complete. Strike-slip and dip-slip faults very probably involve different strain-generating mechanisms. No mechanism proposed to date for generating either strike-slip or dip-slip strains has achieved general acceptance.

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The Bearing of Philosophy on the History of Science

Philosophical mastery of the special theory of relativity is required for unraveling its history.

In what precise ways is philosophy instrumental in illuminating the *genesis* of the conceptual innovations wrought by a particular physical theory? In a book published in 1963 and in some papers which have appeared since 1961, I have used the unraveling of the history of the special theory of relativity to argue concretely that philosophy does have far-reaching relevance to the attainment of the following cardinal objectives of the historian of science: (i) the very posing of well-conceived, searching historical questions and (ii) the avoidance of serious historical blunders of certain kinds, and their discernment as such when they have been committed by those lacking the requisite philosophical mastery (*1*, chap. 12; *2*). Specifically, I maintained in the context of the special theory of relativity that there is a symbiosis of the philosophy and the history of science as follows: no historically correct, let alone illuminating account of the development of that theory can be furnished without a prior rigorous comprehension of the philosophical conceptions underlying it and distinguishing it from its ancestors. At the same time, I recognized that the history of the theory, in its turn, may indeed contribute to the philosophical analysis of the theory by disclosing the vicissitudes in Einstein's own philosophical outlook.

I now return to the theme of these earlier publications in order to develop it anew. And I do so for the following reasons: (i) I can now supply explicit and specific support for my thesis from Einstein himself, in the form of source materials. The materials in question were either published in 1963 or were previously unknown both to me and presumably to nearly all interested people. (ii) I have had second thoughts on my earlier logical *and* historical assessment of the charge that the *aether-theoretic* Lorentz-Fitzgerald contraction hypothesis and the aether-theoretic Lorentz-Larmor time-dilation hypothesis were severally and collectively *ad hoc*. My revised assessment of the charges against these auxiliary hypotheses is prompted by recognition of the need for making previously neglected distinctions between quite different senses in which a collateral hypothesis can have the logical status of being *ad hoc*. If this revised analysis is sound, it will have quite *general* relevance to the philosophy and history of science.

The source materials which have induced me to provide a fresh treatment of some facets of my earlier theme consist of two reports of interviews with Einstein in which he was asked to recall in as much detail as possible the thought processes which led him to propound the special theory of relativity. The first of these came to light in January 1963 with the publication of R. S. Shankland's "Conversations with Albert Einstein," which constitutes a record of what Einstein recalled during 1950 to 1954 concerning the genesis of the theory (*3*). The second report, which I ran across quite recently, is an account by the Gestalt psychologist Max Wertheimer of conversations he had with Einstein starting in 1916 concerning "The thinking that led to the theory of relativity" (*4, 5*).

I welcome the opportunity of this address here in Cleveland as a fitting occasion for a further philosophical glance at the history of the special theory of relativity. For it was in this city that Michelson, Morley, and Miller carried out interferometric investigations which figured prominently in the history of the debate on the theory. Cleveland was also the site at which in 1888 Michelson gave his vice presidential address on optical research to the physics section of the AAAS. It is an irony of history that in that address of 1888 Michelson saw fit *not* to mention the now celebrated null result

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which he and Morley had obtained in 1887 (*6*), a result which the Cleveland scientific community of that time regarded as evidence for the *failure* of that experiment and which led to Morley's being "an object of pity" (*7*, p. 55, p. 49, note 9). Perhaps Michelson's lack of pride in the null outcome of the experiment was accompanied by a premonition that it might inspire theoretical developments in physics that would prove most unpalatable to him. For on several occasions Michelson honestly and good-naturedly expressed his distaste to Einstein personally for the theories that had been ushered in by his optical work (*7*, p. 57). In fact, Michelson's dislike of the relativity theory was such that he referred to it in Einstein's presence as a "monster" (*7*, p. 56). Similarly uncomplimentary characterizations of the theory are given in a 1951 paper by the noted optical experimenter H. E. Ives (*8*), whose 1938 experimental confirmation, in collaboration with Stilwell, of the quadratic Doppler effect lent support to the relativistic clock retardation.

The development of my thesis that a thorough grasp of the philosophical foundations of the special theory of relativity is indispensable to its illuminating historical study will turn out to yield an important consequence for the pedagogy of the theory. Namely, the clear recognition that the standard presentation of the theory in many physics texts such as is given by Richtmeyer, Kennard, and Lauritsen in their well-known *Introduction to Modern Physics* (*9*) must be revamped, because it inverts the *logical order* of Einstein's ideas and badly beclouds their epistemological anchorage.

As my first case in point, I turn to the principle of the constancy of the speed of light in the special theory of relativity—hereafter called "the light principle" for brevity—which asserts that the speed of light is the same constant c in all inertial systems, independent of the relative velocity of the source and observer, and of direction, position, or time.

History and Pedagogy of the Light Principle

The standard textbook presentation would have us believe that historically Einstein resolutely enunciated the light principle as a direct inductive generalization from the null outcome of the Michelson-Morley experiment. Thus we find, in the aforementioned well-known textbook on modern physics, that the statement of that experimental result is used in an attempt to confer credibility on the claim that "Einstein accordingly put the laws of the propagation of light in the forefront of the discussion." And this statement is then immediately followed by the assertion that Einstein based his special theory of relativity on two postulates, the principle of relativity and the light principle.

I now endeavor to show that this kind of logico-historical account on which I was brought up is grossly misleading and unsound for the following reasons. (i) It asks the student to swallow a serious conceptual travesty, buttressed by a historical myth which explodes in the face of a clear comprehension of the philosophical foundations of the light principle. (ii) In *conceptual* respects, the standard textbook account in question is glaringly incompatible even with the text of Einstein's historically taciturn own fundamental 1905 paper on the special theory, a paper that by no means gives an adequate account, as we now know from Einstein himself (*5*, p. 228), of the full reasoning by which he groped his way to an espousal of the theory.

1) It is logically indispensable that the statement of Einstein's light principle be *preceded*—as indeed it is in his 1905 paper—and not followed by the repudiation of the Newtonian conceptions of simultaneity, time interval, and distance in favor of the corresponding relativistic ones. For otherwise the student who is confronted with the assertion of the light principle will assign Newtonian meanings to its terms, a construal which will involve the assumption of the validity of the *Galilean* transformations. Hence in that context the assertion of the light principle will be a self-contradictory claim whose evident absurdity can hardly be removed by the invocation of the null-result of the Michelson-Morley experiment. And it is then also historically incredible that Einstein himself would have propounded and espoused so blatant an absurdity without *first* having supplanted the appropriate Newtonian conceptions by relativistic ones. Yet precisely this combination of logical and historical absurdities is foisted on the student by the rather standard textbook presentation of Richtmeyer, Kennard, and Lauritsen. After describing the null result of Michelson and Morley, these authors introduce the statement of the light principle to the Newtonian student by the following sequence of sentences:

> Yet it seems quite impossible that light should move with the same velocity relative to *each* of *two* frames that are *moving relatively to each other!*
>
> Einstein accordingly [sic] put the laws of the propagation of light in the forefront of the discussion.

2) But on turning to the pages of Einstein's fundamental paper of 1905, we find that it is not until the start of section 2 (§2) that he puts the light principle into the forefront of the discussion. And when he does give a statement of that principle at the beginning of the section, he includes a conceptually crucial qualification which is conspicuously absent from the aforementioned textbook account. Namely, he notes pointedly that the one-way velocity of light to which the principle refers is based on the definition of simultaneity which he had given in § 1. In that *first* paragraph, he had given an epoch-making new philosophical treatment of the problem of simultaneity *within a single inertial system.* And not until §2 did he state the equivalent of the light principle and then show at the end that the latter entails discordant judgments of simultaneity as between Galilean frames moving relative to one another.

Specifically, the conceptual innovation of Einstein's §1 is the anti-Newtonian view that the metrical simultaneity of two spatially separated events involves a convention *within any given inertial system.* It is thus a grievous philosophical blunder to suppose, as is done in many expositions, that the repudiation of Newton's conception of simultaneity by the theory occurs, *in the first instance*, in the context of the relative motion of different inertial systems (*10*). And this philosophical misconstrual of Einstein's conception of simultaneity precludes awareness that his conception in §1 rests on two essential physical assumptions (to be stated below) *which must not be identified with the two well-known postulates of Einstein's §2.* Hence the philosophical misplacement of the *primary* locus of Einstein's conceptual departure from Newton in regard to simultaneity issues in the following serious

penalty for the investigator of the history of the theory: the lack of philosophical mastery on the part of the historian will conceal from him that there exists the historical question as to the grounds on which Einstein felt entitled, in §1 of his 1905 paper, to make the particular two physical assumptions undergirding his philosophical doctrine of the conventionality of the simultaneity of spatially separated events!

Einstein formulates this doctrine by stating in §1 that the equality of the one-way velocities of light in opposite directions within the same inertial system is a matter of *definition* rather than of physical fact: The facts of the temporal order allow the round-trip time of light for a given path connecting two points P_1 and P_2 to be split into any two parts as the respective one-way transit times in the two opposite directions $P_1 P_2$ and $P_2 P_1$. And the two physical assumptions providing the logical underpinning of his conventionalist conception of simultaneity are *not* the two well-known postulates of §2 but the following two different *non*-Newtonian physical assumptions: (i) Within the class of physical events, material clocks of identical constitution do not define uniquely obtaining or "absolute" relations of simultaneity under transport. (ii) Light is the fastest signal in a vacuum in the following topological sense: no kind of causal chain (moving particles, radiation) emitted in a vacuum at a given point A together with a light pulse can reach any other point B earlier, as judged by a local clock at B which merely orders events there in a metrically arbitrary fashion, than this light pulse.

Einstein sets forth his conventionalist conception of simultaneity without stating these two assumptions, which are each a necessary condition and jointly a (partial) set of premises for Einstein's conception of simultaneity (*11*). But philosophical analysis discloses their crucial presence in the logical foundations of his doctrine of simultaneity as set forth in §1. Hence philosophical analysis prevents the historian from overlooking the point that Einstein must have made the two required assumptions and must have justified them to himself before finally stating their upshot in very concise form. And in this way philosophical analysis makes for awareness that there is an historial question as to the grounds on which Einstein convinced himself of the two underlying assumptions when writing his 1905 paper.

That the philosophical discernment of the presence of the two tacitly made physical assumptions does indeed give a correct steer to the student of the genesis of the special theory of relativity is attested by the historical account that Einstein gave to Wertheimer as well as by his "Autobiographical notes." Wertheimer writes (*5*, p. 228, note 7):

> I wish to report some characteristic remarks of Einstein himself. Before the discovery that the crucial point, the solution, lay in the concept of . . . simultaneity, axioms played no role in the thought process—of this Einstein is sure. (The very moment he saw the gap, and realized the relevance of simultaneity, he knew this to be the crucial point for the solution.)

More particularly, when talking to Wertheimer, Einstein made a decisive comment on the exposition of the theory given in his joint book with Infeld, where the two postulates of §2 of the 1905 paper are presented in standard textbook fashion as the fundamental axiomatic starting point of the theory (*12*). He tells us (*5*, p. 228, note 7) that this way of presenting the theory

> is not at all the way things happened in the process of actual thinking. This was merely a later formulation of the subject matter, just a question of how the thing could afterwards best be written. The axioms express essentials in a condensed form. Once one has found such things one enjoys formulating them in that way.

And Einstein's "Autobiographical notes" record (*13*) that all attempts to develop the theory

> were condemned to failure as long as the axiom of the absolute character of time, viz., of simultaneity, unrecognizedly was anchored in the unconscious. Clearly to recognize this axiom and its arbitrary character really implies already the solution of the problem. The type of critical reasoning which was required for the discovery of this central point was decisively furthered, in my case, especially by the reading of David Hume's and Ernst Mach's philosophical writings.

In addition to serving as a pathfinder for the historian of science, philosophical awareness of the foundations of Einstein' conception of simultaneity can provide prophylaxis against certain kinds of historical and pedagogical errors. I noted that philosophical analysis exhibits the *logical* dependence of Einstein's doctrine of simultaneity on the prior assumption that light is the fastest possibile signal in a vacuum, an assumption to which I refer as "the limiting assumption." But there is a widespread failure to realize that the limiting assumption is thus presupposed both by the definition of simultaneity in Einstein's §1 and by the light principle in §2. This philosophical failure inspires the incorrect supposition that the limiting assumption still requires deductive justification within the theory by the time the laws of velocity addition are derived from the Lorentz transformations. The further *erroneous* belief that the limiting assumption is actually deducible from the velocity-addition laws—which it is *not* (*14*)—then begets the *historical* falsehood that these addition laws were the basis on which Einstein was first able to convince himself of the truth of the limiting assumption. Hence one boggles at the extent to which otherwise excellent physics books incorrectly present the limiting assumption as a deduction from the velocity-addition laws (*15*).

Contraction and Time-Dilation Hypotheses

The charge that the aether-theoretic Lorentz-Fitzgerald contraction hypothesis and the aether-theoretic Lorentz-Larmor time-dilation hypothesis were severally and collectively *ad hoc* has figured prominently in the debate between the aether-theoretic conception of the Lorentz transformations and Einstein's rival relativistic interpretation of them. And the aether-theoretic Lorentz-Fitzgerald contraction hypothesis has come to be the most widely used textbook and classroom illustration of an *ad hoc* auxiliary hypothesis. Clarity as to the several senses in which an auxiliary hypothesis may be held to be *ad hoc* will permit determining in which of these several senses, if any, either or both of the aforementioned aether-theoretic auxiliary hypotheses are in fact *ad hoc*. Hence such clarity can contribute to *historical* understanding of the logical and psychological factors which enabled Einstein's relativistic conception of the Lorentz transformations to gain acceptance at the expense of the earlier rival aether-theoretic interpretation of them.

Let me begin with an elucidation and appraisal of Einstein's own rejection of the Lorentz-Fitzgerald contraction hypothesis. It will be convenient

to have a name for an outcome of a particular *kind* of experiment that is embarrassing to a previously successful theory T, as in the case of the embarrassment of the original aether theory by the null result of the Michelson-Morley experiment. Using the first letter of the word "embarrassing" as a prefix, I name such an embarrassing result an E-result of the particular kind of experiment with respect to the theory T. It is to be well understood that in the context of any one particular theory, the specification of the attributes which characterize the given *kind* of experiment as such and distinguish it from other kinds is given by reference to the kind of conditions under which the theory T yields specified particular values of the variables ingredient in its postulates. This specification is required to give precise meaning to the question whether in the context of a given theory an auxiliary hypothesis which explains an embarrassing result of *one kind* of experiment lends itself to independent test in at least one *other kind* of experiment.

Speaking of Einstein's reaction to the Lorentz-Fitzgerald contraction hypothesis, Wertheimer reports (*5*, pp. 218–219) that

> for Einstein the situation was no less troublesome than before; he felt the auxiliary hypothesis to be a hypothesis *ad hoc*, which did not go to the heart of the matter. . . . He felt that the trouble went deeper than the contradiction between Michelson's actual and the expected result.

Since Einstein thus forsook the Lorentz-Fitzgerald version of the aether theory as *ad hoc* in his quest for a new rival theory, I deem it reasonable to take Einstein's rejection of the contraction hypothesis as "*ad hoc*" in this context to be tantamount to the following two-fold claim: (i) Prior to the E-result of the Michelson-Morley experiment, no other kind of experiment had an outcome providing support for the contraction hypothesis. (ii) It is to be expected that the Lorentz-Fitzgerald modification of the aether theory will *not* be confirmed by subsequent tests of a kind *different* from the Michelson-Morley experiment. And this conjecture serves as a reason for not accepting the Lorentz-Fitzgerald contraction hypothesis as an explanation of the null result of the Michelson-Morley experiment. Thus I take Einstein's rejection of the contraction hypothesis as *ad hoc* not only to allow but to assert that this hypothesis is indeed "independently testable," that is, testable by experiments of a kind different from the Michelson-Morley experiment. For I construe Einstein's pejorative usage of the term "*ad hoc*" to refer to the posited fact that though independently testable, the contraction hypothesis would fail to secure subsequent independent experimental confirmation *as against the claims of a new rival theory*.

We shall see after some analysis that if Einstein did consider the contraction hypothesis as *ad hoc* in *this* sense, he was quite right. The analysis that vindicates Einstein will also completely *refute* the standard textbook indictment of the contraction hypothesis, which charges this hypothesis with being *ad hoc in the quite different sense* of *not* being testable *independently* of the Michelson-Morley experiment. Let us appraise the claim that the Lorentz-Fitzgerald contraction hypothesis is *ad hoc* in this widely alleged sense. To do so, it is essential that we remedy the imprecision of the concept of independent testability encountered in the literature. The existence of such imprecision and the need for removing it emerge from the following two sets of considerations.

In the first place, reference is made to the set of *all* observational consequences of a given theory T, when it is claimed that the contraction hypothesis has no consequences by which it might be tested other than the observations in a Michelson-Morley type of experiment. But the set of all observational consequences of a given sophisticated theory T can have a well-defined membership only if we delimit and, if possible, specify the rules of correspondence (sometimes misleadingly called "operational definitions") which, in conjunction with the postulates of T, are held to constitute the given theory and which anchor the postulates of T in the observational base. For in the absence of some such delimitation or "freezing" of T in a given stage of its development, the theoretical terms of the postulates of T represent "open" concepts in the following sense: they admit the adjunction of further rules of correspondence ("operational definitions") to those constituting the merely partial empirical interpretation of the postulates of T at any given time. And this open-textured character of the theoretical terms of T would then make for a corresponding imprecision or open-endedness in the membership of the class of observational consequences of T. Hence the latter class must be relativized to a specific, delimited set of rules of correspondence. In this way, the systemic attribute of independent testability of an auxiliary hypothesis and also that attribute's negate of being *ad hoc* become correspondingly relativized to a delimited set of rules of correspondence. In the context of this proviso of relativization, consider an auxiliary hypothesis H which is introduced into the framework of a given theory T in response to an experimental outcome which is an E-result with respect to T. It is clear that the possession by H of the systemic attribute of being independently testable and thus *not ad hoc* within the framework of T in no way depends on whether the advocates of H are *aware* of such independent testability. To suppose that there is any such dependence is just as erroneous as to maintain that whether a mathematical proposition is a theorem in a given axiom system depends on whether mathematicians possess the psychological attribute of realizing that the given proposition is indeed a theorem. We shall see that the contraction hypothesis indeed does *not* constitute an *ad hoc* modification of the aether theory in the sense now under discussion. For I shall demonstrate that its confirmation is logically possible in an experiment different from the Michelson-Morley type. Hence, if Lorentz and Fitzgerald were in fact unaware of and disbelieved in the latter independent testability of their auxiliary hypothesis, their unawareness and disbelief cannot possibly render that hypothesis systemically *ad hoc*. If these theoreticians did espouse their contraction hypothesis while mistakenly believing it to be systemically *ad hoc*, this espousal would merely establish their own methodological culpability in this respect. In that case, their espousal of the contraction hypothesis can be said to have been *psychologically ad hoc*. But the proof below of the independent testability of their contraction need take no cognizance of the beliefs which they actually entertained about its independent testability.

There is a second respect in which the concept of independent testability requires more precise statement than any given heretofore in the literature to my knowledge. The need for this

refinement has been pointed out to me by Carl Hempel in private correspondence (*16*). Hempel notes that strictly speaking, *no* auxiliary H which is offered to save a theory T from an E-result is independently testable by itself or is ever *ad hoc* by virtue of failure to be independently testable in isolation from T. For H entails testable observational consequences only in conjunction with one or more of the basic or derivative principles of T. Hence Hempel points out that the concepts of independent testability and of being *ad hoc* must make due allowance for this contextual character of the observational import of H. And he suggests that this might be attempted along the following lines: Given that a theory T not containing H entails a false observational consequence F, then an auxiliary hypothesis H is systemically *ad hoc* in the context of T, if the combination of T with H—which we call TH—has the same observational consequences as T with the sole exception of the single observationally false F. Thus, if H is to be *ad hoc*, all observational consequences of T *other than* F must be *identical* with those of TH. And it is to be understood here that the observational consequence F covers an infinite class of observation statements which differ only in the places and times to which they pertain.

Now Hempel notes that this definition of "*ad hoc*" involves the concept of a single observational consequence pertaining only to the outcome of a particular kind of experiment. And he questions whether the latter concept can be circumscribed *in purely logical terms* so as to ever allow an auxiliary hypothesis H to qualify as *ad hoc* by meeting the requirements of this definition. For Hempel doubts that any H can ever qualify or fail to qualify as *ad hoc* on the strength of the feasibility of a specification in purely *logical* rather than "denotative" terms of what constitutes (i) one particular or single kind of experiment, and (ii) one single observational consequence F of T pertaining to the outcome of this one particular kind of experiment. And his reasons for this doubt are as follows. First, any attempt to provide the required purely logical specification of the one kind of experiment and of the content of F would always have to exclude what are, in some imprecise intuitive sense, *variants* upon the one experiment, thereby also excluding the outcomes of these variant experiments from the range of occurrences covered by F. Yet in the case of these variants as well, TH would predict a different outcome from the one entailed by T alone, so that TH differs in its observational import from T not only with respect to F. Thus no H could qualify as *ad hoc* in the sense of the definition. And second, any proof that a particular H is *not ad hoc* might be contestable as inconclusive even though the proof adduces the existence of *two* kinds of experiment for *each* of which TH predicts a different outcome from the one yielded by T alone. For in the absence of a purely logical delimitation of what constitutes a single kind of experiment, it might be claimed that the two different kinds of experiment are merely subvarieties of one single kind of experiment.

On the basis of these doubts that the attribute of being *ad hoc* can be defined in purely logical terms, Hempel suggests that the methodologically important notion of an *ad hoc* hypothesis involves the following idea, which is not specifiable in purely logical terms: an auxiliary H which enables a theory T to explain an E-result in conjunction with H is *ad hoc* if it does *not* have any observational consequences that are *significantly* or *interestingly different* from the E-result.

I regard as sound Hempel's doubt that the property of being a systemically *ad hoc* hypothesis can be defined in purely logical terms. And I concur with his claim that the specification of F is made denotatively, as it were, on the basis of judgments pertaining to the particular theoretical system at issue. Hence I invoke the concept of a *significantly different* observational consequence when now giving specific meaning to the question of whether the Lorentz-Fitzgerald hypothesis is *ad hoc* in the context of the aether theory. To do so, I must now specify what is to be understood by a Michelson-Morley type of experiment in contradistinction to other kinds and by the observational consequence of the aether theory pertaining to the outcome of this kind of experiment. The relevant observational consequence F of the aether theory is that the round-trip time T_v of light for the vertical arm of the interferometer, which is perpendicular to the direction of the earth's motion, is

$$T_v = \frac{2l}{(c^2 - v^2)^{1/2}}$$

while the round-trip time T_h for the horizontal arm, which points in the direction of the earth's motion, is

$$T_h = \frac{2l}{(c^2 - v^2)^{1/2}} \cdot \frac{1}{(1 - \beta^2)^{1/2}}$$

where $\beta \equiv v/c$. And this observational consequence F pertains to *any* experiment which yields an observational comparison of the two round-trip times for the case of arms of *equal* length l located in an inertial system having a constant velocity v through the aether. Any experiment furnishing this observational comparison is thus classifiable as belonging to the one Michelson-Morley type of experiment to which the deduction F from the theory pertains. In the light of Hempel's cautions, the question whether the Lorentz-Fitzgerald hypothesis constitutes an *ad hoc* modification of the aether theory therefore takes the following form: Is the Lorentz-Fitzgerald hypothesis testable in any kind of experiment which differs significantly and interestingly from the specified Michelson-Morley type? By demonstrating now that the answer to this question is decidedly affirmative, I shall establish that the Lorentz-Fitgerald hypothesis is *not* systemically *ad hoc*.

The Lorentz-Fitzgerald hypothesis asserts that the horizontal arm of the Michelson interferometer is of contracted length $l \cdot (1 - \beta^2)^{1/2}$ rather than of length l. And once the length l in the expression for the horizontal round-trip time T_h is replaced by this contracted length, T_h becomes *equal* to T_v, and the *difference* between them *vanishes*, as required by the null result of the Michelson-Morley experiment. That the Lorentz-Fitzgerald hypothesis has observational consequences whose *confirmation* is logically possible independently of the Michelson-Morley type of experiment can now be shown by demonstrating the following: The Lorentz-Fitzgerald modification of the aether theory yields *different* observational consequences from those entailed by the aether theory for the case of the type of experiment performed by Kennedy and Thorndike in 1932. Although the Kennedy-Thorndike kind of experiment also employs an interferometer, it differs importantly from the Michelson-Morley type. For, in the first place, *as measured by laboratory rods*, the horizontal and vertical arms of the Kennedy-Thorndike

experiment are not at all equal but are made as different in length as possible, thereby differing observationally with respect to the values of the relevant theoretical variable of length. And, in the second place, unlike the apparatus in the Michelson-Morley experiment, the interferometer of the Kennedy-Thorndike experiment does not have a constant velocity v in the aether by remaining in a single inertial system; instead the apparatus of the Kennedy-Thorndike experiment acquires different values of the relevant theoretical variable of velocity by being transported to various inertial systems via the diurnal rotation and annual revolution of the earth. If the unequal lengths of the vertical and horizontal arms as measured by rods in the laboratory have the values L and l, respectively, then the *difference* between the vertical and horizontal round-trip times of light entailed by the Lorentz-Fitzgerald hypothesis is *not* the same as the one entailed by the original aether theory. Specifically, in the case of the Kennedy-Thorndike experiment, the Lorentz-Fitzgerald hypothesis entails that the difference $T_v - T_h$ have the non-vanishing value

$$T_v - T_h = \frac{2(L-l)}{(c^2 - v^2)^{1/2}}$$

which *varies* with the diurnally and annually changing velocity v of the apparatus relative to the aether. Were it to materialize, the variation of this quantity would serve to detect any existing velocity v of the apparatus relative to the aether even on the assumption of a Lorentz-Fitzgerald contraction. But *without* the Lorentz-Fitzgerald hypothesis, the original aether theory yields the *different* non-vanishing, variable quantity

$$T_v - T_h = \frac{2}{(c^2 - v^2)^{1/2}}\left(L - \frac{l}{[1 - (v^2/c^2)]^{1/2}}\right)$$

It is evident that it is logically possible for a Kennedy-Thorndike type of experiment to *confirm* the quantitative predictions of the Lorentz-Fitzgerald hypothesis as against those of the original aether theory independently of the Michelson-Morley experiment. And this *logical* fact shows that the Lorentz-Fitzgerald hypothesis was *not ad hoc* in the systemic sense of Hempel's suggestion.

Furthermore, it is a matter of *empirical* fact that the Kennedy-Thorndike experiment of 1932 did *not* yield a shift in the interference fringes corresponding to the time difference deduced from the Lorentz-Fitzgerald hypothesis. In fact, just like the Michelson-Morley experiment, the Kennedy-Thorndike experiment had a negative outcome in the sense that there were no fringe shifts. Thus, one is entitled to claim that the Kennedy-Thorndike experiment failed to produce the kind of positive effect whose occurrence would have served to *confirm* the Lorentz-Fitzgerald hypothesis. But it would be an *error* to suppose that the *non*-obtaining of this particular kind of confirmation suffices to prove that the Lorentz-Fitzgerald hypothesis was *falsified* by the null result of the Kennedy-Thorndike experiment! For we shall now see that the adjunction of the *further* auxiliary hypothesis of time dilation to the Lorentz-Fitzgerald hypothesis does enable the thus *doubly* amended aether theory to explain the null outcome of the Kennedy-Thorndike experiment while upholding the Lorentz-Fitzgerald hypothesis. And it will then become apparent that the justification for rejecting the Lorentz-Fitzgerald hypothesis along with the doubly amended aether theory depends on having philosophical reasons for accepting Einstein's rival theory of special relativity to the exclusion of the doubly amended aether theory.

The velocity-dependent time-difference

$$\frac{2(L-l)}{(c^2 - v^2)^{1/2}}$$

yielded by the Lorentz-Fitzgerald hypothesis can be expressed alternatively as

$$T_v - T_h = \frac{2(L-l)}{c(1-\beta^2)^{1/2}}$$

Now suppose that in addition to the Lorentz-Fitzgerald hypothesis, one accepts the further Lorentz-Larmor auxiliary assumption that the rates of the clocks in a moving system are reduced by a factor of $(1 - \beta^2)^{\frac{1}{2}}$ as compared to the aether-system clocks. On this assumption of "time dilation," the time-difference $T_v - T_h$ assumes the constant value

$$T_v - T_h = \frac{2(L-l)}{c}$$

which is independent of the velocity of the apparatus through the aether, in conformity to the null result of the Kennedy-Thorndike experiment. Thus, when amended by both the Lorentz-Fitzgerald hypothesis and the time-dilation, the aether theory does account for the actual outcome of the Kennedy-Thorndike experiment.

Moreover, purely mathematically the doubly amended variant of the aether theory permits the deduction of the Lorentz transformation equations no less than does Einstein's special theory of relativity. And this aether-theoretic deducibility of the Lorentz transformations now permits us to see that even the *conjunction* of the Lorentz-Fitzgerald hypothesis with the assumption of the time dilation is not *ad hoc*. That the latter conjunction of auxiliary hypotheses is indeed testable in a kind of experiment which is independent of *both* the Michelson-Morley and Kennedy-Thorndike types is shown by the example of the so-called "quadratic" optical Doppler effect as follows: Being *mathematically* identical with the space and time transformations of the special theory of relativity, the Lorentz transformations of the doubly amended aether theory entail an optical Doppler effect which is *quantitatively different* from the one that is deducible from the original aether theory (*17*). Hence, the rejection of the doubly amended aether theory cannot be justified by claiming that the conjunction of its two auxiliary hypotheses is *ad hoc*, unless the term "*ad hoc*" is understood in a sense *different* from the one rendered by Hempel's suggestion.

I maintain that there is a very useful and interesting *different* sense of the term "*ad hoc*" which becomes relevant when the aether-theoretic conjunction of the Lorentz-Fitzgerald contraction and the time-dilation is appraised in the context of the conceptual rivalry between the doubly amended aether theory and the special theory of relativity. And that different sense of "*ad hoc*" is associated with a correspondingly different sense of "independent testability" which is the following. Since the observational consequences of the *aether-theoretic interpretation* of the Lorentz transformations are the same as those of their rival relativistic interpretation, the aether-theoretic interpretation can have no observational consequences that are different from those of the rival special theory of relativity. Hence there can be no observational consequences which would support the doubly amended aether theory as against the new rival special theory of relativity, a theory that refuses to postulate the existence of some

one preferred inertial aether frame when there is no kind of physical foundation for doing so. In the light of the absence of *this kind* of independent testability of the combined Lorentz-Fitzgerald and Lorentz-Larmor auxiliary hypotheses, their espousal solely for the sake of upholding the aether theory *to the exclusion* of the special theory of relativity can be said to be "*ad hoc*" in the new sense (*18*). And this new sense clearly makes the *ad hoc* attribute of the conjunction of the two auxiliary hypotheses relative to *two* theories which are *conceptually* rivals of one another though not differing in observational import (*19*).

My analysis has endeavored to show that *prior* to the availability of the special theory of relativity, Lorentz, Fitzgerald, Larmor, and the remaining aether-theoreticians could not justly be accused of having put forward auxiliary hypotheses which were, in fact, systemically *ad hoc* in *either* of our two senses. On the strength of the philosophically mistaken assessment of these auxiliary hypotheses as either singly or collectively systemically *ad hoc* in the sense of Hempel's suggestion, the historical conclusion has been drawn that the original theorizing of the aether-theoreticians involved a grave infraction of scientific method of which they ought to have been aware. In this way, a philosophical error *unfoundedly* generated the historical allegation that the aether-theoreticians were unable to envision independent tests for their auxiliary hypotheses and hence were methodologically culpable from the beginning for espousing them nonetheless. And laboring under the philosophical misconception that the auxiliary hypotheses are systemically *ad hoc* in the sense of Hempel's suggestion could dissuade an historian of science from making the effort to uncover the kind of *historical* evidence which alone can show whether they were *psychologically ad hoc* either severally or collectively.

Let me conclude by noting that in a paper of 1951 entitled "Is there an aether?" P. A. M. Dirac attempted to resuscitate the aether with the aid of quantum mechanics (*20*). That the aether may yet further engage the attention of philosophers of science in ways relevant to the concerns of the historian of science emerges from the following statements by Dirac, who wrote:

> . . . We may set up a wave function which makes all values for the velocity of the aether equally probable. Such a wave function may well represent the perfect vacuum state in accordance with the principle of relativity.
>
> . . . We thus see that the passage from the classical theory to the quantum theory makes drastic alterations in our ideas of symmetry. A thing which cannot be symmetrical in the classical model may very well be symmetrical after quantization. This provides a means of reconciling the disturbance of Lorentz symmetry in space-time produced by the existence of an aether with the principle of relativity.

I hope that this examination of the philosophy and history of the relativistic light principle and of the aether-theoretic auxiliary hypotheses has served to illustrate that philosophy is indeed instrumental in illuminating the *genesis* of the conceptual innovations wrought by a physical theory.

Summary

There are several specific ways in which the philosophy of science is instrumental in illuminating the *genesis* of the conceptual innovations wrought by a particular physical theory. The unraveling of the history of Einstein's special theory of relativity is used to maintain concretely that the philosophy of science does have far-reaching relevance to the attainment of several particular cardinal objectives of the historian of science. The development of this thesis by reference to special relativity focuses on (i) the principle of the constancy of the speed of light and (ii) the evaluation of the charge that the Lorentz-Fitzgerald contraction hypothesis and the Lorentz-Larmor time-dilation hypothesis were severally and collectively *ad hoc* modifications of the aether theory of light propagation. The analysis yields a corollary for the pedagogy of the special theory of relativity: the standard textbook presentation of that theory must be revamped, for it inverts the logical order of Einstein's ideas and beclouds their epistemological anchorage.

References and Notes

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4. This report constitutes chapter 10 of Max Wertheimer's *Productive Thinking* (*5*).
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10. For a detailed refutation of this error, see my book (*1*, pp. 352–367).
11. For details on these logical relations see my book (*1*, pp. 342–370).
12. A. Einstein and L. Infeld, *The Evolution of Physics* (Simon & Schuster, New York, 1942), p. 186.
13. See A. Einstein, "Autobiographical notes," in *Albert Einstein: Philosopher-Scientist*, P. A. Schilpp, Ed. (Library of Living Philosophers, Evanston, Ill., 1949), pp. 52–53.
14. For a proof that this belief is indeed erroneous, see my book (*1*, pp. 375–376).
15. For examples of the commission of this fallacy, see E. T. Whittaker, *A History of the Theories of Aether and Electricity* (Nelson, London, 1953), p. 38; and R. C. Tolman, *Relativity, Thermodynamics and Cosmology* (Oxford Univ. Press, Oxford, 1934), p. 26.
16. I thank Professor Hempel for his kind permission to cite this private correspondence and also for helpful comments on an earlier version of this paper.
17. See section 7 of Einstein's fundamental paper of 1905 on the special theory of relativity, and M. von Laue, *Die Relativitätstheorie* (Vieweg, Braunschweig, 1952), vol. 1, p. 20.
18. This account of the sense in which the two combined auxiliary hypotheses can be said to be *ad hoc* supersedes the treatment in my *Philosophical Problems of Space and Time* (*1*, p. 392).
19. It is an open question whether this new sense of "*ad hoc*" can be explicitly defined quite generally by reference to any two rival theories of an appropriate kind, and to what extent that putative new general sense would have relevance to diverse episodes in the history of science.
20. P. A. M. Dirac, "Is there an aether?" *Nature* **168**, 906 (1951).