The Special Theory of Relativity as a Case Study of the Importance of the Philosophy of Science for the History of Science.

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To Enrico Bompiani on his scientific Jubilee

Summary. - 1. *The philosophical justification of Einstein's conception of distant simultaneity as conventional depends on two cardinal physical assumptions which are stated. Awareness of these two assumptions poses the following historical problem: On what grounds did* EINSTEIN *feel entitled to make them in 1905? In an endeavor to answer this question, the contribution of experimental results to Einstein's postulational achievement in the Special Theory of Relativity («RT») is examined. As a consequence, the author rejects (i)* M. POLANYI'S *recent citation of the history of RT as evidence against an empiricist account of scientific knowledge, and (ii)* G. HOLTON'S *assessment of the relevance of knowledge of the history of RT to the philosophical mastery of its logical foundations.*

2. *An analysis of the* KENNEDY-THORNDIKE *experiment is used to provide a refutation of the widespread belief that the aether-theoretic* LORENTZ-FITZGERALD *contraction hypothesis was ad hoc in the logical sense. A distinction is drawn between a logical and a psychological sense in which an auxiliary hypothesis can be ad hoc.*

3. E. T. WHITTAKER'S *disparaging estimate of* EINSTEIN'S *contributions to RT vis-à-vis those of* LORENTZ *and* POINCARÉ *is shown to rest on fundamental philosophical misunderstandings of* EINSTEIN'S *conception of the* LORENTZ *transformations.* HOLTON'S *maxim for the study of the history of RT is then tested in the light of his evaluation of* WHITTAKER'S *belittlement of* EINSTEIN'S *role.*

§ 1. Introduction

Despite the vast literature on the special theory of relativity (hereafter denoted by «RT»), some of the most fundamental and tantalizing questions which can be raised in regard to its genesis are unanswered at the present time. And such answers as have been given to some of them are beset by serious and puzzling contradictions.

An account of the genesis of a theory can indeed serve as a propaedeutic to the analysis of its logical foundations. But in the case of RT, what is known so far about its *history* has failed signally to contribute to the clarification of the more subtle questions concerning its *epistemological* basis. On the other hand, serious *historical* blunders in regard to the respective contributions to RT by LORENTZ, FITZGERALD, POINCARÉ, LARMOR and EINSTEIN have been committed because of the failure to achieve adequate *philosophical* mastery of the logical foundations of the theory.

The major objective of this essay is to deal with some important issues in the history and philosophy of RT which have been neglected or misunderstood, and to show that a correct philosophical comprehension of the fully-evolved theory is decisively prerequisite to the following two undertakings: (1) the very posing of well-conceived, searching *historical* questions in regard to RT, and (2) the achievement of a historically sound assessment of the respective contributions made to the theory by LORENTZ, FITZGERALD, POINCARÉ, LARMOR, and EINSTEIN.

In particular, I shall deal with three main topics as follows: (*i*) EINSTEIN'S grounds in 1905 for making the two cardinal physical assumptions which are the basis for his philosophical doctrine that the simultaneity of two spatially-separated events is *conventional*, and the epistemological moral of his postulational achievement of 1905 in RT, (*ii*) The fallacy underlying the widespread citation of the history of RT as authority for the demonstrably false claim that the aether theoretic LORENTZ-FITZGERALD contraction hypothesis as such was *ad hoc* in the *logical* sense, and the support lent by the history of RT to the quite different conclusion that the *espousal* of the contraction hypothesis by LORENTZ and the other aether-theoreticians was *ad hoc* in the *psychological* sense, (*iii*) The role of fundamental philosophical misunderstandings of EINSTEIN'S conception of the LORENTZ transformations in conferring plausibility on the historical error committed by E. T. WHITTAKER'S disparaging estimate of EINSTEIN'S contributions to RT.

The discussion to be given of these topics will also yield a critical appraisal of recent claims by M. POLANYI (1) concerning the alleged *epistemological* import of the *history* of RT and by G. HOLTON (2) regarding the purported scope of the relevance of the *history* of science (RT) to the *philosophy* of science (RT). Specifically, pointing to the history of RT, POLANYI indicts almost all contemporary philosophy of science for having adduced the non-existence of adequate *rules* for making discoveries as a justification for systematically omitting the actual historical process of discovery from the account of the scientific method (3). And HOLTON makes the sweeping assertion that «it is through the dispassionate

(1) M. POLANYI, *Personal Knowledge* (hereafter cited under the abbreviation "PK"); Chicago, 1958, pp. 9-15, and "Notes on Professor GRÜNBAUM'S Observations,, in: *Current Issues in the Philosophy of Science* (ed by H. FEIGL and G. MAXWELL), New York, 1961, pp. 53-55 (hereafter cited under the abbreviation "Notes etc.").

(2) G. HOLTON, "On the Origins of the Special Theory of Relativity", *American Journal of Physics*, **28**, 627 (1960).

(3) M. POLANYI, PK, pp. 13-14.

examination of historically valid cases that we can best become aware of the preconceptions which underlie all philosophical study (4).

§2. History and Epistemological Moral of Two of Einstein's Assumptions of 1905 in RT.

What specific problems still need to be solved by the contemporary historian of RT in order that his results might serve as a propaedeutic to a purely philosophical investigation such as H. Reichenbach's *Axiomatik der relativistischen Raum-Zeit-Lehre* (5)?

Prior to dealing with this important question, we note briefly the gains which can accrue to the philosopher of science either logically or psychologically from the *historical* inquiry into the *role of experiment*, for example, in the genesis of RT. A philosophical investigation such as the aforementioned one by REICHENBACH aims to exhibit the logical foundations of the full-fledged theory, thereby permitting an assessment of the *experimental* warrant which can be claimed for it at any given time. This objective is achieved by the provision of an epistemologically illuminating, as distinct from a merely mathematically resplendent axiomatization. By uncovering for us the extent of the experimental support which RT was able to command in 1905 as contrasted with some later time, historical study can acquaint us with the erstwhile epistemological basis of the theory. And, as HOLTON has emphasized, historical inquiry can also disclose the vicissitudes in EINSTEIN's own philosophical orientation, thereby explaining why the advocates of contending schools of philosophic thought can each «find some part of EINSTEIN's work to nail to his mast as a battle flag against the others» (6). Thus, historical knowledge may facilitate the task of carrying out philosophical analysis of the theory. But even if this ascription of logical merit to genetic study could not be sustained, it would seem that psychologically few students of the philosophical foundations of the RT could long repress an insistent, gnawing curiosity about the reasoning by which its creator was able to arrive at it. Nor would many of them be immune to the fascination of tracing EINSTEIN'S actual reasoning, since such study would afford glimpses of the creative operation of man's soaring scientific imagination at its best.

If the history of RT is to serve as a propaedeutic to the student of that theory's epistemological foundations, then the following key problem beckons detailed investigation by the historians: what considerations

(4) G. HOLTON, *op. cit.* p. 636.
(5) Braunschweig, 1924.
(6) G. HOLTON, *op. cit.*, p. 632.

prompted EINSTEIN to make the *two* physical assumptions which are at the root of his philosophical doctrine of the *conventionality* of the simultaneity of spatially-separated events? This doctrine is set forth *very concisely* in § 1 of his 1905 paper, which is entitled « Definition of Simultaneity ». There he writes: «We have not defined a common «time» for [the spatially-separated points] A and B, for the latter cannot be defined at all unless we establish *by definition* that the « time » required by light to travel from A to B equals the « time » it requires to travel from B to A » ([7]).

The *two* physical assumptions on which this conception of simultaneity rests are the following:

(*i*) within the class of physical events, material clocks do *not* define relations of *absolute simultaneity* under transport, because the relations of simultaneity which are defined by transported clocks do *depend* on the particular clock that is used, in the following sense: if two clocks U_1 and U_2 are initially synchronized at the *same* place A and then transported via paths of *different lengths* to a different place B such that their arrivals at B coincide, then U_1 and U_2 will no longer be synchronized at B. And if U_1 and U_2 were brought to B via the *same* path (or via different paths which are of *equal* length) such that their arrivals do *not* coincide, then their initial synchronization would likewise be destroyed. Thus, depending upon which one of the discordant clocks would serve as the standard, a given pair of events at A and B would *or* would *not* be held to be simultaneous. Hence, this dependence on the particular clock used prevents transported clocks from defining relations of absolute simultaneity within the class of physical events.

(*ii*) light is the fastest signal *in vacuo* ([8]) in the following topological sense: no kind of causal chain (moving particles, radiation) emitted *in vacuo* 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.

We note that our very awareness of the fundamental historical problem we have just posed depends upon a prior philosophical understanding of EINSTEIN'S conception of simultaneity.

As will presently be evident, EINSTEIN himself gives us tantalizingly

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([7]) *The Principle of Relativity*, a collection of original memoirs, annotated by A. SOMMERFELD, Dover Publications, New York, 1952, p. 40; *italics in the original.* This collection will be cited hereafter as « PR ».

([8]) An account of the reason for the qualification *in vacuo* can be found in I.E. TAMM, "Radiation of Particles with Speeds Greater than that of Light" *American Scientist* **47** 169 (1959), and "General Characteristics of VAVILOV-CHERENKOV Radiation", *Science* **131**, 206 (1960).

incomplete explicit information concerning the grounds for his original confidence in his intuition that assumption (*ii*) is true. But even if we did possess full clarity on that score, we still confront the same question in regard to assumption (*i*) and are compelled to try to answer it with even less assistance from EINSTEIN himself, as we shall see.

The importance of understanding the grounds on which EINSTEIN thought he could safely make assumption (*i*) can be gauged by the following basic fact: if, in accord with NEWTONIAN conceptions, assumption (*i*) had been thought to be *false*, then the belief in the truth of (*ii*) alone would *not* have warranted the abandonment of the received NEWTONIAN doctrine of absolute simultaneity. And, in that eventuality, the members of the scientific community to whom EINSTEIN addressed his paper of 1905 would have been fully entitled to *reject* his *conventionalist* conception of *one*-way transit times and velocities. But the denial of absolute simultaneity by the latter conception is *crucial* for his principle of the constancy of the speed of light, as is evident from his statement of this principle in § 2 of his 1905 paper [9]. The indispensability of assumption (*i*) for EINSTEIN'S conception of simultaneity is further apparent from the fact that a NEWTONIAN physicist quite naturally regards *not* signal connectibility but the readings of suitably transported clocks as the fundamental indicators of temporal order. The NEWTONIAN recognizes, of course, that the truth of (*ii*) compels such far-reaching revisions in his theoretical edifice as the repudiation of his second law of motion, which allows particles to attain arbitrarily high velocities through accelerations of appropriate durations. But he stoutly and rightly maintains that *if* (*i*) is *false*, absolute simultaneity remains intact, unencumbered by the truth of (*ii*).

Specifically what does EINSTEIN himself tell us about his original grounds for assuming the truth of (*ii*)? At this point, it is essential to quote him *in extenso*. He writes [10]:

« By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results. The example I saw before me was thermodynamics. The general

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(9) Specifically, Einstein emphasizes in §2 that if the *one-way* velocity of light is also to have the numerical value c, then « time interval is to be taken in the sense of the definition [of simultaneity] in § 1 ». For details on the logical relations relevant here, cf. A. GRÜNBAUM, «Logical and Philosophical Foundations of the Special Theory of Relativity» in: A. Danto and S. Morgenbesser (editors) *Philosophy of Science*, New York, 1960, pp. 401-416. Hereafter this essay will be cited under the abbreviation « LFR ».

(10) A. EINSTEIN, «Autobiographical Notes », in P. A. Schlipp (editor), *Albert Einstein: Philosopher-Scientist*, Evanston, 1949, p. 53.

principle was there given in the theorem: the laws of nature are such that it is impossible to construct a *perpetuum mobile* (of the first and second kind). How, then could such a universal principle be found? After ten years of reflection such a principle resulted from a paradox upon which I had already hit at the age of sixteen: If I pursue a beam of light with a velocity c (velocity of light in a vacuum), I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to MAXWELL'S equations. From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as for an observer who, relative to the earth, was at rest. For how, otherwise, should the first observer know, i. e., be able to determine, that he is in a state of fast uniform motion?

One sees that in this paradox the germ of the special relativity theory is already contained. Today everyone knows, of course, that all attempts to clarify this paradox satisfactorily 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 ».

We see that EINSTEIN gives essentially three reasons for his original belief in assumption (*ii*), noting that, like two of the laws of thermodynamics, this assumption is a « principle of impotence », to use E. T. WHITTAKER'S locution [11]. EINSTEIN'S seemingly distinct three reasons are the following: (1) « on the basis of experience », there are no « stationary » light waves, (2) Neither are there any such phenomena on the basis of MAXWELL'S equations, and (3) At the very beginning, there was intuitive clarity that preferred inertial systems do not exist, the laws of physics, including those of light propagation, being the same in all of them. These three reasons invite the following corresponding three comments.

1. The failure of our experience to have disclosed the existence of stationary light waves is not, of course, presumptive of their non-existence, unless that experience included the circumstances requisite to our observation of such waves if they do exist. What could such circumstances be? Suppose it *were* physically possible for a star to recede from the earth at the speed c of light. Assuming that there actually is such a star, postulate

(11) Cf. E.T. WHITTAKER, *From Euclid to Eddington*, Cambridge, 1949, §25, pp. 58-60.

further that the speed of the light emitted by the star in the direction of the earth is c relatively to the *star*, the light's speed relatively to the earth being given by the *Galilean-Newtonian* velocity addition and hence being *zero*. Then the earth would maintain a *constant distance* from the light wave. And if there were a way for us to register the presence of that stationary light wave, then we could have evidence of its existence. *Mutatis mutandis*, a light source in the laboratory moving at the velocity c might have produced the same kind of phenomenon.

Perhaps EINSTEIN envisaged these kinds of conditions as situations in which our experience ought to have disclosed the existence of stationary light waves.

If so, one wonders, however, how much weight he actually attached to this observational argument on behalf of assumption (*ii*). For he was undoubtedly cognizant of the contingency of the conditions governing the observable occurrence of the phenomenon in question. In particular, it should be noted that EINSTEIN'S mention of «the basis of experience» in this context *cannot* be assumed to be referring to the 1902-1906 experiments by KAUFMANN and others on the deflection of electrons (β-rays) in electric and magnetic fields. For if we suppose him to have been familiar with these experiments, they must have left him in a quandary precisely in regard to the truth of assumption (*ii*): while yielding a mass variation with velocity incompatible with NEWTONIAN dynamics, the results of these experiments were unable to rule out the formulae of *Abraham's* dynamics, which *allowed* particle velocities *exceeding* the velocity of light *in vacuo* [12].

John STACHEL has made the interesting suggestion that the clue to EINSTEIN'S reference to «the basis of experience» may be found in his commentary on the significance of the results of FIZEAU'S experiments on the velocity of light in moving liquids. Writing concerning the empirical formula codifying FIZEAU'S results, EINSTEIN says [13]: «From the stated formula, one can make the interesting deduction that a liquid which does not refract light at all ($n=1$) would not affect the propagation of light with respect to it even if the liquid is moving».

2. Can stationary light waves be regarded as ruled out by MAXWELL'S equations, if one does not already accept the principle of relativity, which guarantees the validity of the usual form of MAXWELL'S equations in all inertial systems? In other words, can the *second* of EINSTEIN'S avowed reasons for his dismissal of the possibility of stationary light waves be

(12) For details, cf. M. von LAUE, *Die Relativitätstheorie*, Branschweig, 1952, vol. I., pp. 26-27.

(13) «Die Relativitätstheorie» in: *Physik*, edited by E. WARBURG, Leipzig, 1915 p. 704.

regarded as other than a logical consequence of the third? Clearly, if MAXWELL'S equations *are* coupled with the principle of relativity, then these equations indeed rule out stationary light waves in every inertial system. But since MAXWELL'S equations are not covariant under *Galilean* transformations, it is far from clear that stationary light waves are precluded by the form assumed by the equations in an inertial system S moving with the velocity c relatively to the primary (aether) frame K *and* having coordinates which are related by the *Galilean* transformations to those of the K-system. Since EINSTEIN does not mention the «Galilean transform» of MAXWELL'S equations, it would seem that the only reason why he felt justified in regarding MAXWELL'S equations as support for his repudiation of stationary light waves was that he had already assumed the principle of relativity on intuitive grounds.

3. In view of the presumably flimsy character of the appeal to experience and of the redundancy of (2) with (3) among the reasons given by EINSTEIN, we are pretty much left with his intuitive confidence in the principle of relativity as the basis for his assumption of (*ii*).

We must emphatically *reject* the historical corollary of the not uncommon but altogether erroneous belief that the assertion of the limiting character of the velocity of light *in vacuo* depends on the relativistic velocity addition laws for its deduction. The following are compelling reasons for the *falsity* of this belief and hence of its historical corollary that EINSTEIN arrived at assumption (*ii*) only after deducing the formulae for the composition of velocities.

He deduced the velocity addition laws in § 5 of his 1905 paper via the mediation of the LORENTZ transformations from the two basic principles of his § 2, i. e., from the principle of relativity and the principle of the constancy of the speed of light. Now, the latter principle presupposes, as he notes *pointedly* at the start of § 2, that the *one*-way transit-time ingredient in the *one*-way velocity of light is based on *the definition of simultaneity given in his* § 1. Since the deduction of the velocity addition laws thus presupposes the *denial* of absolute simultaneity, it clearly presupposes as *one* of its premisses the limiting character of the velocity of light *in vacuo*. Furthermore, the relativistic formulae for the composition of velocities show only that *if* each of the velocities to be added does *not* exceed c, then the addition of them will *not* result in a velocity greater than c; these formulae do *not* themselves show that velocities greater than c are physically impossible, as is inferred fallaciously by E. T. WHITTAKER in the following statement by him [14]: «We see that *any*

[14] E. T. WHITTAKER, *A History of the Theories of Aether and Electricity*, vol. 2, London, 1953, p. 38. WHITTAKER'S error is also committed in R. C. TOLMAN'S *Relativity, Thermodynamics and Cosmology* (Oxford, 1934), p. 26.

velocity compounded with c gives as the resultant c over again, and therefore [*sic!*] that no velocity can exceed the velocity of light». That WHITTAKER'S inference of the impossibility of velocities in excess of the speed c of light from the velocity addition laws is indeed a *non-sequitur* becomes further apparent from the following considerations. If an object has components of velocity u'_x, u'_y, and u'_z in an inertial system K' moving with the velocity v along the positive x-axis of another inertial system K, then the corresponding components of velocity u_x, u_y and u_z in K are:

$$u_x = \frac{u'_x + v}{1 + u'_x v/c^2},$$

$$u_y = \frac{u'_y \sqrt{1 - v^2/c^2}}{1 + u'_x v/c^2},$$

$$u_z = \frac{u'_z \sqrt{1 - v^2/c^2}}{1 + u'_x v/c^2}.$$

Whatever may be the components of velocity in one system at a given time, these transformation equations relate them to the corresponding components in the other system. This is shown by the behavior of the function $u_x = f(u'_x, v)$ for values of the *independent* variable u'_x which *exceed c,* values whose exclusion from the range of u'_x clearly requires grounds *other than* the above velocity transformations. Thus, if $u'_x = 2c$ while $v = c/2$, then $u_x = 5c/4$. And the super-c values of the component velocities which are allowed by the velocity addition laws are, in fact, likewise allowed by the special theory of relativity as a whole *provided* that these velocities pertain to propagations in which no causal influence, energy or matter are transmitted at super-c speeds. It is *immaterial* that the above equations for the components u_y and u_z do *rule out* that *the system* K' have a relative velocity v in excess of c. For this result does *not* preclude that accelerating objects have time-dependent *super-c* component velocities in K'.

We are now in a position to make a conjecture as to EINSTEIN'S grounds for assumption (*i*). We recall that (*i*) asserts that within the class of events, material clocks do *not* define relations of absolute simultaneity under transport. As EINSTEIN states explicitly both in our citation from his intellectual autobiography and in his formulation of the principle of the constancy of the speed of light in § 2 of the 1905 paper, *the absolutistic conception of simultaneity* was the GORDIAN knot obstructing the resolution of his boyhood paradox, i. e., the reconciliation of the two basic principles of his § 2. But, assumption (*i*) was *required* no less than (*ii*) for the denial of absolute simultaneity! Hence his confidence in (*i*) must be presumed to have derived from his belief in the correctness of *both* of the two principles in his § 2. One wonders in this connection what role, if any, was played in the development of EINSTEIN'S reflections on simultaneity by POINCARÉ'S *obscurely* conventionalist treatment of simultaneity in his paper «La Mesure du Temps», published in 1898 in vol. 6 of the *Revue de Métaphysique et de Morale*, pp. 1–13.

Would it be safe to conclude from EINSTEIN'S autobiographical statement that actual experimental results bearing on the velocity of light *in vacuo* and on its status as a limiting velocity in fact played no role at all when he groped his way to an espousal of the principle of relativity? If so, there would be the following serious question: can the theoretical guesses of an EINSTEIN be regarded to have been genuinely more *educated*, as opposed to just more *lucky*, than the abortive phantasies of those quixotic scientific thinkers whose names have sunk into oblivion and whose subjective confidence in having made a *bona fide* discovery was no less passionate than was EINSTEIN'S? This important question, which I raised in an earlier publication (15), was not answered but merely evaded by M. POLANYI, who-on the strength of the widsom of a half century of hindsight!-offers the following *petitio principii* in criticism of me: «Dr. GRÜNBAUM ... seems repeatedly to express the view that EINSTEIN had no 'sufficient reason to adopt the fundamental assumptions of relativity when he in fact did so. But he [GRÜNBAUM] is more puzzled by this than I am, for he does not allow for the unspecifiable clues which justifiably [*sic*] guided EINSTEIN'S formulation» (16). But this declaration immediately prompts the further question of just how POLANYI'S account illuminates the epistemological (methodological) attributes of a *bona fide* scientific discovery as opposed to those of a plausible and cherished but abortive speculation, while maintaining, as he does, both the *fallibility* and the *unspecifiability* of the clues on which scientific theorists rely. Flying the banner of unspecifiability of clues while heaping scorn on the demand for adequate empirical support of knowledge claims in physics does not absolve POLANYI from the necessity of answering this further question. For example, what besides his present, inherently *ex post facto* possession of the wisdom of hindsight enables POLANYI'S unspecifiability thesis to discriminate in regard to methodological justifiability between the following two hypotheses at the time of their initial enunciation: (*i*) EINSTEIN'S assumption that the velocity of light is independent of the velocity of the emitting source, which has sub-

(15) Cf. A. GRÜNBAUM, *The Genesis of the Special Theory of Relativity*, in: *Current Issues in the Philosophy of Science*, (ed. Feigl & Maxwell), *op. cit.*, p. 49.

(16) M POLANYI, «Notes etc.», pp. 54-55.

sequently turned out to be successful (H. DINGLE'S allegations to the contrary notwithstanding) [17], *(ii)* RITZ'S contrary hypothesis, which, to the profound disappointment and chagrin of his followers, turned out to be unsuccessful after his death in 1909? If POLANYI'S answer were to be that each of these contrary hypotheses was indeed methodologically justified in the context of its own set of unspecifiable but fallible clues, then his account altogether loses its relevance to its avowed subject. For it then throws no light whatever on the epistemology of scientific discovery as distinct from the psychology of imaginative but pathetically abortive speculation. The conclusion seems inescapable that if POLANYI is at all to avoid the well-known aprioristic pitfalls of classical rationalism, his conception of the logic of scientific discovery compels him to seek refuge in the unspecifiability of clues as an *asylum ignorantiae.* And his answer to my question as to what, besides good fortune, does distinguish *methodologically* between scientific discoveries of genius and brilliant flights of scientific fantasy merely baptizes the difficulty which I raised and gives it the name «unspecifiability».

But is it true that actual experiments on the velocity of light such as the MICHELSON-MORLEY experiment did *not* play any genetic role in RT? E. T. BELL, writing *before* the publication of EINSTEIN'S «Autobiographical Notes», and referring to the influence of the MICHELSON-MORLEY experiment on EINSTEIN, claims that «he has stated explicitly that he knew of neither the experiment nor its outcome when he had already convinced himself that the special theory was valid» [18]. And M. POLANYI reports [19] that EINSTEIN had authorized him in 1954 to publish the statement that «the MICHELSON-MORLEY experiment had a negligible effect on the discovery of relativity».

Yet if *both* of the reasons for RT adduced by EINSTEIN in the *Introduction* to his 1905 paper were among the factors which had prompted his initial espousal of RT, the consonance of the foregoing claims by BELL and POLANYI with EINSTEIN'S own text there is quite problematic. For in that *Introduction*, EINSTEIN cites the following *two* considerations as suggesting «that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest» [20]: (1) the lack of *symmetry* in the classical electrodynamic treatment of a current-

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(17) Cf. A. GRÜNBAUM, «Professor DINGLE on Falsifiability: A Second Rejoinder», *British Journal for the Philosophy of Science*, vol. 12, August, 1961, pp. 153-156.

(18) E. T. BELL, *The Development of Mathematics*, New York, 1945, p. 210.

(19) M. POLANYI, *op. cit.*, pp. 10-11.

(20) PR, p. 37; italics are mine.

carrying wire moving relatively to a magnet at rest, on the one hand, and of a magnet moving relatively to such a wire at rest, on the other, (2) «*the unsuccessful attempts to discover any motion of the earth relatively to the «light medium»* [aether]». Unless they provide some other consistent explanation for the presence of the *latter* statement in EINSTEIN'S text of 1905, it is surely incumbent upon all those historians of RT who *deny* the inspirational role of the MICHELSON-MORLEY experiments to tell us *specifically* what *other* «unsuccessful attemps to discover any motion of the earth relatively to the "light medium"» EINSTEIN had in mind here. And it was likewise incumbent upon HOLTON to tell us how he reconciles this explicit reference to experiments, whose identity was presumably clear to EINSTEIN'S public in 1905, with the following claims (21): EINSTEIN'S paper «begins with the statement of formal asymmetries or other incongruities of a predominantly esthetic nature (rather than, for example, a puzzle posed by unexplained experimental facts)», [p. 629] and «The [1905 RT] paper does not invoke explicitly any of the several well-known experimental difficulties» [p. 630]. This obligation to take cognizance of the second consideration in the *Introduction* to the 1905 paper should also have been shouldered by the mature *reminiscing* EINSTEIN himself when authorizing the statement given by POLANYI.

All the more so, since, as early as 1915, EINSTEIN himself gave the following *historical* account in his contribution «Die Relativitätstheorie» to the volume *Physik* (22) «It is hardly possible to form an independent judgment of the justification of the theory of relativity, if one does not have some acquaintance with the experiences and thought processes which preceded it. Hence these will need to be discussed first. [p. 703] ... The successes of the LORENTZIAN theory were so significant that the physicists would have abandoned the principle of relativity without qualms, had it not been for the availability of an important experimental result, ..., namely MICHELSON'S experiment». [p. 706] It is quite unfortunate that the serious *prima facie* discrepancies between EINSTEIN'S testimony of 1905 and 1915, on the one hand, and of 1954 (as transmitted to POLANYI via Dr. N. BALAZS), on the other, did not restrain POLANYI from making the following assertion or at least induce him to modify it: «When EINSTEIN discovered rationality in nature, unaided by any observation that had not been available for at least fifty years before, our positivistic textbooks promptly covered up the scandal by an appropriately embellished account

(21) HOLTON, *op. cit.*, p. 629 and p. 630.
(22) *Op. cit.*, pp. 703 and 706.

of his discovery», i.e., by falsely portraying relativity «as a theoretical response to the MICHELSON-MORLEY experiment» ([23]).

Conceivably our historical conundrum is resolved by Professor HENRY S. FRANK'S suggestion that when writing his RT paper for the scientific public of 1905, EINSTEIN deemed it appropriate to appeal also to the failure of the aether theory in the MICHELSON-MORLEY experiments as a *justification* of RT, even though that failure had genetically *not* been a reason for his own initial confidence in RT. But this suggestion leaves us just as puzzled concerning the logical, as distinct from psychological grounds which would then originally have motivated EINSTEIN to have confidence in the principle of relativity *without* the partial support of the MICHELSON-MORLEY experiment, while that very lack of support would have sufficed, by his own admission, to assure the abandonment of the principle «without qualms» by his colleagues. And our puzzlement is deepened by BERNARD JAFFE'S report [*Michelson and the Speed of Light*, Garden City, New York, 1960, pp. 100-101] that in a letter to him, «EINSTEIN expressed his debt to the American physicist [i. e., MICHELSON] in these words:

«It is no doubt that MICHELSON'S experiment was of considerable influence upon my work insofar as it strengthened my conviction concerning the validity of the principle of the special theory of relativity. On the other side I was pretty much convinced of the validity of the principle before I did know this experiment and its result. In any case, MICHELSON'S experiment removed practically any doubt about the validity of the principle in optics, and showed that a profound change of the basic concepts of physics was inevitable».

In 1931, just before the death of MICHELSON, EINSTEIN publicly attributed his theory to the experiment of MICHELSON».

Oddly enough, POLANYI quotes EINSTEIN'S passage about the non-detection of the earth's aether-motion just after having declared that the outcome of the MICHELSON-MORLEY experiment was «on the basis of pure speculation, rationally intuited by EINSTEIN before he had ever heard about it».

On this, my comments are the following. Suppose it *were* clear, which it is not by any means, that EINSTEIN'S frustratingly *non*-specific mention of attempts to confirm the aether theory experimentally is to be explained by Professor H. S. FRANK'S suggestion and is thus consonant with POLANYI'S discounting of the MICHELSON-MORLEY experiment. Then we would still have to emphasize that this historical conjecture cannot be

([23]) M. POLANYI, *op. cit.*, p. 11.

used to vitiate an account of both the genesis and justification of scientific theory which has the following two characteristics: (1) while being *empiricist,* it is broad enough to accommodate the *valid core* of the KANTIAN emphasis on the *active,* creative role of the scientific imagination in the postulational elaboration of hypothetico-deductive theories, and (2) by being empiricist, it avoids the notorious aprioristic pitfalls of classical rationalism, pitfalls on whose brink POLANYI hovers [24] in the protective twilight of his thesis of the unspecifiability of fallible clues. Can the history of RT as conjectured by POLANYI be validly adduced here to prove any more than the untenability of a *crudely* empiricist MACHIAN or ARISTOTELIAN-THOMIST, abstractionist account of theory-construction as a mere codification of the results of experiments? And is the untenability of the latter kind of account not fully recognized by any empiricist conception of scientific knowledge which incorporates the lesson of KANT? Does such an empiricist conception not allow for the difference between knowing of an experimental result in a narrow sense, on the one hand, and speculatively assigning a wider significance to it, on the other?

That the answer to the last two of these questions is indeed «yes» is indicated by a single chapter title in a book by an empiricist writer whose views are presumably anathema to POLANYI: chapter 6 of REICHENBACH's *Rise of Scientific Philosophy* is entitled «The Twofold Nature of Classical Physics: Its Empirical and Its Rational Aspect.»

In a further attempt to document the fideist nature of allegiance to physical theories, POLANYI refers to the scientific community's reception of D. C. MILLER's reports of *positive* effects obtained by him in his repetitions of the MICHELSON-MORLEY experiment. Thus, POLANYI cites the history of RT to maintain further that once the «rational» appeal of the theory had captured the minds of the scientific community, contrary evidence hardly acted as a check on its subsequent acceptance. But POLANYI's indictment scores only against those who mistakenly portray evidence-reports as not already theory-laden. For it is the false absolute dichotomy between theory and evidence which would prompt an uncritically hasty *disconfirmatory* use of what is *prima facie* contrary evidence. As DUHEM has emphasized, *collateral* hypotheses pertaining to the conditions of an experiment and to the laws of operation of its test equipment are an essential ingredient in the logical fabric of disconfirmation. [25] And thus there is a mutuality of accreditation between theory and evidence in virtue of the interpenetration of the

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(24) M. POLANYI, *op. cit.*, p. 15.

(25) This is *not* to say that DUHEM's thesis of unavoidably inconclusive falsifiability of part of an *explanans* is correct: cf. A. GRÜNBAUM, «The Duhemian Argument», *Philosophy of Science*, 27 (1960), pp. 75-87.

criteria of credibility which certify evidence as *bona fide*, on the one hand, and theory as evidentially warranted on the other.

It was therefore hardly a case, as POLANYI would have it, of simply ignoring negative evidence when adherents of RT surmised — correctly as it seems to have turned out [26] — that the conditions under which MILLER obtained his results were different from what he had supposed them to be. And there was also the inductively-reasonable suspicion that the CLEVELAND site at which MILLER had obtained his aberrant results was *not* a terrestrial singularity in regard to delicate optical phenomena, since it had *not* proven to be a singularity with respect to other (coarse) physical phenomena.

We must examine a final but essential point in POLANYI's critique of the account given by philosophers of science of the relevance of the discovery of RT to an analysis of its epistemological foundations. POLANYI denies the legitimacy of the distinction between the *psychology* and the epistemology (logic) of discovery, a distinction which REICHENBACH usefully termed as being between the «context of discovery» and the «context of justification». Quoting H. MEHLBERG's statement that «The gist of the scientific method is ... verification and proof, not discovery», POLANYI writes: «Actually, philosophers deal extensively with induction as a method of scientific discovery; but when they occasionally realize that this is not how discoveries are made, they dispose of the facts to which their theory fails to apply by relegating them to psychology». [27] And while denying that he intends to discount completely the role of experimental results in the genesis of acts of discovery [28], POLANYI does make the following claims concerning the criteria by which *bona fide* scientific discoveries can legitimately be judged as such:

> «Knowledge of the external world is in general acquired by relying on clues which cannot be fully identified. [29] ... Scientific discoveries are likewise based on clues that are never fully specifiable. This may be called «intuition» but I rarely use this term, for it is traditionally charged with the fallacious connotation of infallibility, which I do not wish to imply. [30] ... A good problem is a passionate intimation of a hidden truth; [31] ... I recognize the continued operation of these anticipatory powers in the verification of discovery and holding of knowledge. The scientist's conviction of having arrived at some true knowledge is akin to the powers by which he recognises a problem. It is an anticipation of yet hidden truths,

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(26) Cf. SHANKLAND, MC CUSKEY, LEONE, and KUERTI, *Rev. Modern Physics*, **27**, 167 (1955).
(27) POLANYI, PK, p. 14 n. 1
(28) POLANYI, «Notes etc.», p. 55.
(29) *Ibid.*, p. 53.
(30) *Ibid.*, p. 54.
(31) *Idem.*

which in this case are expected to emerge in the uncertain future as the unknown consequences of the truth as known at present. When we are told that fruitfulness is the characteristic of a true discovery, the actual facts are obscured. For it is absurd to suggest that we should recognise truth by the wealth of its still unknown future consequences; on the contrary, our recognition of a true piece of knowledge is an anticipation of such unknown consequences, and if we acknowledge that knowledge is rightly held we subscribe to these anticipations. They are an expression of the belief that true knowledge is an aspect of a hidden reality which as such can yet reveal itself in an indeterminate range of future discoveries». ([32])

Given his affirmation of the unspecifiability of clues and his admission of their fallibility, this statement of POLANYI's succeeds no more than his earlier ones in providing a consistent articulation of the *epistemological* attributes of a *bona fide* discovery as contrasted with those of an initially plausible, passionately espoused but wholly abortive speculation. But in the absence of precisely such an articulation, what is to be our verdict on his indictment of MEHLBERG, REICHENBACH and others who do invoke the distinction between the *psychology* of the propounding of scientific hypotheses, on the one hand, and the *epistemological justification* of these hypotheses, on the other? It can be none other than that POLANYI's indictment is altogether gratuitous. To repeat, the anticipations and commitments of which he speaks are initially no less passionate in cases of hypotheses which turn out to be untenable than in cases of theoretical conjectures which are abundantly borne out by subsequent evidence. And hence those of POLANYI's remarks which are perceptive pertain *not*, as he believes, to scientific discovery; instead, they do apply after all only to the *psychology* of the propounding of scientific speculations. For what is it that makes a hypothesis *true*, and what warrants the claim that a *bona fide* discovery has actually been made? Surely it is *not* the fact that the scientist has *committed* himself to the as yet unknown and unverified anticipations flowing from the hypothesis! When maintaining that fruitfulness is the test of a true discovery, philosophers of science are *not* asserting, as POLANYI supposes incorrectly, that we *recognize* truth «by the wealth of its still unknown consequences». Fruitfulnes in the form of both available evidence *and* future corroborations is indeed the test of the truth of a hypothesis. But we *recognize* the latter truth at a given time on the basis of supporting evidence then available, this recognition being understood to require the certification of credibility instead of being constituted by some ineffable, thaumaturgical insight.

We conclude against POLANYI that the philosopher's relegation of the

([32]) *Idem.*

promptings of what turn out to be lucky hunches to the psychology of creative work in science is *not* an example of the fallacy of ignoring negative instances. As well condemn the avowed neglect by the literary critic of FRIEDRICH SCHILLER's reported use of the smell of rotten apples as an aid to his writing of DIE GLOCKE.

§ 3. Was the Aether-Theoretic Lorentz-Fitzgerald Contraction Hypothesis Ad Hoc in the Logical or Only in the Psychological Sense?

An auxiliary hypothesis, introduced in response to new evidence embarrassing to an established theory, is *ad hoc* in the *logical* sense within the framework of the theory modified by it, if it does *not*, in principle, lend itself to any independent test whatever. Now suppose that a certain collateral hypothesis such as the aether-theoretic LORENTZ-FITZGERALD contraction hypothesis is actually independently testable, at least in principle, but that its advocates *fail* to be *aware* of such testability and espouse it nonetheless. Clearly, the auxiliary hypothesis in question can then *not* be held to be logically *ad hoc* but can then be regarded as *ad hoc* only in the *psychological* sense. For the methodological culpability of those who espouse such an auxiliary hypothesis despite their own (mistaken!) belief that it is not independently testable cannot detract from the actual, purely logical independent testability of the hypothesis in the context of its theoretical framework. Thus, we must distinguish between the logical and psychological senses of being *ad hoc,* much as we distinguish between the logical property possessed by a mathematical proposition which is a theorem within a given axiom system and the psychological attribute of mathematicians who *realize* that the given proposition is indeed a theorem. The latter distinction invalidates the claim that a certain mathematical proposition cannot be a theorem in a given axiom system merely because all mathematicians are *unaware* that the proposition is, in fact, provable. Analogously, it is a serious error to infer that the aether-theoretic LORENTZ-FITZGERALD contraction hypothesis is logically *ad hoc* on the strength of having shown *historically* that its proponents were unable to envision an independent test of it. (33) There is widespread unawareness that the latter inference is not only a *non-sequitur* but also presents us with a demonstrably false conclusion. I shall therefore now show

(33) Precisely this error was committed recently in a paper by H. DINGLE in *The British Journal for the Philosophy of Science,* vol. 10, 1959, pp. 228-229. DINGLE does adduce historical evidence which establishes the biographical fact that, in the case of LORENTZ, the contraction hypothesis was *psychologically ad hoc.*

that the original LORENTZ-FITZGERALD contraction hypothesis is *not* logically *ad hoc* in the context of the aether theory. (34)

A comparison of the reasoning underlying the KENNEDY-THORNDIKE experiment (35) with the design of the MICHELSON-MORLEY experiment will serve to establish that the aether-theoretic LORENTZ-FITZGERALD contraction hypothesis (hereafter referred to as the L-F hypothesis) did indeed have independently falsifiable consequences and that it was, in fact, falsified by the null outcome of the KENNEDY-THORNDIKE experiment. Specifically, our analysis will show that (*i*) although the coupling of the L-F hypothesis with the classical aether theory entails the null outcome produced by the MICHELSON-MORLEY experiment, it does *rule out* the negative result which was actually yielded by the KENNEDY-THORNDIKE experiment, and (*ii*) the aether theory as *modified* by the L-F hypothesis entails a positive outcome of the KENNEDY-THORNDIKE experiment *differing quantitatively* from the positive result required by the aether theory *without* the L-F hypothesis.

The essential difference between the apparatus used in these two experiments is the following: as measured by rods in the laboratory, the horizontal and vertical arms of the MICHELSON interferometer used in the KENNEDY-THORNDIKE experiment are *not* equal but are made as *different* in length as possible, so as to assure a considerable difference in the travel times of the two partial beams from the source to the point at which they recombine to produce interference fringes. On the other hand, the horizontal and vertical arms of the apparatus used in the MICHELSON-MORLEY experiment are *each* of the *same* length l as measured by the rods in the laboratory.

Accordingly, in the *latter* experiment the classically-expected round-trip times T_v and T_h for the vertical and horizontal arms are given respectively by

$$T_v = \frac{2l}{\sqrt{c^2 - v^2}} \text{ and } T_h = \frac{2l}{\sqrt{c^2 - v^2}} \cdot \frac{1}{\sqrt{1 - \beta^2}},$$

where c represents the velocity of light, v the velocity of the apparatus relatively to the aether, and $\beta \equiv \frac{v}{c}$. Now, without a LORENTZ-FITZGERALD contraction hypothesis, the initial time difference $T_h - T_v$ between the two partial light beams would be expected to change in the course of the rotation of the apparatus through 90° in the MICHELSON-MORLEY experiment. And thus

(34) For my earlier discussion of this issue, containing also a detailed rebuttal of H. DINGLE's views, see A. GRÜNBAUM, *The British Journal for the Philosophy of Science*, vol. 10 (1959), pp 48-50; vol. 11 (1960), pp. 143-145; vol. 12 (August, 1961), pp. 153-156.

(35) Cf. R. J. KENNEDY and E. M. THORNDIKE, *Physical Review*, 42, 400 (1932), and W. PANOFSKY and M. PHILLIPS, *Classical Electricity and Magnetism*, Cambridge, 1955, p. 236.

a shift in the interference fringes corresponding to this change was anticipated. But, once the aether theory is amended by the introduction of the LORENTZ-FITZGERALD contraction, the length l in the expression for T_h must be replaced by the length $l\sqrt{1-\beta^2}$. As a consequence of the introduction of this auxiliary hypothesis, T_h becomes *equal* to T_v and the difference between the round trip times of the two partial beams becomes zero throughout the MICHELSON-MORLEY experiment, in conformity with its null result.

It will be noted that the equality of the *terrestrially-measured* lengths of the two arms is a *necessary condition* for the constant vanishing of the difference between the two round trip times in the LORENTZ-FITZGERALD account of the MICHELSON-MORLEY experiment. But precisely this necessary condition is *not* fulfilled in the KENNEDY-THORNDIKE experiment in which the *terrestrially-measured* lengths of the vertical and horizontal arms have the *unequal* values L and l respectively. Thus, *upon assuming the Lorentz-Fitzgerald contraction*, the difference between the travel times of the two light beams of the KENNEDY-THORNDIKE experiment is given by

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

Instead of vanishing throughout the expriment, this time difference *varies* with the diurnally and annually changing velocity v of the apparatus relatively to the fixed aether. Moreover, if we do *not* assume a LORENTZ-FITZGERALD contraction, the difference between the two travel times of the KENNEDY-THORNDIKE experiment has the *different* value given by

$$T_v - T_h = \frac{2L}{\sqrt{c^2-v^2}} - \frac{2l}{\sqrt{c^2-v^2}} \cdot \frac{1}{\sqrt{1-\beta^2}} = \frac{2}{\sqrt{c^2-v^2}}\left(L - \frac{l}{\sqrt{1-\beta^2}}\right).$$

And this difference is likewise a function of the diurnally and annually varying velocity v of the apparatus.

Under what conditions can the changing time-difference called for by the L-F modification of the aether theory be expected to give rise to corresponding observable shifts in the interference fringe pattern? On that theory, these fringe shifts should occur if the clocks in the moving system are presumed to have the *same* rates as the «true» clocks of the aether-system and if the *frequency* (period) of the light source — as measured by *either* the moving clocks *or* the aether-system clocks — does *not* itself depend upon the changing velocity of the apparatus with respect to the aether. But since

KENNEDY and THORNDIKE found that the expected fringe shifts failed to materialize, the crucial question before us is whether the L-F hypothesis is falsified by this null result even though the positive result expected on the basis of the L-F form of the aether theory depended for its deduction also upon the assumption that the frequency of the light source is *independent* of its velocity through the aether. Hence we must inquire whether the null result of the KENNEDY-THORNDIKE experiment could be explained by *denying* the latter assumption of independence while *preserving* the L-F hypothesis, so that this hypothesis would then not be falsifiable by this experiment.

Now, the denial of the assumption of independence could take either of the following two forms with a view to explaining the null result of KENNEDY and THORNDIKE while *affirming* the L-F hypothesis:

(*a*) The velocity-dependent time difference $T_v - T_h$ given above, which can be expressed as

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

does *not* give rise to fringe shifts, because the frequency of the moving source does depend on its velocity through the aether as follows: both its frequency as determined by the clocks of the aether system and its frequency as measured by the clocks of the *moving* system, the *latter* of which we shall call the «proper» frequency for brevity, are reduced by a factor of $\sqrt{1 - \beta^2}$ as compared to the frequency that would be measured in the aether system, if the moving source came to rest there. Accordingly, *an observational consequence of this way of denying the independence of the frequency from the velocity is that the proper frequency of the moving source would vary with its velocity v (and hence with* β).

(*b*) The velocity-dependence of the time-difference $T_v - T_h$ given under (*a*) does not issue in a fringe shift, because there is also the following compensatory velocity-dependence: both the frequency of the source, as measured by the *aether*-system clocks, *and* the rate of the clocks in the moving system, as compared to the clocks in the aether system, are *reduced* by a factor of $\sqrt{1 - \beta^2}$. Thus, the *proper* frequency is now *independent* of the velocity of the source, and the values of the round-trip times as measured in the *moving* system are given by the quantities

$$T'_v = \frac{2L}{c} \text{ and } T'_h = \frac{2l}{c},$$

which are independent of β.

We are now ready to state the grounds for a unique choice of the *third* from among the following three rival interpretations of the null result of the KENNEDY-THORNDIKE experiment:

(*i*) The L-F hypothesis is confirmed *and* the observable *proper* frequency of the moving source *varies* with its velocity,

(*ii*) The L-F contraction *and* the «time-dilation» (or «dilatation» i. e., reduction of the rate of the moving clocks) operate together while the *proper* frequency of the source is *constant*, and hence the compound auxiliary hypothesis comprising *both* the L-F contraction *and* the time-dilation is confirmed,

(*iii*) The L-F contraction hypothesis is *falsified* by the outcome of the KENNEDY-THORNDIKE experiment in the sense of being (highly) disconfirmed. And since the KENNEDY-THORNDIKE experiment constitutes a test which is independent of the MICHELSON-MORLEY experiment, the former experiment shows that the espousal of the L-F hypothesis in response to the negative result of the MICHELSON-MORLEY experiment was not logically *ad hoc*, although it happened to have been psychologically *ad hoc*.

The first interpretation is to be rejected: its claim that the observable proper frequency of the moving source varies with the velocity is testable *apart* from the KENNEDY-THORNDIKE experiment and has been found to be false by separate empirical evidence. The issue is therefore whether the fact that interpretation (*ii*) is consistent with the null outcome of the KENNEDY-THORNDIKE experiment renders (*ii*) a *methodologically* acceptable interpretation and inductively a legitimate *alternative* to interpretation (*iii*). That (*ii*) does not, however, pass muster at all methodologically and hence cannot qualify as a rival to (*iii*) emerges from the fact that the *combined* auxiliary hypothesis affirming *both* the L-F contraction *and* the time-dilation is plainly *ad hoc* by not lending itself to any independent test whatever. Hence, despite its compatibility with observational findings, methodological grounds prompt us to reject the doubly-amended variant of the aether theory offered by (*ii*). And it is now apparent that the attempt to endorse interpretation (*ii*) in order to rescue the L-F hypothesis from the refutation which is claimed by interpretation (*iii*) founders on the following fact: (*ii*) succeeds in preserving the L-F hypothesis from refutation by the null outcome of the KENNEDY-THORNDIKE experiment only by dint of incorporating that hypothesis in an augmented auxiliary hypothesis which is indeed logically *ad hoc*.

We have shown, therefore, that instead of being logically *ad hoc*, the L-F hypothesis has a falsifiable bearing on the outcome of the KENNEDY-THORNDIKE experiment, an experiment which could have detected any existing velocity of the apparatus relatively to the aether even on the assumption of a L-F contraction. And we note that the invalidation by the KENNEDY-THORNDIKE experiment of the charge that the L-F hypothesis is logically *ad hoc* is *internal to the aether theory*: this invalidation no more depends logically on the availability of the special theory of relativity than does the refutation of the *original* aether theory by the null outcome of the MICHELSON-MORLEY experiment.

In addition to the KENNEDY-THORNDIKE experiment, the following experiment suggested by C. MØLLER and others qualifies as a test of the L-F contraction within the framework of *pre*-relativistic conceptions: although a *terrestrial* observer could *not* detect a L-F contraction in a MICHELSON-MORLEY experiment, «an observer at rest in the aether outside the earth would, however, in principle, be able to observe the shortening and he would find the earth and all objects on the earth contracted in the direction of motion of the earth». [36]

§ 4. Philosophical Mastery of RT As A Safeguard and Corrective Against Historical Errors In Accounts of Its Genesis.

We saw that the history of RT (or of science generally), in addition to its intrinsic interest, can contribute to the provision of an epistemological analysis of RT (or of science). But it must be emphasized that no historically correct, let alone illuminating, account of the genesis of RT can be furnished without a thorough prior comprehension of the *philosophical* conceptions underlying that theory and distinguishing it from its ancestors. The capacity of philosophical mastery of RT to preclude historically unsound assessments of the contributions to it made by EINSTEIN's precursors (LORENTZ, POINCARÉ, FITZGERALD, LARMOR, VOIGT *et al.*) can be well illustrated by a consideration of the role of philosophical blunders in conferring plausibility on E. T. WHITTAKER's disparaging estimate of EINSTEIN's contribution to RT.

Entitling his chapter on the history of the development of RT, «The Relativity Theory of POINCARÉ and LORENTZ», E. T. WHITTAKER gives the following depreciatory evaluation of EINSTEIN's role in the genesis of RT: [37] «In the autumn of the same year (1905), ... EINSTEIN published a paper which set forth the relativity theory of POINCARÉ and LORENTZ with some amplifi-

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(36) C. MØLLER, *The Theory of Relativity*, Oxford, 1952, p. 29. For a discussion of the *important differences* between the *pre*-relativistic and the relativistic conceptions of the status of any contraction disclosed by the extra-terrestrial experiment suggested here by MØLLER, cf. H. REICHENBACH, *The Philosophy of Space and Time*, New York, 1958, § 31, and A. GRÜNBAUM, «LFR», pp. 419-420.

(37) E. T. WHITTAKER, *A History of the Theories of Aether and Electricity*, New York 1953, vol. 2, p. 40.

cations, and which attracted much attention. ... In this paper EINSTEIN gave the modifications which must now be introduced into the formulas for aberration and the Doppler effect». This historical assessment of the range of EINSTEIN's originality and of the magnitude of his contribution to RT derives at least its plausibility (even if perhaps not its inspiration) from WHITTAKER's *philosophically incorrect* conception of *Einstein's interpretation* of the LORENTZ transformations. For, as *I* have shown in some detail elsewhere, (38) WHITTAKER committed grievous logical errors as follows: (*i*) he failed to be cognizant of EINSTEIN's philosophical *repudiation* of the LORENTZ-LARMOR-POINCARÉ distinction between «true» and «spurious» (apparent, local) times and lengths, a repudiation which renders EINSTEIN an authentic conceptual revolutionary of genius in *this* respect, and correlatively (*ii*) WHITTAKER made no allowance at all for the crucial *logical* differences between the pre-Einsteinian and Einsteinian conceptions of the status of any extraterrestrially observed contraction of a rod moving with the earth and pointing in the direction of the latter's motion.

Thus, only philosophical awareness of the fact that EINSTEIN's conception of the LORENTZ transformations is *not* to be construed along the modified aether theoretic lines of LORENTZ and POINCARÉ makes possible the discernment of the mistake in WHITTAKER's treatment of RT.

It is instructive, therefore, to examine the recent appraisal of WHITTAKER's treatment of the history of RT given by G. HOLTON as part of HOLTON's endeavor to sustain the following thesis of his: «it is through the dispassionate examination of historically valid cases that we can best become aware of the preconceptions which underlie all philosophical study». (39)

Preparatory to an evaluation of E. T. WHITTAKER's account of the genesis of EINSTEIN's ideas on RT, HOLTON invokes a *generalized* form of BOHR's principle of complementarity as a suggestive (though not prescriptive) analogy for dealing with conflicting historical reports of matters other than dates and places. Specifically, HOLTON presents his evaluation of WHITTAKER's treatment as a concrete illustration of the following maxim of historical inquiry championed by HOLTON: (40) «on larger and more qualitative questions (for example, the acceptance of the theory) one can profitably adopt the attitude that evidence obtained by biographical research under different points of view cannot be comprehended within a single picture, but must be regarded as complementary in the sense that *only the totality of the presentations exhausts the possible information about the subject.* This will be recognized as closely ana-

(38) Cf. GRÜNBAUM, LFR, § 5, pp. 418-425.
(39) HOLTON, *op. cit.*, p. 636.
(40) *Ibid.*, p. 633.

logous to one part of the complete statement of the complementarity principle in physics. [41] To look for an «independent» view in qualitative matters in any other way is likely to lead one to take merely a position equidistant between all others, or between the «isms» that motivate them».

Of course, to understand what factors did induce different interpreters to give conflicting accounts of the history of RT and of the significance of the conceptual and physical innovations wrought by EINSTEIN's RT, one must indeed take account of their prior intellectual loyalties and preconceptions. In this limited sense, a suitably guarded use of the eclecticism espoused by HOLTON may be fruitful in the utilization of source materials, and HOLTON employs it validly and enlighteningly, when he writes:

«the commentaries on the historical origins of the theory of relativity have tended to fall into two classes, each having distinguished proponents: the one views it as a mutant, a sharp break with respect to the work of the immediate predecessors of EINSTEIN; the other regards it as an elaboration of then current work, e. g, by LORENTZ and POINCARÉ.

To my mind, the Einsteinian innovation is understood best by superposition of both views, by seeing the discontinuity of methodological orientation within an historically continuous scientific development. [42] [43] ... while it is usually stressed that EINSTEIN challenged Newtonian physics in fundamental ways, the equally correct but neglected point is the number of methodological correspondences with earlier classics, for example, with the *Principia*». [44]

But HOLTON is *not* content to say that in *some* respects, EINSTEIN's RT constitutes a radical departure from its predecessors while in *others* it represents an elaboration or even resuscitation of earlier conceptions. The latter historical characterization of RT employs logical devices whose conceptual edge is blunted rather than sharpened by HOLTON's injection of the BOHRIAN complementarist analogy into his plea for eclecticism. For what EINSTEIN did in fact think at any given time, even if it was internally inconsistent, is wholly *independent* of the rival retrospectives of historians of RT, *contrary* to HOLTON'S *analogy with the quantum mechanical system.* Thus, by presenting his advocacy of eclecticism in the garb of historical complementarism, HOLTON presents us with an eclecticism so unbridled as to be a license for making genuinely contradictory claims about the content of EINSTEIN'S thought processes at a given time. That HOLTON'S analogy must be deemed obfuscatory, instead of suggestive, along with most

(41) «I employ it here as a suggestive, though not prescriptive, analogy».

(42) «G. HOLTON, IX *Congreso Internacional de Historia de las Ciencias, Guiones de las Comunicaciones* (Barcelona-Madrid, 1959), Vol. II., p. 41».

(43) G. HOLTON, «On the Origins of the Special Theory of Relativity», *op. cit.*, p. 628.

(44) *Ibid.*, pp. 630-631.

earlier attempts to extend quantum mechanical complementarity proper to other domains, (45) becomes clear from the following fact. Invoking his appraisal of WHITTAKER'S treatment, HOLTON regards it as amiss to inquire, independently of the conflicting perspectives of historians of RT, what philosophical conceptions actually did guide EINSTEIN when he developed RT, these conceptions allegedly not being comprehensible «within a single picture». And the reason HOLTON gives for this conclusion is *not* that EINSTEIN'S philosophical outlook was beset by internal inconsistencies, but rather the following (46): «a second problem involving divergent or contradictory views concerning a scientist's work ... is generated not by internal changes or conflicts, but by external ones. I can discuss this in the briefest way by pointing to the question of what one is to do with biographical works which are not in agreement». And then HOLTON makes the previously quoted recommendation of our adoption of a complementarist attitude toward incompatible historical accounts. Thus, the lack of agreement among historians of RT *about* what EINSTEIN did actually think inspires HOLTON to conceive of the historical task as follows: EINSTEIN'S thoughts or conceptions are the counterpart of the quantum mechanical system to be characterized, while the students of EINSTEIN'S work play the role of the measuring devices securing observational information concerning the quantum mechanical system, and otherwise logically incompatible accounts of the development of RT cease to be so in virtue of being affirmed only in the context of mutually exclusive perspectives.

That the acceptance of HOLTON'S recommendation of complementarism is productive of error and confusion rather than of catholicity of understanding, as he supposes, is shown clearly, it seems to me, by the incorrect verdict which it yields in regard to the merits of WHITTAKER'S assessment of EINSTEIN'S role in the development of RT. There can be no doubt that WHITTAKER'S disparaging estimate of the magnitude of EINSTEIN'S contribution to RT will be fully justified in the mind of someone who supposes *incorrectly* with WHITTAKER that EINSTEIN construed the LORENTZ transformations along the aether-theoretic lines of LORENTZ and POINCARÉ, these precursors having employed the non-Einsteinian distinctions between «true» and «local» times and between «real» and «apparent» lengths. But it is nonetheless a fact that WHITTAKER reached a wholly *false* historical estimate of the range of EINSTEIN'S originality. Nor can

(45) For a detailed statement of my criticisms of these earlier attempts, see A. GRÜNBAUM «Complementarity in Quantum Physics and its Philosophical Generalization», *The Journal of Philosophy* 54, 1957, pp. 713-727.

(46) *Ibid.*, p. 632.

WHITTAKER'S justification of his view to himself mitigate the philosophical incorrectness of his conception of EINSTEIN'S interpretation of the LORENTZ transformations. And while our awareness of the logical and perhaps psychological source of WHITTAKER'S historical error fournishes us with its *cause*, there can be no question of some kind of complementarist justification of it. Does HOLTON himself not appeal tacitly to an objectivist rather than a complementarist conception of the historical task in the following correct declaration of his, made by him à propos of WHITTAKER'S phrase « the relativity theory of POINCARÉ and LORENTZ », and saying (47): « in a crucial sense LORENTZ'S paper was of course not on the relativity theory as we understand the term since EINSTEIN » ?

(47) *Ibid.*, p. 636.