) works.

<sup>28</sup> 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

$$(4) \quad \text{curl } \mathbf{d} = -\frac{1}{c} \frac{\partial \mathbf{h}}{\partial t}$$

where  $\mathbf{d}$  is the dielectric displacement,  $\mathbf{h}$  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 Stoney'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 0."<sup>29</sup> 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:

$$(1)^* \quad \text{div } \mathbf{d} = \rho$$

and in place of (3) we substitute:

$$(3)^* \quad \text{curl } \mathbf{h} = \frac{1}{c} \left( \frac{\partial \mathbf{d}}{\partial t} + \rho \mathbf{v} \right)$$

where  $\mathbf{v}$  is the absolute velocity of the charge. Lorentz also adds a fifth equation:

$$(5)^* \quad \mathbf{f} = \mathbf{d} + \frac{1}{c} (\mathbf{v} \times \mathbf{h})$$

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.

apparent failure of Newton's third law in his electron theory. See Lorentz (1904a, b, and 1909) for discussion of electromagnetic momentum.

<sup>29</sup> Lorentz (1909), p. 11.

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.<sup>30</sup> 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.<sup>31</sup> 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.<sup>32</sup> 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

<sup>30</sup> Cf. Lorentz (1909), pp. 62ff.

<sup>31</sup> See Schaffner (1969a) and especially (1970), chapters 3 and 6, for a discussion of the Fresnel partial dragging or convection coefficient.

<sup>32</sup> See same references as cited in note 31 for a discussion of the Michelson-Morley experiment

the time scale was assumed to be unchanged.”<sup>33</sup> 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,”  $x_r = x - vt$ , to these fundamental equations, and then superadded another change of variables:

$$(6) \quad x' = \beta l x_r$$

$$(7) \quad y' = l y_r$$

$$(8) \quad z' = l z_r$$

$$(9) \quad t' = \frac{1}{\beta} t - \beta l \frac{v}{c^2} x_r$$

where

$$\beta = \sqrt{\frac{c^2}{c^2 - v^2}}$$

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.<sup>34</sup>

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.

<sup>33</sup> The partial quotation from Lorentz is from his (1915), p. 321, the Pauli quote is from his (1958; originally published in 1921), p. 1.

<sup>34</sup> 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).

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

Transformations for  $\mathbf{d}$  and  $\mathbf{h}$  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:

$$(10) \quad \rho' = \rho/\beta l^3$$

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

$$(11) \quad u_x' = u_x\beta^2 \quad u_y' = \beta u_y \quad u_z' = \beta u_z$$

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.<sup>35</sup> 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

<sup>35</sup> Holton (1960) lists 11 ad hoc hypotheses in the (1904b) Lorentz paper, and Goldberg (1969b) concurs in this assessment.

able to obtain the value of the undetermined  $l$  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 seat 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.<sup>36</sup> 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.<sup>37</sup> Einstein also felt that Lorentz's theory, with its ether rest frame, implied the existence of asymmetries in

<sup>36</sup> Einstein knew of Lorentz's (1892a) and (1895) papers, but not of his later modifications. See Born (1956) for a letter from Einstein stating this.

<sup>37</sup> Cf. Einstein (1905a).

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,"<sup>38</sup> 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.<sup>39</sup>

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 veloc-

ities 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.<sup>40</sup>

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

<sup>38</sup> Einstein (1905b).

<sup>39</sup> See Reichenbach (1957) and Grünbaum (1963) for in-depth analyses of the philosophical aspects of Einstein's reanalysis of simultaneity.

<sup>40</sup> 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,<sup>41</sup> 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.<sup>42</sup> The success of thermodynamics as a nonmechanical theory had engendered a vigorous, if rather polemical, energeticist 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.<sup>43</sup>

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.<sup>44</sup> 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

<sup>41</sup> The relation of Maxwell's theory to mechanics is discussed extensively in my (1970), chapters 4 and 5.

<sup>42</sup> See Jammer (1961), McCormach (unpublished), and Goldberg (1970b).

<sup>43</sup> Important discussions of the theoretical context at this time are given by Holton (1967–68), by Klein (1967) and, of course, in the most important autobiographical notes of Einstein (1949).

<sup>44</sup> See note 28, and especially see Lorentz (1909), pp. 30–32.

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.<sup>45</sup>

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-

<sup>45</sup> Born (1956), pp. 193, 195.



netic 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 time  $t'$  must be regarded as no more than an auxiliary mathematical quantity. In Einstein's theory, on the contrary,  $t'$  plays the same part as  $t$ .<sup>46</sup>

Similar resistance to the revolutionary analysis of time is to be found in the willingness of some scientists of that period to give up Maxwell's equations if traditional kinematics could be maintained. W. Ritz's ballistic theory was such an attempt. W. Pauli (1958) commented on the motives behind the Ritz theory, writing:

The constancy of the velocity of light, in combination with the relativity principle, leads to a new concept of time. For this reason W. Ritz, and independently, Tolman, Kunz, and Comstock have raised the following question: Could one not avoid such radical deductions and yet retain agreement with experiment, by rejecting the constancy of the velocity of light and retaining only the first postulate [i.e., relativity]? It is clear that one would have to abandon not only the idea of the existence of an aether but also Maxwell's equations for the vacuum, so that the whole of electrodynamics would have to be constructed anew. Only Ritz has succeeded in doing this in a systematic theory.<sup>47</sup>

The conclusion which can be drawn from these examples is that, with respect to a decision made in 1905 on the *theoretical context sufficiency* of the Lorentz and the Einstein theories, Lorentz's was clearly favored. It not only retained the ether, but also retained Newtonian kinematics, and did not, therefore, contradict in the same fundamental manner the Newtonian dynamics which still appeared, to most, to be a fundamentally sound and still viable theory.

I shall defer consideration of the comparative "experimental adequacy" of the Einstein and Lorentz theories for a moment and shall turn to an examination of the third of the categories of the logic of comparative theory evaluation.

### B. Simplicity

On the grounds of fitness and of ontological and system simplicity, Einstein's theory was clearly better than Lorentz's.<sup>48</sup> Even before Einstein's theory was available as a contrasting approach, Poincaré had criticized Lorentz's pre-1904 attempts at a theory of the electrodynamics of moving

bodies, noting that Lorentz seemed to require a new hypothesis for his theory each time a new experiment was done.<sup>49</sup> The Lorentz theory had to postulate in an unconnected manner, and in a way that increased the ad hoc character of the postulates, many special transformations of electromagnetic parameters. Special postulates for electron contraction, velocity addition, charge density modification, and the like, as were discussed above, were added in piecemeal fashion in attempts to outflank the results of *prima facie* falsifying experiments. In contrast, Einstein's theory began with two postulates and after deductively developing the various kinematical transformations, was applied to the fundamental equations of the Maxwell and Lorentz theories, resulting in the derivation of the electro-dynamical transformations which Lorentz had to postulate. With respect to system simplicity, then, Einstein's theory was better than Lorentz's.

This was acknowledged by the scientists of the time. M. Laue (1911), in the first textbook on the special theory of relativity, wrote: "Though a true experimental decision between the theory of Lorentz and the theory of relativity is indeed not to be gained, and that the former, in spite of this, has receded into the background, is chiefly due to the fact that, close as it comes to the theory of relativity, it still lacks the great simple universal principle, the possession of which lends the theory of relativity . . . an imposing appearance."<sup>50</sup> Lorentz himself, as quoted above on page 342, also acknowledged that Einstein's theory had a "simplicity that I had not been able to attain."

Einstein's theory is also *ontologically* simpler than Lorentz's inasmuch as it dispenses with that strange entity, the ether. In Lorentz's theory the ether still has the role of the special framework for Maxwell's and Lorentz's equations, and it apparently also exerts causal influences, such as the contraction phenomena. In Einstein's theory, however, as in Lorentz's recasting of his own theory in the last few pages of his (1909) monograph, such contractions are reciprocal for two reference frames moving with a constant velocity, and accordingly cannot be ascribed to the asymmetrical ether rest frame.

With respect to fitness, Einstein's theory is also in a better position than is Lorentz's. Though the potential scope of Einstein's theory was considerably greater than Lorentz's theory, Einstein's theory did not introduce distinctions at the theoretical level which clearly had no further empirical

<sup>46</sup> Lorentz (1915), p. 321.

<sup>47</sup> Pauli (1958), pp. 5-6.

<sup>48</sup> This is a point which is heavily stressed by Holton (1960), though he uses a different analysis of simplicity than the modified Hertzian analysis presented here.

<sup>49</sup> Cf. Poincaré (1900).

<sup>50</sup> Cf. Laue (1911), as quoted and translated in Cassirer (1923), p. 376.

consequences associated with them. Both Maxwell's theory and Lorentz's analysis make a distinction between the case of a magnet stationary in the ether and a circuit moving near it, and the case of a circuit stationary in the ether with the magnet moving through the ether. These theoretically distinct states of affairs have the same observable consequences, and Einstein, at the very beginning of his fundamental (1905b) paper, called attention to these "asymmetries which do not appear to be inherent in the phenomena" to make his own symmetrical theory more plausible. There are other asymmetrical effects which Lorentz's theory permits, but which have no experimental support. Lorentz apparently thought, until about 1910, that observations on Jupiter's moons might reveal an ether wind effect. Pauli (1958) commented on this unrealized hope, writing:

In Lorentz's theory it might be possible to obtain first-order effects of the "aether wind" by considering gravitation. Thus, as mentioned by Maxwell, the motion of the solar system relative to the aether would produce first order differences in the times of the eclipses of the Jupiter satellites; but it was found by C. V. Burton (*Phil. Mag.* 19 (1910), 417; cf. also H. A. Lorentz . . . (1914), p. 21) that the inherent observational errors would be as large as the expected magnitude of the effect. Observations on the satellites would therefore not help in deciding for or against the old aether theory.<sup>51</sup>

Einstein's theory denied the possibility of any asymmetrical "ether wind" effects, and as part of the systematic consequences of the theory, obtained a set of consistent transformations which allowed full covariance. Lorentz only obtained approximate covariance using his own transformations, and the departures from full covariance did not have any experimental support, though they were not at the time ruled out by experiments. Clearly, however, a theory that claims the existence of such asymmetries which are not empirically accessible is deficient in *fitness*, as compared with a theory which does not make any such claims.

In regard to what was said earlier about the interaction of the categories, however, we can also see why Lorentz did not feel that the complexities of his theory were necessarily a poor feature. In Lorentz's judgment, the ether was still a viable concept and not one to be easily relinquished. In terms of the theoretical context of this judgment, it is a legitimate one. Moreover, Lorentz saw good reasons for wishing to retain the eminently successful Newtonian kinematics. In the light of such judgments, the ontological simplicity of a theory without the ether was not necessarily desirable.

<sup>51</sup> Pauli (1958), p. 1, n. 3.

Lorentz also seemed willing to tolerate a certain amount of complexity and ad hoc hypotheses in his theory if he could maintain the ether and traditional kinematics. From his point of view, certainly until about 1910, one could rationally anticipate experimental effects of the ether wind. After this time, Lorentz still seemed to hope for some unexpected but possible ether wind effect; at least he wished to keep the possibility open. To the extent that he did this, he did not accept relativity.

Lorentz could appreciate the simplicity of the Einstein theory over his own theory, and in fact, as we saw, admitted such simplicity. Nevertheless he felt that conflicts with the theoretical context were too great to permit him to accept Einstein's theory.

In spite of the existence of an interfering influence of judgments based on other categories, we can conclude that Einstein's theory enjoyed a considerable advantage with respect to simplicity, as displayed in its higher degree of fitness, its less complex ontology, and its higher degree of systematic interrelation of transformation equations.

### C. *Experimental Adequacy*

A determination of the relative merits of the Einstein and Lorentz theories in regard to experimental adequacy is rather straightforward, though an account of how it can be so involves the complex issues of the interaction of theory and observation discussed earlier. First it should be noted that the consensus view of physicists in the years 1905–15 was that no experimental evidence was available which would permit a clear choice between the Lorentz and the Einstein theories. (See the quotation from Laue (1911) above.) This was in interesting contrast to Hertz's theory of the electrodynamics of moving bodies which had been eliminated by its failure to account for the Fresnel partial dragging coefficient and thus for Fizeau's (1851) moving water experiment. Ritz's theory also was eliminated on experimental grounds, principally by de Sitter's (1913) experiment on the light from double stars, but partly also because no natural explanation of the Fresnel coefficient was forthcoming from Ritz's theory.<sup>52</sup> Experiments with clearly associated asymmetrical ether wind effects would have decided for Lorentz's theory against Einstein's, but as pointed out above, the lack of such experiments did not *falsify* the Lorentz theory; it only diminished its fitness.

Mutual experimental adequacy, however, does not imply that both

<sup>52</sup> See Pauli (1958), pp. 5–9, and also Panofsky and Phillips (1955), chapter 14, for a discussion of the experimental inadequacy of the Ritz theory.

theories account for the results of experiments in the same way. On the contrary, the Lorentz analysis and the Einstein analysis of the Michelson-Morley experiment, and most other experiments associated with the theories, involve rather different and incompatible analyses of what the light rays and the instruments are purportedly doing when readings are being taken. This is not to deny the thesis developed earlier that there is overlap between the theories: both the Lorentz and the Einstein theories make contact with experience by using observation terms which have identical primary senses. Moreover, both theories even overlap to some degree in more theoretical sectors. Both utilize Maxwell's and Lorentz's equations for bodies at rest, though the two theories diverge in that Lorentz ultimately associates his equations with an ether and an absolute reference frame, whereas Einstein does neither. Both theories also find that the behavior of reflected intersecting orthogonal light rays, such as exemplified in the Michelson-Morley experiment, is relevant as a test of their contentions about electromagnetic phenomena associated with moving bodies. It is the overlap of the observation terms, and the ability of both absolutist and relativist to agree about the primary sense of the observation terms and O-sentences, that permits both theorists to use the Michelson-Morley experimental results.

Let us also refer to a rather different experiment, the one mentioned above which was responsible for the elimination of the Hertz theory, and also for suspicion concerning the Ritz theory. Lorentz, as was mentioned in section VI, formulated an explanation of the Fizeau experiment by finding a way to derive the Fresnel partial dragging coefficient from the equations of his electron theory (Lorentz, 1892a). The Lorentz explanation involved physical hypotheses concerning the interaction of light in the moving water (in the Fizeau experiment) with the electrons of the water, and the reradiation of light waves by these electrons. The net result is a light wave which moves with a velocity of  $c/n + v(1 - 1/n^2)$ , where  $c$  is the velocity of light in a vacuum,  $n$  is the absolute index of refraction of the water, and  $v$  is the absolute velocity of the water. In Lorentz's derivation, he used standard Newtonian kinematics in adding velocities.

Einstein himself did not offer an explanation for the partial dragging coefficient, but M. Laue (1907) was able to provide one by showing that the required observed effects followed simply from the relativistic velocity addition theorem. In this explanation, *partial* drag was not assumed; the moving water completely dragged the light with it. The velocity of the

light in the water and the velocity of the water, however, added relativistically so as to give the appearance of a partial dragging effect. Again, in carrying out the Fizeau (1851) experiment or the later Michelson-Morley (1886) more precise version of the experiment, both a relativist and an absolutist would agree on the primary sense of the observation sentences reporting the results of the experiment. In this experiment there is a positive fringe shift of measurable amount. The two theories would, however, as this account of O-sentences permits, take the O-sentences as evidence for, or indexes of, quite different aspects of nature's regularity.<sup>53</sup>

Accordingly, we see in connection with our example how it is possible for two inconsistent and perhaps incommensurable theories to have common points of contact with experiment.

With regard to the question of the *relevance* of experiments vis-à-vis the issue of experimental control over theory acceptance, one can, I believe, conclude that if Einstein's theory had been unable to account for the Fizeau experiment and the "partial" dragging coefficient, Lorentz's theory would have ranked over it as regards experimental adequacy. For the Einstein theory asserts itself to be a theory of the electrodynamics and optics of moving bodies, and it gives no reasons why it should not account for first-order experiments in this domain. Thus the Einstein theory (taken together with the theoretical context of optics) informs a physicist that such an experiment is relevant. Lorentz's theory does the same, and accordingly, even though the two theories disagree on what is occurring in the experimental device, the experiment does constitute a common standard for the judgment of the competing theories' experimental adequacy.

In summary, then, the three categories of comparative theory evaluation which we have been considering yield a split decision in, say, 1905-7: theoretical context sufficiency supports Lorentz's theory, relative simplicity supports Einstein's, and experimental adequacy, because of the flexibility of the interpretation of "observation" statements, selects neither theory as the better one.

But science was not a static enterprise in these years, and judgments made in 1905 are not quite what they would be were they made in 1906 or 1907. In 1906 M. Planck (1906) reanalyzed Einstein's suggestions concerning slowly accelerated electrons, a topic to which Einstein had turned his attention at the very end of his (1905b) paper. Planck was able to show that a simpler form of the mass dependency on velocity would follow if a

<sup>53</sup> Cf. Kuhn (1962), p. 129, from whom this sentence is closely paraphrased.

different, and entirely plausible, definition of force were employed. In 1907, Minkowski (1908) worked out a revision of mechanics in an Appendix to a systematic representation of relativistic electrodynamics. In this Appendix Minkowski referred to the contradictions in the relation of the special theory of relativity to the theoretical context (especially with Newtonian mechanics) when he noted that many authors were arguing that “classical mechanics contradicts the postulate of relativity which is here taken as the basis of electrodynamics.” Minkowski went on to assert that “it would be extremely unsatisfactory if the new conception of time should be valid only in a particular branch of physics.”<sup>54</sup>

Minkowski’s reformulation of mechanics, which now made mechanics agree with the special theory of relativity, was subsequently followed by Born’s (1909) refinement as well as by Lewis and Tolman’s (1909) famous paper on elastically colliding masses. The latter paper was the first non-electrodynamical derivation of the relativistic mass-velocity effect.

Clearly, then, we have evidence of a change in the theoretical context of Einstein’s special theory of relativity in these four years from 1905 to 1909. The reluctance of physicists to accept Einstein’s theory because of its conflicts with Newtonian kinematics and Newtonian dynamics lessened as mechanics itself was gradually altered so as to agree with the new theory. Other derivations from the relativistic theory followed, one of the most important being the mass-energy relation first proposed by Einstein (1905c) shortly after the publication of his fundamental relativistic paper. Experimental tests of this implication were not available for many years afterwards, however, so that this perhaps most famous implication of relativity theory was not instrumental in affecting its acceptance.

What seems to be responsible for the acceptance of the Einstein theory over the Lorentz theory by, say, 1910 in Germany, and a bit later in other countries, is the complete experimental adequacy of the theory, its high degree of simplicity, and the revision in the theoretical context caused largely by itself. Such a revision took time, as did the derivation of further consequences from the Einstein theory. Nevertheless it does appear that many scientists did relinquish the Lorentz theory in favor of Einstein’s, and the reasons which are often given appear to agree with the analysis of the logic of comparative theory evaluation sketched in these pages. There were, to be sure, some physicists who did not relinquish the older ether theories. Apparently they did not do so because they saw no clear experi-

mental reasons for doing so, and they could comprehend neither a physics of fields without an ether nor a mechanics without absolute simultaneity. But such physicists represent another instance of the “subjective fluctuations” from the consensus of physics, fluctuations which here indicate not that theory change is irrational, but rather that some physicists are irrational.

Accordingly, we see that judgments made using these categories of comparative theory evaluation are themselves dependent on what experiments have been done and what revisions in theory scientists have attempted. But such a relativization does not imply the subjectivity of different scientific schools which some of the historical philosophers of science have suggested. Rather the time dependency of the assessment of strengths of different and competing scientific theories seems to reflect the dynamism and actual progress of science through history.

#### IX. Implications of the Above Tricategorical Analysis for the Relation of the History and Philosophy of Science

Let us now consider whether the philosophical analysis sketched here might admit of application by the historians of science, and in particular, how the analysis might clarify the controversy in the history of recent physics over who had priority in the development of the special theory of relativity. The controversy arose when Whittaker (1953) argued that the theory of relativity really belonged to Lorentz and Poincaré, and that Einstein had simply made some minor contributions to the subject.<sup>55</sup>

It should be clear that if the antecedent theoretical meaning of the terms of the two theories is taken cognizance of there are certainly two different types of “relativity”: Lorentz’s theory maintains *absolute* velocities, Newtonian universal time, an ether, and fundamental asymmetries; Einstein’s theory argues for *relative* velocities, a new time concept which is incompatible with Newtonian kinematics, repudiation of the ether, and a fundamental symmetry of all transformations. Thus attention to the antecedent theoretical meaning of the two theories already discloses a number of basic differences between the two theories.

If we now apply the tricategorical logic of comparative theory evaluation to the two theories, and of course I shall now only summarize what has been argued in more detail earlier, further differences between the two theories are brought forth. The conflicts between the theoretical context

<sup>54</sup> Minkowski (1908), as translated and quoted in Hirose (1968), p. 47.

<sup>55</sup> Cf. Born (1956), Holton (1960), and Grünbaum (1961) for earlier critiques of Whittaker’s position.

and the Lorentz theory were not nonexistent, but they were less fundamental than were the conflicts between the accepted theories of the time and Einstein's theory. Lorentz's theory, if extended to all mechanics as Poincaré (1909) suggested, would result in a new mechanics owing to science taking account of new forces and force transformation laws. But the potential conflict with previous mechanics entailed by such an extension was not as immediate and fundamental as that between Einstein's new kinematics and the then accepted theories of physics. Einstein's theory also eliminated the ether, whereas Lorentz's theory did not, and this was at the time a significant difference for many physicists who considered the ether a precondition of the intelligibility of any field theory.

Though both theories were comparable as regards experimental adequacy, they did differ in the ways in which they accounted for the various experiments which supported them. The account of the interrelations of theory and experiment given above in sections II and IV B, and applied in section VIII C, suggests further differences between the Einstein and Lorentz theories. Finally the application of the category of relative simplicity in its dimensions of fitness, ontological simplicity, and system simplicity also indicates some interesting differences between the two theories. The relevance and logical status of Einstein's objections against Maxwell's and Lorentz's theories, on the grounds that they involved asymmetries in the phenomena of induction, are disclosed. Further, the difference between Einstein's full covariance and Lorentz's approximate covariance is best measured according to this logic in terms of the subcategory of relative fitness. (This particular difference does not apply to the relation between Einstein's and Poincaré's modification of Lorentz's theory.) The systematization, or system simplicity, which Einstein achieved in comparison with Lorentz, and the different roles of the principle of relativity—for Einstein a fundamental postulate, for Lorentz an approximately true *theorem*—also indicate differences between the two theories.

With the aid of such philosophical distinctions, as informed with a detailed knowledge of the historical case which is being studied, a historian could perhaps understand better why Whittaker might have made a mistake in attributing "relativity" theory to Lorentz and Poincaré, though such a combination of historical and philosophical knowledge will not account for Whittaker's refusal to acknowledge Einstein's fundamental and revolutionary contributions to the subject.

Though I argue the case for the relevance of philosophical distinctions

and philosophical analyses for the historian of science, I should not want to overreact and claim that the philosopher of science has little *more* to learn from the historian of science than he now knows. Certainly the historical school's philosophical thesis of the irrationality and incommensurability of the relations between historically successive scientific theories is not borne out by the careful study of the relations between the Lorentz and Einstein theories, and in spite of what I find to be the fruitfulness of some of the claims which the historical school makes, I think the claims would have been better founded on a more accurate view of the history of science.

The relation of the Lorentz and Einstein theories as set out here is based on and supported by a number of historical studies. It is hoped that the outlines of the logic of comparative theory evaluation sketched out in these pages might in turn admit of some further application by historians attempting to reconstruct the chronological record of science.

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## COMMENT BY HOWARD STEIN

1. A point of historical-theoretical explication: The issue of action-reaction between electrons and the ether, or of electromagnetic momentum, was in one way (cf. note 18 of my paper in this volume) a stage in the elimination of the ether. For according to Lorentz, as Professor Schaffner has said, the ether is—i.e., all its parts are—immobile. If the ether were a material medium interacting with charged bodies, its parts would receive momentum through such interaction; this is what Maxwell conceived. Lorentz's dilemma was this: if the ether is rigorously immobile, either the third law of motion (i.e., conservation of momentum) fails, or ethereal (i.e., electromagnetic) momentum is unrelated to ethereal velocity. Either constitutes a serious falling-off from the classical notion of a dynamical theory: in Professor Schaffner's terms, a failing in theoretical context sufficiency.

2. In discussing the role of a positivist attitude in Einstein's work, I think it important to distinguish between what might be called critical positivism on the one hand and a kind of blanket epistemological positivism on the other. In the history of science, as it seems to me, critique of a (roughly) positivist or empiricist type has on several occasions been ex-

tremely valuable—and I think the Einstein-Lorentz case is an example; but what has been of exceedingly dubious value is the attempt to describe a certain procedure of concept construction as a requirement on all notions to be introduced into theoretical science. That is to say—and this is a general normative moralistic remark—a concept ought not to be required to exhibit its credentials in order to be entertained in a theory; but when one has a theory, and one examines the mathematical structure of the theory (for example) and shows that a certain notion appearing in it can be eliminated, then one has shown that that notion is devoid of content. This I think is what Einstein did vis-à-vis Lorentz—in that version of the Lorentz theory which is mathematically equivalent to his own. Einstein's critique, eliminating the "velocity vector attached to empty space," of course involved still more fundamentally the time interval between two events as an absolute notion, and in particular the notion of absolute time-interval zero (i.e., absolute simultaneity). Einstein did not simply impugn this notion on epistemological grounds: he showed that the notion could be sacrificed in a certain definite way, without violence to any known facts, and with notably improved justice to some facts. (I say *improved* justice because the version of Lorentz that was equivalent to Einstein had after all not yet been formulated; and although, as Professor Schaffner has said, the issue of experimental adequacy was uncertain, the *elusiveness* of "motion relative to the ether" did already stand as a very notable circumstance.)

Again, as to the elimination of that velocity vector attached to empty space, I think one should not overemphasize the epistemological or ontological importance for Einstein of this elimination per se: he was, after all, perfectly willing to attach the electromagnetic field tensor to empty space. In one version of the controversy about the ether, we do really face a verbal quibble: whether to call the electromagnetic field itself "the ether"—as if it were a serious issue whether one uses that word or not. Setting this verbal question aside, the possibility of vectors or fields or what have you attached to empty space is certainly one that Einstein was happy with: it always formed an essential part of his notion of the structure of physics, and hence of reality. His critique consisted in asking, of a theoretical term or principle, whether it does in fact have an effect—and an effect which contributes positively to the theory's experimental adequacy.

## COMMENT BY ARNOLD KOSLOW

## I. The Three Criteria

Professor Schaffner has generalized Hertz's criteria of *Zulässigkeit* (permissibility), *Richtigkeit* (correctness), and *Zweckmässigkeit* (appropriateness) in order to show how certain theories of the physical sciences may be compared and ranked. His stated aim is to provide criteria which "will resolve some of the paradoxes raised by the historical school of the philosophy of science."

He has very ably described those "paradoxes" and there is no need to rehearse them once more. My first point is that contrary to Professor Schaffner's claim, the criteria introduced by him do not resolve the critical issues raised by the "historical school." These criteria, it seems to me, cannot be applied to the theories in question unless some resolution is made or answer is given to the criticisms of the historical school. The application of Schaffner's criteria do not provide a resolution, they presuppose that one has been given. Perhaps some examples will make the point more clear. Schaffner's parallel for Hertz's *permissibility* is *theoretical context sufficiency*, by which he refers to "the amount of concordance between a theory to be assessed and the corpus of accepted scientific knowledge." Hertz's criterion of permissibility required consistency, and Schaffner's generalized version refers to the concordance of theories. Obviously it will not do to speak of the amount of consistency, but "concordance" and "the amount of concordance" seem to me to require explication if we are to regard theoretical context sufficiency as a serviceable notion. If, however, Schaffner does mean consistency, or something which at least requires consistency, then it seems to me that he has begged those issues raised about the commensurability of theories. When Einstein compared his theory with Lorentz's, he stated that "the introduction of a 'luminous aether' will prove to be superfluous inasmuch as the view here to be developed will not require an 'absolutely stationary space' provided with special properties, nor assign a velocity vector to a point of the empty space in which electromagnetic processes take place."<sup>1</sup> In the context of the quotation, Einstein uses the terms "material point," "body," "velocity-vector," and "electromagnetic process." However, if certain members of the "historical school" are correct, the terms mentioned above have different meanings, even though they are shared by both theories. Indeed, in one

passage, Schaffner indicates that there are important differences of meaning for the term "velocity" in both theories. I do not wish to defend these theses of the historical school because I think that most of them are false or ill-grounded. But I think that a simple application of the criterion of theoretical context sufficiency does not resolve the basic problems or paradoxes of that school.

The same kind of point can be raised against Schaffner's use of the criteria of experimental adequacy and appropriateness or simplicity. If we take the puzzles of the historical school seriously, then according to some of its members, all the terms of different theories (relative of course to some criteria for individuating theories which have so far remained unspecified) will differ. In particular, "body" will have different meanings in the theories of Lorentz and Einstein, and the purportedly contrasting assertions about the way bodies behave may not contrast at all. (I say "may," since two sentences sharing a term may be contradictory even though the term occurs in each sentence with a different meaning; indeed they may be contradictory even though they have no term with the same meaning in common.) All the problems central to the historical school—the issues of meaning variance and the consequent incommensurability of theories—are untouched by Schaffner's criteria. These issues would first have to be settled if the criteria are to be applied at all.

I find it surprising that one could resolve the paradoxes of the historical school by granting, "absorbing," and not contradicting any of that school's major theses. It does sound as if Schaffner is saying that with suitable adjustments—presumably acceptable to that school, but maybe not—he can settle some of their outstanding paradoxes. It is hard to understand how Schaffner can accept, absorb, and not contradict all the major theses of that school, and still argue, counter to their view, for the comparative evaluation of theories. He does not contradict their logic, their epistemology, or their pronouncements on meaning. He does present some arguments concerning what he calls O-sentences and C-sentences. But from what I can detect, nothing that he says constitutes a challenge to the historical school. I have some difficulty understanding his characterization of the primary and secondary senses of O-sentences partly because there is a shift in his exposition between O-sentences, O-terms, and O-predicates. I take it that Schaffner's main concern in making these distinctions is to show how, given two theories, there could be an intertheoretic common base. But his distinction is far from being a clear one. For example, he con-

<sup>1</sup> *The Principle of Relativity*, trans. W. Perrett and G. G. Jeffery (New York: Dover, 1923), p. 38.



siders two competing theories of optics and introduces  $O_a$ , an O-sentence which describes a fringe pattern on a surface illuminated by a monochromatic beam through a double-slit screen. According to Schaffner, the primary sense of the O-predicate is “the referent [sic], the bandlike appearance one experiences in learning the meaning [sic] of ‘optical fringe phenomena.’” He also states that 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.”

It would appear that not only do certain terms have a *meaning*, they also have primary and secondary senses as well. Schaffner’s distinction of two senses therefore does not obviate all reference to meaning; it depends upon some notion of meaning. Further, I am not convinced that his notion of the primary sense of an O-term does the work he requires of it. He would like to say that different theories such as Lorentz’s and Einstein’s overlap in their observation terms, and that the proponents of those theories agree in the primary sense of those terms. But why should it be the case that two physicists have had the same experience when they *learned* the meaning of a shared O-term? Trivially, one of them could have learned the meaning through pictures, another through slides, another through diagrams, and another by means of glances through the telescope of an interferometer properly set up. There is no special reason why the proponents of the *same* theory would agree on the primary sense of an O-term, as Schaffner has described it. Further, Schaffner claims that it is only with respect to the second sense of an O-sentence that we speak of that sentence as evidence for one theory or another. I assume that Schaffner would also accept the related view that only in the secondary sense of an O-sentence do we speak of wave optics, for example, explaining why the sentence is true. Without further qualification, Schaffner’s description of secondary senses permits as many secondary senses of a specific O-sentence as there are “different” theories which that sentence is “embedded or hooked into through correspondence rules or C-sentences.” Thus if it is true that M’s being evidence for a theory R rests on the relation of a secondary sense of M to T, then we may find that M cannot be evidence for both of two theories T and T’. For in the former case, it is some secondary sense of M that stands in a certain relation to T; in the latter, it is again some secondary sense of M which stands in that relation to T’. If “different” theories T and T’ imply different secondary senses for O-sentences related to the theories by C-sentences, then T and T’ have no common evidential base. According

to a similar argument, different theories can have no explanations in common. Schaffner maintains that both theorists can use the Michelson-Morley experimental results. Thus he writes, “It is the overlap of the observation terms, and the ability of both absolutist and relativist to agree about the primary sense of the observation terms and O-sentences, that permits *both* theorists to use the Michelson-Morley experimental results.” And later, in the same vein, “In this [Michelson-Morley] experiment there is a positive fringe shift of measurable amount. The two theorists would, however, as this account of O-sentences permits, take the O-sentences as evidence for, or indexes of, quite different aspects of nature’s regularity.”

Professor Schaffner states that both theorists can use the Michelson-Morley experimental results. But these results, as he refers to them, are not part of any intertheoretic common base between Lorentz’s and Einstein’s theories. In fact, the results are irrelevant to the support of either theory, for they cannot even be used for evidence. No common description of the experimental results would figure in a record of the support, the evidence, the explanatory or predictive successes of either theory. If we cannot use experimental results such as the outcome of the Michelson-Morley experiments to convince ourselves and others of the merit of a theory, then what use of experiment can Schaffner have in mind?

## II. Ontology: The Ether and Simplicity

Professor Schaffner’s use of relative simplicity raises an interesting question. He includes differences in the ontological commitment of theories under the criterion of simplicity. He seems to think of the repudiation of the ether as an ontological loss for Einstein’s theory, and a gain in relative simplicity. I have some questions about how these two observations are related. Is it supposed to be true that an ontological loss is always a gain in simplicity? I think not. There are the obvious objections that the statements of the theory may become terribly complex with a change in ontology. In Quine’s terminology, ontological shifts may be offset by ideological ones.<sup>2</sup> Moreover there is in the Lorentz-Einstein situation a very special complicating factor. It may count against the simplicity of a theory if it reduces the totality of causal problems under investigation. Galileo

<sup>2</sup> Having said this, I should also add that I think that the general principle put forward by Schaffner is false. It is not true that “a broader and more complex theory will be acceptable over a simpler and narrower theory.” Suppose that T is broader than T’, and more complex. We may *not* prefer T to T’, if T yields very “shallow,” not very detailed answers to a broad spectrum of problems, while T’, though narrower in scope, gives good “in-depth” answers.

reduced the number, according to a well-known story, by claiming that only deviations from inertial motions required causal explanation. Lorentz, and many with him, devoted their energies to a host of problems involving the explanation of electromagnetic processes in terms of the interaction of the ether and charged bodies. Einstein's ontological move, if it was one, implied that a certain class of scientists, Lorentz included, had a good deal less explanatory work on their hands than they supposed. It is therefore not clear to me that an ontological reduction results automatically in a simpler theory. Second, is it so plain that Einstein's maneuver with respect to the ether is best understood as an ontological shift? Has there been a reduction in, say, Quine's sense?<sup>3</sup> As far as I know, there has been no careful investigation which establishes the existence of a suitable proxy function. And I am not convinced that the omission of a singular referring term like "ether" results in an automatic ontological shift downward. One reason for skepticism in this matter has to do with the belief that ontology concerns *kinds* of things, not specific items. It might be objected that the term "is the ether" does represent a "kind of thing" in the minimal sense that it is a general term. But I do not think so, since one consideration relevant here is whether there are principles of individuation. Are there criteria for determining where one ether begins and another leaves off? I think not, since the ether, in the nature of the case, neither begins nor leaves off. A similar argument could be given for the notion of electron in various theories of electrons, and the notion of body in certain nineteenth-century hydromechanical theories of matter. But this is a large problem which cannot be pursued here. All I wish to register here is my skepticism that the removal of a singular referring term from a theory represents an ontological simplification.

The problem is complicated somewhat by Einstein's later "reintroduction" of the ether in 1920.<sup>4</sup> The reintroduction makes it hard to understand the claim, often put forward, that the theory of relativity implies that there is no ether. If the difference is, as Pauli described it in 1921,<sup>5</sup> that Lorentz believed that the ether had mechanical properties, that it

<sup>3</sup> W. V. Quine, "Ontological Reduction and the World of Numbers," in *The Ways of Paradox and Other Essays* (New York: Random House, 1966); also "Ontological Relativity," in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969).

<sup>4</sup> A. Einstein, *Äther und Relativitätstheorie: Rede gehalten an der Reichs-Universität zu Leiden* (Berlin: Springer, 1920).

<sup>5</sup> W. Pauli, *The Theory of Relativity*, trans. G. Field (Oxford: Pergamon, 1958; original German publication 1921), p. 4.

was a *substance*, but Einstein did not, then it is not apparent that the difference is a simple one over the existence of the ether. It looks as if the difference concerns the properties of the ether, not whether the ether exists. Einstein's later view of the matter is surely not an irrelevant consideration. In *Äther und Relativitätstheorie* (1920) he plays down the idea that the special theory of relativity differs in ontology from Lorentz's theory of 1909. He offers instead, a picture of the continuous development of physical theory which, in its latest phase, involved programs intended to reduce electromagnetic theory to mechanics, and the reverse program of the believers in an electromagnetic world view. Hertz's theory, according to Einstein, was experimentally inadequate and the Hertzian ether as the bearer of both mechanical properties and electromagnetic fields was basically no different from the ponderable matter in it. Einstein argued that Lorentz's great contribution was his bringing of theory into accord with experiment by a remarkable simplification of the theoretical foundations. He says that Lorentz "erzielte diesen wichtigsten Fortschritt der Elektrizitätstheorie seit Maxwell, indem er dem Äther seine mechanischen, der Materie ihre elektromagnetischen Qualitäten wegnahm." Einstein thought that Lorentz left the ether with only one mechanical property, namely, immobility, and that the entire change which the special theory of relativity brought to the conception of the ether was that it denied even that quality to the ether. The concept of the ether, however, was not banished from theory in this view, but all hope for the program of reducing electromagnetics to mechanics vanished once the ether was denied any mechanical properties. And the proponents of electromagnetic world views no longer had to worry that electromagnetic theory would always be parasitic upon mechanics. For it now looked as if it was not true that states of matter were states of a something which has some mechanical properties. No wonder, then, that for these and other reasons, backers of electromagnetic world views could regard Einstein (after a while) as one of their own. Einstein's picture of the sequence of theories formulated by Maxwell, Hertz, Lorentz, and himself is one which, in one dimension, is marked by a continuous development. This is not of course to deny the revolutionary character of his theory: the special theory of relativity as Einstein described it in 1920 was both revolutionary and traditional. His reference to the concept of the ether was one way in which he described the tradition to which the special theory belonged. It should be noted that in a lighter vein, Philipp Frank once said that at a Solvay Congress, Ein-

stein was particularly disturbed by the arguments which many younger physicists were using: if the effect cannot be detected, then it does not exist. When Frank reminded Einstein that these arguments were only variants of Einstein's, he replied: "Yes. But a good joke shouldn't be repeated too often." Despite the vagueness of some of the preceding remarks, it seems clear that in the absence of careful representations of the theories under consideration, and without any fairly definite criterion for the ontological commitment of a physical theory, the claim that there is an ontological difference between the theories of Lorentz and Einstein is not yet a result which ought to be taken for granted.

### III. Schaffner's Criteria and the Ranking of Theories

Aside from problems related to the application of Schaffner's criteria, there are some questions about the role he thinks they have in the evaluation of theories. I think that he does not wish to say that *theoretical context sufficiency*, *experimental adequacy*, and *simplicity* are relevant only to the Lorentz-Einstein discussion. He intends that they be relevant to all cases of theory evaluation and choice, and the Lorentz-Einstein discussion is used to illustrate how his criteria work. But how do they function? What is Schaffner's view about the way in which these criteria are related to the choices scientists make, or even the preferences that they have between theories? If a scientist prefers theory T to T', must T be ranked over T' by all of the three criteria, one, or none? Are the three criteria jointly necessary and sufficient for T to be preferred over T', or only sufficient? Further, there is a view upon which the criteria may be neither necessary nor sufficient, taken singly or jointly. It is the view that a scientist's preferences are set or determined by some combination or "weighting" of the three criteria. I shall call this view *combinatorial*. It seems to me that Professor Schaffner has not clarified how the preference between theories is related to the three criteria he has isolated. There is an additional problem which is not settled by a mere listing of criteria, no matter how carefully described. When a list of relevant criteria is given, is it intended that these criteria will be the only ones which enter into or are relevant to a preference, or that these are the only central or "dominant" criteria? Is it excluded that, over time, the list may be altered, new criteria introduced and old ones dropped? If Schaffner's view is combinatorial, then does he allow that even though the criteria might be the same over time, the "weighting" might vary? If the weighting can shift over time, can it also shift over

people—can different members of a scientific community employ significantly different combinations of shared criteria? These, it seems to me, are some of the important questions for a logic of comparative theory evaluation; they cannot be settled by a careful characterization of the criteria which enter into such a logic.

Consider Professor Schaffner's summation toward the end of his paper, "In summary, then, the three categories of comparative theory evaluation which we have been considering yield a split decision, in, say, 1905–7: theoretical context sufficiency supports Lorentz's theory, relative simplicity supports Einstein's, and experimental adequacy, because of the flexibility of the interpretation of 'observation' statements, selects neither theory as the better one."

Professor Schaffner calls this situation a "split decision." But who is split on this? Certainly not Lorentz, and certainly not Einstein. For each, even in the light of these rankings, preferred his own theory over the other, at the time in question. On a combinatorial view, these remarks seem to show that if Lorentz and Einstein thought that the only relevant considerations are the three introduced by Schaffner, then they weighted the criteria differently; sufficiently so that their preferences were the reverse of each other. Schaffner's researches, coupled with some combinatorial view of theory evaluation, yield interesting conclusions about the way in which scientists aggregate or weigh the agreed-upon relevant criteria.

If it is not Lorentz's split decision, and not Einstein's, then it seems that it is ours or Schaffner's, or possibly some contemporary of Einstein's. Why is the decision split? This is not evident just from the tally which Schaffner offers. On one score (experimental adequacy), neither theory at the time outranks the other. The other two criteria give high marks, one to each theory. Schematically, we might construct a table:

	<i>Lorentz</i>	<i>Einstein</i>
Theoretical context sufficiency .....	+	–
Experimental adequacy .....	=	=
Relative simplicity .....	–	+

where it is supposed that Lorentz, Einstein, and others who had to make a decision would agree that this table represents the results of the three comparisons at the time in question. Why does Schaffner describe his result as a split decision? Is it because each theory has held its own, or done better, on two of the three rankings, or is it split because neither theory

has the advantage with respect to experimental adequacy, and an advantage in theoretical context sufficiency cancels a disadvantage in relative simplicity? Schaffner has described some of the criteria which practicing scientists take into account in making their preferences between theories. He has not described the preference pattern of scientists. I should like to say that he has described the components upon which preferences depend, but he has not described how the total preference of working scientists depends upon those components. To that extent, his logic of comparative theory evaluation falls short of the stated task.

COMMENT BY PETER A. BOWMAN

Professor Schaffner evidently assigns equal weight to the three categories of comparative theory evaluation, but one can question whether that is justified. Surely, practicing scientists give more weight to a theory's being experimentally adequate than to its being theoretical context sufficient. We see a clear example of this in the case at hand through 1905, when Lorentz and Poincaré are going to great lengths to modify the theory of electrons so as to be able to incorporate the results of the Michelson-Morley and other second-order experiments; i.e., they adjust the theoretical context to fit experimental results. Further, it would seem that consideration of relative simplicity comes into play only if considerations of the other two categories prove indecisive. For example, in 1915 Lorentz can only consider the simplicity of Einstein's approach if it is given that (a) there is little chance of an experiment's revealing an asymmetry between the ether frame and the moving frame; thus, it is unlikely that any experiment will show Lorentz's theory to be more adequate than Einstein's; (b) the concomitant development of atomic theory renders risky at best the sort of basic assumptions about the constitution of matter made in the theory of electrons; thus, it is no longer desirable to make the detailed theoretical assumptions about the electronic context of electrodynamics which the latter theory, but not Einstein's, does.

Therefore, we are forced, I think, to assign priority to Professor Schaffner's categories in the following order: (1) experimental adequacy, (2) theoretical context sufficiency, (3) relative simplicity. Then, assuming that his assessment of how the two rival theories compare in each category is correct, we have to conclude that, considerations as to (1) being ambivalent in 1905, a scientist would be justified in accepting Lorentz's theory on the basis of (2) vis-à-vis Einstein's theory on the basis of (3).

REPLY BY KENNETH F. SCHAFFNER

In replying to the comments on my paper, I think that it would be best to begin with Professor Stein's contribution, which I consider amplificatory of my own position, and then turn to Professors Koslow's and Bowman's comments, which are more critical.

Professor Stein makes two points, both of which I agree with. His comments about the failure of Lorentz's theory to agree in toto with Newtonian mechanics concern something which I drew attention to in my paper, though I did not put it in quite the way Stein does. Stein's second point is indirectly concerned with my views on "antecedent theoretical meaning," but it is more explicitly relevant to a discussion by Professors McMullin, Cohen, Buchdahl, and Feigl, which ensued after the paper was presented, and which revolved around the extent to which the positivistic and operationalistic attitude of Einstein was responsible for the special theory of relativity, with its denial of the ether.

Apropos of the notion of antecedent theoretical meaning which was advanced in my paper in section II, and earlier elsewhere (Schaffner, 1969b), one can say, in part, that the ether was *prima facie* scientifically meaningful by virtue of (1) the antecedently understood properties definitionally associated with it, and (2) the hypothetical characteristics imputed to the ether in terms of these and other antecedently understood properties. For Lorentz, the stationary ether possessed experimental consequences, sentences expressing these consequences being derivable from antecedently meaningful statements concerning the ether, the electromagnetic field and electrons, and parts of associated theories, such as mechanics. Lorentz believed that Einstein, by accepting a principle of relativity, eliminated the *possibility* of the ether possessing experimentally accessible consequences; it was not the case that the ether had *necessarily*, by 1905, been stripped of its consequences by experiments such as that of Michelson and Morley. I went to some lengths to indicate what the situation in 1905 was as regards the ether and experiments in my paper, and do not think it necessary to rehearse that material again.

In the unreproduced discussion which followed my paper, Professor Buchdahl suggested that "Einstein . . . believed the suggestion of a velocity vector with respect to the ether *had become meaningless and must be given up* [my italics] and wasn't *this* what was viewed by the positivists as being the antithesis of an ontological [or physical realist's] position?" Professor Feigl supported this interpretation, historically, mentioning that

the Vienna Circle, including Schlick, Carnap, and Reichenbach, and in this country Bridgman and C. I. Lewis, believed that the last-ditch defense of the ether (in 1904 and beyond) by Lorentz and Larmor, for example, had made the ether *in principle* immune to the verification or falsification of its existence, and that, accordingly, its elimination by Einstein “fell under the critique of our meaning criterion” (from Professor Feigl’s comment).

From the point of view of the history of philosophy this is apparently the case, but it seems to me that the positivists’ position is not defensible as a reconstruction of the history of early twentieth-century physics. My position is based on the comments which I made in the previous paragraph concerning Lorentz’s conceptions of the ether and the principle of relativity. Similar comments were more extensively presented in my paper. Lorentz, himself, did not think that the ether’s effects had been forever masked; had they *in principle* been so masked, he would have been quite willing to relinquish the ether.

Accordingly, though I cannot subscribe to what Professor Stein refers to as a “blanket epistemological positivism” because of my views concerning antecedent theoretical meaning and associated metascientific notions, I would agree with his defense of a “critical positivism,” and in fact, think that my subcategory of “fitness” incorporates this “critical positivism” within the analysis presented in my paper.

I must now turn to consider the interesting and incisive comments of Professor Koslow. I shall also reply to Mr. Bowman’s comments in the context of my rejoinder to Koslow.

Koslow criticizes several of my theses. First, he contends that my generalized Hertzian criteria for the ranking of competing scientific theories cannot be applied without *first* resolving the paradoxes concerning the relation between theories and between theories and experiments which have been articulated by the historical school. Koslow writes that “the application of Schaffner’s criteria do not *provide* a resolution; they presuppose that one has been given.” He seems persuaded that it is impossible both to accept *many* of the insights of the historical school concerning the epistemology of science and at the same time to discover theory-transcendent criteria. Koslow elaborates on his critique, arguing that my acceptance of some meaning variance of important scientific terms in competing scientific theories raises the possibility that “purportedly contrasting assertions about the way . . . [entities] behave may not contrast at all,” if the sense

of the concepts concerning the entities changes. Koslow also does not seem persuaded that my rather different analysis of theory and observation—different from the historical school’s account, that is—will do the task it is intended to do, since the distinction which I make between the “primary” and “secondary” senses of “observation” terms is, according to him, both (1) too weak, as it does not even require that “proponents of the same theory would agree on the primary sense of an O-term,” and (2) too strong, as it is only by virtue of the secondary, more theoretical and non-common sense, that the observation term is characterizable as evidence for some theory.

Let me reply to these criticisms before going on to Koslow’s other points.

First, it does not seem to me that the acceptance of aspects of the historical school’s epistemology necessarily entails that theory-transcendent criteria cannot be provided. It is true that both Kuhn and Feyerabend do think that this is so, but this is no reason for accepting what they consider is the consequence of their epistemologies. Let me show how Koslow’s more specific difficulties in this regard are met and outflanked by the meta-scientific analysis presented in my paper.

Koslow is hesitant on his point that the acceptance of meaning variance may render the theories under question incapable of clashing with the theoretical context and, though this is not quite as important, with each other. He notes that “two sentences sharing a term may be contradictory even though the term occurs in each sentence with a different meaning.”

I think that Koslow is correct in his hesitation on this point, because it does appear that this conflict occurs. As I understand the historical school’s thesis of meaning variance, the meaning of a term *t*, such as “velocity” or “mass,” changes when it is understood as a part of theory  $T_1$  rather than  $T_2$ , if and only if those characteristics which are definitionally true for the term are incompatibly different in  $T_1$  and  $T_2$ . There are apparently, in all cases considered by authors such as Kuhn and Feyerabend, sufficiently common concepts or categories which can be taken as a common base to demonstrate this incompatibility.<sup>1</sup>

That conflict between the theoretical context and a new theory does occur in Feyerabend’s analysis is indicated by his remark that “it is . . . impossible to *define* the exact classical concepts in relativistic terms or to

<sup>1</sup> References to the literature, unless otherwise indicated, are to the bibliography appended to my paper in this volume. Discussions which are relevant to this issue are presented by Scheffler (1967), pp. 36–44, and by Shapere (1966), pp. 77–78.

relate them with the help of an *empirical generalization* [my italics]. Any such procedure would imply the false assertion that the velocity of light is infinitely large."<sup>2</sup> T. S. Kuhn similarly states: "Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they are not conceived to be the same."<sup>3</sup>

Koslow is perhaps misled by Feyerabend's comment about the relation between Newtonian mechanics and relativity which is based on the argument cited above. Feyerabend wrote: "The order introduced into our experiences by Newton's theory is retained and improved upon by relativity. This means that the concepts of relativity are sufficiently rich for the formulation of all the facts which were stated before with the help of Newtonian physics. Yet the two sets of concepts are completely different and bear no logical relation to each other."<sup>4</sup>

Assuming that the "logical relation" spoken of here refers to those two relations characterized in Feyerabend's earlier citation, though the historical school can be interpreted as claiming that no *positive* logical relation may be constructible, conflict between the Newtonian theory and the theory of relativity is admitted. In sum, then, it appears that the difference in meaning can and does permit the type of conflict which I refer to in my paper as "the amount of concordance." Since Professor Koslow also finds this locution somewhat unclear, let me attempt to explicate it again.

It is impossible in these pages to attempt to construct a *calculus* which would provide a metrical analysis of the notion of "amount of concordance," though it is not certain that such a calculus is a priori unconstructible. The idea can be made rather clear in a comparative sense, however, though I thought that I had sufficiently done so in the context of the Lorentz-Einstein example discussed in my paper. There, the relative *amount of concordance* between the theoretical context and a new theory, either Lorentz's 1904 theory or Einstein's 1905 theory in this case, is dependent on the fundamentality and extent of the contradiction between the theoretical context and each of the new theories. Einstein's theory, as was stated, conflicted with the very fundamental and extensively employed concepts of Newtonian space and time, whereas Lorentz's theory only contradicted—primarily by enlarging on—some of the dynamical postulates

concerning moving ions or electrons and molecules. I do not believe that there is any doubt among historians of this period either about the difference in the depth and extent of the conflicts between Einstein's theory and the accepted classical theories, or about the thesis that Einstein's theory was in greater conflict with traditional physics than was Lorentz's theory.

Professor Koslow has also criticized my critique of the historical school's account of the relation between theoretical and observational terms. Here I believe that Koslow does make an important point concerning the "primary" sense of an observational term, when he suggests that it may not be necessary for two physicists to have the *same* experience when they learn the primary sense of an O-term. This does appear to be a consequence of my analysis as given in the paper. On further reflection, I think it important to be able to generalize the *referent* to a *referent class*, and to permit "pictures," "slides," "diagrams," and the like, any and all of which can convey the primary sense of an O-term such as "optical fringe phenomena," or an O-sentence in which such a term appears. Though there are interesting epistemological problems associated with determining the boundaries of this class of referents, I do not think that they are peculiar or crucial to this inquiry.

But this point being granted, I do not see that the distinction into primary and secondary senses does not do the job for which it was proposed. Specifically, Koslow suggests that the distinction cannot fulfill its task because it is only by virtue of the secondary non-common sense that an O-sentence stands in relation to a theory. Consequently, Koslow notes, "if different theories T and T' imply different secondary senses for O-sentences related to the theories by C-sentences, then T and T' have no common evidential base."

Koslow considers this difficulty more specifically in the context of the Michelson-Morley experiment, and the support which it provides for both the Lorentz theory and Einstein's theory. He contends, contrary to my own position, that the interferometer experiment's results are not part of any intertheoretic common base between Lorentz's and Einstein's theories.

I believe that Professor Koslow has not completely understood my account of the relation of the O-sentences to theoretical sentences. Let me present, first, a most elementary example, and then re-present the interferometer example.

<sup>2</sup> Feyerabend (1962), pp. 80–81.

<sup>3</sup> Kuhn (1962), p. 101.

<sup>4</sup> Feyerabend (1962), p. 89.

Consider the case of an apple which is found each morning on a teacher's desk. The apple can be described as "red," "round," and, if tasted, as having an "apple-like taste." Perhaps simply describing it as an "apple" will be sufficient. The apple can also be taken as evidence for the existence of (1) a thoughtful, concerned student, or (2) a diabolical student who is attempting to worry the teacher about the reason for the appearance of the apple each morning. We can assume that (1) and (2) are inconsistent with each other. We can also, depending on our assessment of the situation, ascribe either the secondary sense, "evidence for a thoughtful concerned student," to the apple, or the sense associated with the alternative explanation of the apple's appearance. The adjectives "red" and "round" and the noun "apple" are part of the primary vocabulary in this case and capture the primary sense of the referent, the apple. (This is not to say, of course, that "red" and "round" and "apple" are necessarily atheoretical terms.) A description of the apple in these terms is accepted by all parties concerned, and such a description can be accepted on hearsay by proponents of either explanation (1) or (2), even if a "neutral" observer records the appearance of the apple each morning.

The same type of analysis can be made regarding the Lorentz and Einstein accounts of the Michelson-Morley experiment. The Lorentz and Einstein theories are both theories which are proposed to account for the electrodynamics (and optics) of moving bodies. The earth is a moving body, in both theories, and the interferometer experiment, in both theories, tests for a "second-order" effect of this motion in a very precise fashion. The O-sentence reporting the result of a rotation of the interferometer through 90° states that "no experimentally significant optical fringe shift was discernible." This sentence expresses the primary sense associated with the observation just as a sentence characterizing the apple on the teacher's desk expressed the primary sense associated with that observation. The interferometer result just given, can, with the (possibly tacit) aid of auxiliary theories and C-sentences connecting the observation with the theories, be taken to support either the Lorentz theory, cum L-F contraction, or the Einstein theory, by being shown to be a consequence of these theories, auxiliary theories, and appropriate initial conditions. The O-sentence can then have associated with it, or grafted onto it, the secondary sense which interprets the observation as *support for* this or that theory.

It should be clear that this is not the epistemology of the historical

school, which is at this point being defended by Koslow. The historical school would not accept the distinction between the primary and secondary senses ascribable to an O-sentence. What I am defending here is the thesis that both the Lorentzian and the Einsteinian can and do withdraw to a level, characterized by the primary vocabulary, where *both* agree on the observational evidence. That the Michelson-Morley data were used both by Lorentzians and relativists is a hard fact of scientific history; the rationale for that fact has been re-presented here.

I shall now turn briefly to Koslow's claim that Einstein's theory may not be simpler than Lorentz's because it eliminates a "singular referring term," the ether, which has only one property, immobility. First let me note that the ether, though it is principally characterized by Lorentz as possessing immobility, and as having velocity vectors ascribable to it, in the sense of the ether "wind," also exerts specific causal influences, such as the contraction effect, and may exert other causal influences as yet unrevealed by experiment. Accordingly, it is not simply a substantiated property of immobility, but a more complex entity, at least as regards its effects.

Further, Lorentz did not dispute the hypothesis that the ether had some structure, for as I noted on page 332 of my paper, Lorentz ascribed the existence of electrical and magnetic forces to its state. It is true that Lorentz did not think there was much value in the *mechanical* analyses of the ether which had been pursued throughout the nineteenth century, but his characterization of the ether did not preclude further analysis of this entity, which he himself, in 1909, regarded "as endowed with a certain degree of substantiality, however different it may be from all *ordinary matter*" (my italics).

I would not deny Koslow's suggestion, following Quine, that a theory with a simpler ontology but a more complex ideology might be, on balance, more complex than a theory with a more complex ontology. I would hope, however, that the various senses of relative simplicity, including fitness, discussed in my paper, might prove sufficiently sensitive in such a situation and accord with both Quine and our intuitions. I do not see that Koslow has provided any arguments to show that this is not the case.

Finally, let me consider Koslow's difficulty with the joint application of the assessments made in terms of the three theory-evaluative categories. It should be clear from my paper that the total judgment functions in a combinatorial fashion. Contrary to Mr. Bowman's suggestions, it seems to me that theoretical context sufficiency and experimental adequacy have rough-

ly equal weight, with relative simplicity becoming an important factor when the assessment made by the application of the previous categories is contradictory or indeterminate. (It should be remembered, however, that the theories constituting the theoretical context are themselves tested by experiments, so that *indirectly* experimental adequacy enjoys a paramount position.) I perhaps also ought to mention that though I believe the categories to be weighted as I suggested in my paper and again in this rejoinder, in different specific cases of theory conflict there will be differences in the importance of judgments made in the three categories. This will depend on the extent and the fundamentality of the contradictions between a new theory and the theoretical context, and between the new theory, experimental results, and the expectations one has of the theory's need to be able to account for specific experimental results. As a case in point, the acceptance of the old quantum theory probably depends more heavily on black-body radiation experiments, and not significantly on relative simplicity.

I shall reply in two different ways to the question which Professor Koslow raises concerning "who" is making a split decision in 1905–7 as regards the worth of the competing Lorentz and Einstein theories. First, insofar as the tricategorical logic proposed in my paper constitutes a *logic*, the question does not have to be answered by naming a particular person or group of persons. It is similar to the question "who believes that the syllogism in Barbara is valid?" Though I have admitted that the proposed tricategorical logic does not admit of any easy and logically "effective" application, once the application has been made it is up to Koslow to indicate where an incorrect premise has been admitted, or a fallacious inference made. Since he has not done this, I feel fairly secure that my analysis still stands, and that the "who" from *this* point of view is superfluous.

But if the tricategorical logic is to be relevant to the history of science, it should also be possible to find a group of scientists who did indeed make a split decision in 1905–7, and who came to be convinced of the superiority of Einstein's theory by 1909–10. Also, since both Lorentz and Einstein, as Koslow points out, did not make such a decision themselves, it is necessary to account for their polar positions on the issue.

In my paper I introduced the notion of a "subjective fluctuation" from the scientific consensus. For reasons which I presented in some detail in the paper, both Einstein and Lorentz constituted subjective fluctuations. (See especially pages 337 and 344. Parenthetically related to this idea is the

conjecture that great scientific creativity necessarily involves such "subjective fluctuations." Though there are interesting implications which might be based on this conjecture, to consider them further would take us beyond the scope of this rejoinder.)

Were the sociology of science a well-developed discipline instead of a still relatively new branch of science, it would be tempting to compare the results of my "logical" analysis with the sociologically determined consensus of science in the years 1905–10. Since this is not a viable possibility, I will close by going out on a limb, and suggesting that the verdict of a "split decision" as regards the comparative worth of Lorentz's and Einstein's theories in 1907, say, is to be predicated of the "group mind" of the scientific elite primarily in Germany—France, England, etc., not having yet been in sufficient journalistic contact with Einstein's theory at this time. The sense of the term "group mind" is not very different from E. Durkheim's term *l'âme collective*, which is discussed in his classic *The Rules of Sociological Method*.<sup>5</sup> According to Durkheim, such a "group mind" ought to be reflected in an appropriate statistical sampling composed of individual members of this elite, and should be revealed in scientific journal articles and in correspondence. To the best of my knowledge, which is based on reading such material written by Planck, Sommerfeld, von Laue, and Minkowski, among others, this is in fact the case.

<sup>5</sup> Cf. Emil Durkheim, *The Rules of Sociological Method*, trans. S. A. Solovay and J. H. Mueller, ed. G. E. G. Catlin (New York: Free Press, 1938; originally published in French, 1895), pp. 8ff.