Part Two: Reassessing Lorentz's theory for the electrodynamics of moving bodies

Introduction: Manifesto on how to do history of special relativity

Chapter 21 of the revised 1973 edition of Grünbaum's *Philosophy of space and time*, a chapter entitled "The bearing of philosophy on the history of the special theory of relativity," starts with the following observation:

> *Philosophical Mastery of the Special Theory of Relativity is Required for Unraveling Its History* (Grünbaum 1973, p. 709; italics in the original).

The reader will immediately recognize this as a special case of the second part of Lakatos's famous variation on Kant: "history of science without philosophy of science is blind" (Lakatos 1970, p. 102). While recognizing that both Grünbaum and Lakatos use the word 'history' in the admittedly limited and controversial sense of 'rational reconstruction,' I agree with both slogans.[1] Yet, two questions remain. What kind of philosophy of science do we need for a rational reconstruction of the history of special relativity? And how much of it? Since Grünbaum makes his statement in a book that has become a classic in the philosophy of space and time, one might expect that the answer to the first question is just that: philosophy of space and time. And this actually is the answer I want to give to the first question. However, it is not, I am afraid, the answer one would come away with after reading Grünbaum's essay. Before I elaborate on this, I want to give a quotation from the most authoritative source I can think of to support my diagnosis of what type of philosophy is needed to unravel the history of special relativity. But first of all, I need to answer the second question. Fortunately, that is very simple and straightforward: as little as humanly possible.

After years of preparation and painstaking work by some of the best Einstein scholars in the field, the crucial volume two of *The Collected Papers of Albert Einstein* finally saw the light of day in 1989. This volume covers the period from 1900 to 1909, and thus includes the papers of

[1] This does not necessarily mean that I agree with Grünbaum and Lakatos on the relation between history of science and philosophy of science. My sense is that both philosophers think of the two disciplines as natural allies. I think of the two disciplines as natural enemies. My endorsement of the slogans of Grünbaum and Lakatos should therefore be read as the reluctant concession of a historian of science that philosophy of science is a necessary evil if, as a historian, one wants to do more than answer questions of the 'who did what when?'-variety.

Einstein's *annus mirabilis* 1905. The editorial apparatus of this volume has universally been praised for its thoroughness and insightfulness. One of the best pieces in the volume is undoubtedly the editorial note "Einstein on the theory of relativity," which precedes Doc. 23, the famous 1905 paper "On the electrodynamics of moving bodies." Senior editor John Stachel and his associates must have thought long and hard about the first sentence of this crucially important headnote, written for the ages. This is what they settled on:

> Einstein was the first physicist to formulate clearly the new kinematical foundation for all of physics inherent in Lorentz's electron theory. (Stachel et al. 1989, p. 253)

If I were allowed one single sentence to describe the breakthrough Einstein achieved with special relativity, this would be the one. No doubt about it. This one sentence, I claim, sets the agenda for the rational reconstruction of the history of special relativity. My dissertation can be seen as an attempt to make good on that claim. I hope my contribution will fit in with the bigger picture John Stachel is currently working out in a planned multi-volume comprehensive history of relativity theory. My more modest project has two specific objectives.

My first objective is to articulate Einstein's radically new interpretation of the relevant aspects of Lorentz's theory. I already presented my formulation in the overall introduction to this dissertation: Einstein recognized that Lorentz invariance is the symmetry of a new relativistic space-time that would come to be known as Minkowski space-time, whereas Lorentz thought of Lorentz invariance as a property accidentally shared by all dynamical laws governing systems in a classical Newtonian space-time.[2]

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[2] This dissertation deals with Lorentz rather than Einstein, but I do want to point out that my focus on the interpretation of Lorentz invariance fits nicely with the most promising route for reconstructing Einstein's path to special relativity. For lack of contemporary documentary evidence, we may never know for sure, but it seems very plausible that what John Norton likes to call 'the electrodynamically driven account,' outlined in the notorious *Dreimännerarbeit* of Earman, Glymour, and Rynasiewicz (1982), captures the essence of the genesis of special relativity. This account has Einstein discovering the Lorentz invariance of Maxwell's equations pretty much the way Lorentz did. When, six weeks prior to the submission of the 1905 paper, he hit upon the crucial idea of the relativity of simultaneity, the story continues, Einstein realized that this invariance was directly related to the structure of space and time.

The Earman-Glymour-Rynasiewicz proposal has not gotten the attention it deserves, and is mostly remembered for the suggestion that Einstein actually incorporated an earlier piece dealing only with the electrodynamics of moving bodies in his 1905 paper (hence its title!). There is not a shred of evidence for the existence of this earlier document. Earman, Glymour, and Rynasiewicz's case rests entirely on internal evidence from the 1905 paper, in which they discern two different strata than can be distinguished by paying attention to vagaries in style and presentation. The quickest way to dispose of this feeble argument is to recall what distinguished Kant scholar Lewis White Beck is reported to have said about Hans Vaihinger's claim that the transcendental deduction of the categories in the first edition of Kant's *Critique of pure reason* is a "patchwork" (the term comes from Paton 1929) of fragments written over the years, which are, in fact, inconsistent with one another. Kant scholar Paul Guyer relates: "As Lewis White Beck once argued (in an oral presentation), any man capable of putting together inconsistent notes, is also capable of writing an inconsistent text in a single go" (Guyer 1987, p. 432, note 1). Lewis White Beck's truism obviously applies to logical as well as stylistic inconsistencies. Robert Rynasiewicz told me, even before I mentioned this example, that he no longer believes in the existence of a proto-manuscript of Einstein's 1905 paper. Let me emphasize once more that, without this

I then want to raise the question why Lorentz held on to his own interpretation of Lorentz invariance. In this context, I will show two things. First, I will show that Lorentz's most important argument for preferring his own theory over Einstein's is much better than he is usually given credit for. The argument is simply that there has to be a substratum for the field. This argument survives in modern philosophy of space and time in the guise of the basic argument for manifold substantivalism in the context of a field theory. Second, and nonetheless, I will present what I think is a knock-out argument against Lorentz's theory, even though I fully accept Lorentz's claim that his theory is empirically equivalent to special relativity. Taking my inspiration from Wesley Salmon (1984, 1989), I want to argue that Lorentz's interpretation of Lorentz invariance leads to a violation of something akin to the common cause principle, one of the strongest principles of inductive inference, which tells us to infer to a common cause whenever one can be found. It is against this philosophical backdrop, I think (see the quotation from Grünbaum with which I started this introduction), that the historical question arises why Lorentz failed to appreciate the crucial advance that Einstein had made.

It will be clear from this brief statement of my goals that the philosophy I will invoke in unraveling the history of special relativity are elements from modern philosophy of space and time. To tell the good from the bad in Lorentz's arguments for sticking to the ether one needs a basic grasp of how one can look upon space-time as a differentiable manifold dressed up with a metric field and maybe other geometric object fields encoding its metric and affine properties. And the 'common cause'-type argument I alluded to turns on carefully distinguishing kinematical from dynamical effects.

I now return to Grünbaum's essay from 1973, which is actually a revised version of a talk he gave in 1963. One should keep in mind that it was not until 1969 that Holton published his now classic paper "Einstein, Michelson, and the "crucial" experiment," which finally succeeded (though not instantaneously) in dispelling "the myth of the Michelson-Morley experiment." By now, it has been so thoroughly eradicated that it may not be inappropriate to remind the reader what the myth is. It is that Einstein's postulates and his abolition of the ether were direct inductive generalizations of the negative result of the Michelson-Morley experiment, which classical ether theories could only account for at the cost of introducing *ad hoc* the Lorentz-FitzGerald contraction hypothesis.

Grünbaum played an important role in the demise of this folklore story. In 1959, he published a convincing rebuttal of an old claim by Popper, recently reiterated in the first English edition of the latter's 1934 classic *Logik der Forschung*, viz. that the Lorentz-FitzGerald

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"reification," the 'electrodynamically driven account' of Earman, Glymour, and Rynasiewicz is the most promising account available of Einstein's route to special relativity.

For further elaboration of the *Dreimännerarbeit*, see Rynasiewicz 1990.

contraction hypothesis is "an example of an unsatisfactory auxiliary hypothesis" which "had no falsifiable consequences but merely served to restore agreement between theory and experiment—mainly the findings of Michelson and Morley" (Popper 1959, p. 83; quoted in Grünbaum 1959, p. 48). Grünbaum showed that the Kennedy-Thorndike experiment of 1932 could be seen as a test of the hypothesis (see sections 3.2 and 3.3). Not surprisingly, a large portion of Grünbaum's 1963/1973 essay is devoted to the alleged *ad-hoc*-ness of Lorentz's theory.[3]

What I want to show is how further discussion of this particular issue, in combination with the general impact of Kuhn and Lakatos on the philosophy of science, would create the impression in the late sixties and early seventies that the comparison of the theories of Lorentz and Einstein is a topic squarely within the purview of the then extremely fashionable philosophy of theory change[4] and not a topic that simply calls for a little philosophy of space and time, as I urged above. The basic *Ansatz* of the seventies approach, the infamous case study in the philosophy of theory change, is that Lorentz and Einstein are the proponents of competing research programmes, paradigms, or what have you. This basic assumption has clouded rather than enlightened much of the history of special relativity. The basic *Ansatz* of the philosophy of space and time approach I propose is much more specific to the case at hand: Lorentz and Einstein had different interpretations of the Lorentz invariance of one and the same formalism. As I hope to show in chapters three and four, this actually provides a very helpful focal point in reconstructing the history of special relativity. The skeptic may object at this point that my diagnosis that the rational reconstruction of special relativity calls for manifolds instead of methodologies may reflect no more than the unprecedented popularity philosophy of space and time has come to enjoy in the nineties, thanks to the work of Sklar, Friedman, Earman, Norton, Torretti, Malament, and others. My reply to the skeptic: time will tell.

Let me return to the *ad-hoc*-ness issue. In a footnote added to the later editions of *The logic of scientific discovery*, Popper granted Grünbaum's point, but added: "Yet, as this hypothesis is less testable than special relativity, it may illustrate *degrees of adhocness*" (Popper 1959, p. 83, note; emphasis in the original).

The notion of *ad-hoc*-ness plays an important role in Lakatos's methodology of scientific research programmes. In the section on the Michelson-Morley experiment in the well-known exposition of this methodology for a symposium on Kuhn's work (Lakatos and Musgrave

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[3] As an aside, I want to mention that it is in this essay that Grünbaum seems to have coined the phrase "doubly amended ether theory" (Grünbaum 1973, p. 723).

[4] I am grateful to Andrea Woody for inviting me to give a presentation in a mini-conference she organized on the relation between history and philosophy of science, in the context of which I developed the idea for the discussion that follows. I also want to thank Jonathan Simon and Greg Morgan for helpful discussion of Lakatos's methodology of scientific research programmes.

1970), we find the following interesting footnote in which Lakatos, among other things, comments on Popper's reply to Grünbaum:

> FitzGerald at the same time, independently of Lorentz produced a testable version of this 'creative shift' [i.e., the contraction hypothesis] which was quickly refuted by Trouton's[5] Rayleigh's and Brace's experiment: it was theoretically but not empirically progressive.[6]
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> There is a widespread view that FitzGerald's theory was *ad hoc*. What contemporary physicists meant was that the theory was *ad hoc*$_2$ [...]: that there was '*no independent* [*positive*][7] *evidence*' for it.[8] Later, under Popper's influence the term '*ad hoc*' was primarily used in the sense of *ad hoc*$_1$, that there was *no independent test* possible for it. But, as the refuting experiments show, it is a mistake to claim, as Popper does, that FitzGerald's theory was *ad hoc*$_1$ [...] This shows again how important it is to separate *ad hoc*$_1$ and *ad hoc*$_2$.
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> When Grünbaum [...] pointed out Popper's mistake, Popper admitted it but replied that FitzGerald's theory was certainly *more ad hoc* than Einstein's and that this provides yet another 'excellent example of [...] one of the main theses of [his][9] book that *degrees* of *ad-hocness* are related (inversely) to degrees of testability and significance'. But the difference is not simply a matter of degrees of a unique *ad-hocness* which can be measured by testability. (Lakatos 1970, p. 75, note 5)

One of the main points of Lakatos's discussion of the Michelson-Morley experiment is the inadequacy of a naive falsificationist account, in which "the Michelson-Morley experiment single-handedly defeated the ether theory, the tenacity of which was only due to obscurantist conservatism" (ibid., p. 77). To illustrate this inadequacy, Lakatos points out, quoting some remarks by Ehrenfest and Dorling's discussion of them (see section 3.5), that "Lorentz's theory can easily be strengthened in such a way that it becomes, in an interesting sense, equivalent with Einstein's no-ether theory" (ibid.). In the footnote appended to this passage, Lakatos made a point which would have serious consequences for the historiography of special relativity:

> *two specific theories, while being mathematically (and observationally) equivalent, may still be embedded into different rival research programmes, and the power of the positive heuristic of these programmes may well be different*. (Lakatos 1970, p. 77, note 2; italics in original).

Lakatos's remarks about the importance of distinguishing different shades of *ad-hoc*-ness and of paying attention to differences in "the power of the positive heuristic" of different research programmes read like a blueprint for the controversial Lakatosian reconstruction of the history

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[5] I assume Lakatos means Trouton's original experiment (Trouton 1902). As is clear from the note Larmor attached to the reprint of Trouton 1902 in FitzGerald's collected papers, the negative result of the Trouton-Noble experiment was seen as a *confirmation* of the contraction hypothesis (see section 1.1 and 1.3).

[6] Lakatos refers to two books by Whittaker for this information, the second one being Whittaker 1953, II, pp. 28–30.

[7] Square brackets in original.

[8] Lakatos cites Larmor 1904, p. 624, as an example. What contemporary physicists meant when they derogated the contraction hypothesis as *ad hoc* is not nearly as obvious as Lakatos suggests (see Holton 1969, p. 327). However, Lakatos is right to emphasize that they did not mean the same thing as Popper.

[9] i.e., Popper's; Lakatos's square brackets.

of special relativity by Elie Zahar, originally in a paper published in 1973 and amplified in his 1989 book *Einstein's revolution*.

Zahar takes it upon himself to exonerate Lorentz's research programme from all charges of *ad-hoc*-ness. He is not satisfied with showing that in the succession of theories comprising Lorentz's research programme there are no theories that are *ad hoc*$_1$ or *ad hoc*$_2$, he also wants to show that non of these theories were *ad hoc*$_3$. The notion of *ad hoc*$_3$ is much more elusive, though historically perhaps more interesting, than the notions of *ad hoc*$_1$ and *ad hoc*$_2$: "[A] theory is said to be *ad hoc*$_3$ if it is obtained from its predecessor through a modification of the auxiliary hypotheses which does not accord with the spirit of the heuristic of the programme" (Zahar 1973, p. 217). What gives Einstein's theory the edge over Lorentz's, Zahar argues, is its superior positive heuristic that would eventually pay off empirically with the celebrated explanation of the anomalous advance of the perihelion motion of Mercury on the basis of the general theory of relativity (Zahar 1973, p. 262).

In 1974, the year after it published Zahar's paper, the *British Journal for the Philosophy of Science* devoted more than 50 pages of one of its issues to discussions of Zahar's work by Paul Feyerabend, Arthur Miller, and Ken Schaffner. These discussions amount to a devastating critique of Zahar's piece. Feyerabend seems to have some sympathy for Zahar's general project, Schaffner has very little, if any, and Miller has absolutely none. With admirable patience, Miller (1974) and Schaffner (1974) straightened out many historical mistakes in Zahar 1973 (see chapter three).[10] Zahar did take notice of these responses to his work (see Zahar 1977, 1978), but apparently chose not to take advantage of the useful service rendered to him by Miller and Schaffner. As a consequence, he simply repeated many of the mistakes he made in his 1973 paper when he wrote his 1989 book. By and large, I agree with Miller's and Schaffner's criticism of Zahar's history, although it has to be said that in their own work in this area they have made blunders of the same order of magnitude as the ones they accuse Zahar of, as I will amply illustrate with quotations from their work in chapter three.

Here I want to look at more general criticisms of Zahar's project. To understand why, as a historian of relativity, I find Zahar's paper a deeply troubling piece, it suffices to look at its title: "Why did Einstein's research programme supersede Lorentz's?"[11] Let me explain what worries me.

First of all, as Feyerabend complained, "Zahar never spells out the hard core of Einstein's research programme. One may guess, however, that it consists of some principles which are

[10] Personally, I am indebted to A. J. Kox for pointing out many of Zahar's errors to me.

[11] Nancy Nersessian managed to come up with a title far more outrageous than this one: "Why wasn't Lorentz Einstein?" (Nersessian 1986). On Nersessian's behalf, it has to be said that she duly apologizes for this title in the introduction of her paper, explaining that she just wanted to share with her readers a question she got asked frequently by her undergraduate students.

shared by the special and the general theories of relativity" (Feyerabend 1974, p. 25). I agree with Feyerabend that this is Zahar's intention and also, as Feyerabend goes on to argue, that it would seem to make more sense to give a broader definition of Einstein's research programme that would encompass his work in quantum and statistical physics as well. For a proper understanding of the genesis of special relativity, for instance, it is important to appreciate that Einstein's search for what he would later call a theory of principle (Einstein 1919; 1949, p. 53) was closely related to his doubts about the exact validity of Maxwell's equations in the light of quantum phenomena (Rynasiewicz 1990, pp. ???–???). Of more pressing concern to Zahar is the problem that, with his identification of Einstein's research programme, it is simply false to say that it superseded Lorentz's. Lorentz's interpretation of Lorentz invariance was superseded by Einstein's; Lorentz's research programme was superseded by quantum mechanics.

In response to this criticism by Feyerabend, Zahar writes: "It is my thesis that the switch from the Ether programme to Relativity is mainly a switch of heuristics" (Zahar 1978, p. 50). I always thought that a change of research programme necessarily involved a change of hard core, but far be it from me to contest Zahar's understanding of the advanced rules of his own game. What I do want to argue is that even this move will not do. The basic heuristic of Einstein's programme is, I take it, the way it exploits Lorentz invariance. Zahar's favorite example is Planck's 1906 modification of Newton's second law (Zahar 1973, p. 259; 1989, pp. 227–237). As I will show in detail in section 3.3, Lorentz had already given a mathematically equivalent derivation of that generalization in 1899. To mention one other example that I will work out in detail in section 3.2: in 1895, Lorentz already gave a concise derivation of the Fresnel dragging coefficient with the help of the embryonic Lorentz transformation, which is mathematically equivalent to Laue's celebrated 1907 derivation of this coefficient from the relativistic addition theorem of velocities. In chapter three, we will see that, before 1905, Lorentz invariance was *nothing but* a heuristic for Lorentz, but for now these examples must suffice to show that Lorentz knew perfectly well how to take advantage of the heuristic potential of Lorentz invariance within the confines of his own research programme. Hence, the superiority of Einstein's interpretation of Lorentz invariance over Lorentz's interpretation can not be cashed out in terms of heuristic power. Instead, it is a matter of space-time ontology.[12] Unfortunately, 'ontology' does not quite have the lure the magic word 'heuristic' has in the philosophy of science, in the seventies even more so than today.

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[12] I do not want to deny that, for instance, Planck and Laue realized the heuristic potential of Lorentz invariance because of Einstein's work rather than Lorentz's. The point is that the heuristic potential of Lorentz invariance does not make it irrational to stick to its Lorentzian interpretation, whereas the violation of the common cause principle inherent in Lorentz's space-time ontology does make this irrational.

Another problem for Zahar's approach, a problem Zahar explicitly acknowledges (Zahar 1973, p. 262), is that he has to assume sufficient continuity between special and general relativity to make these two theories part of the same research programme. In fairness to Zahar, it has to be said, that in 1973 this assumption probably did not look all that problematic. Two seminal papers by Norton (1984) and Stachel (1989) on the genesis of general relativity have meanwhile changed this situation completely. It is hard to imagine how all the twists and turns of Einstein's path from special to general relativity could be reconstructed as a series of theories with a stable enough hard core and stable enough heuristics to meaningfully view them as constituting a Lakatosian research programme.[13]

The development of Lorentz's theory is very different in this respect. Despite all the confusion surrounding the various versions of the theory in the literature, its development from, say, 1892 to 1916 is essentially straightforward, and could, in my opinion (not that I have any intention of doing so), be fitted quite naturally into the Lakatosian mold, in such a way that the sequence of theories would come out as a series of progressive rather than degenerating problem shifts. In other words, I think that Miller's and Schaffner's criticisms of Zahar's reconstruction of the development of Lorentz's theory are not fatal to his project. The more serious problem, it seems to me, is how to fit Einstein's research programme into the mold, and how to do so in a way that it makes sense to say that it superseded Lorentz's research programme.

Suppose, for the sake of argument, that this can be done. What do we end up with? Given that in the end only empirical criteria can decide between research programmes (a point rightfully emphasized in Schaffner 1974, p. 73) and given that Lorentz's theory is, by Zahar's own admission, empirically equivalent to special relativity, Zahar's conclusion will not change much. It became irrational for Lorentz to stick to his own research programme when Einstein calculated the perihelion advance of Mercury on the basis of the latest incarnation of his still developing general theory of relativity in November 1915, and out popped the 43 seconds of arc that had worried many physicists and astronomers since Le Verrier and Newcomb.[14]

Is this a happy outcome for a Lakatosian? No, it is a total disaster! The point of the type of exercise Zahar goes through is to show that Lakatos's model of scientific rationality captures

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[13] Let me give just one admittedly arbitrary example. Stachel has convincingly shown that Einstein had to unlearn the operational definition of the space-time coordinates that had played such an important role in his work on special relativity in order to overcome the so-called hole argument against generally covariant gravitational field equations, one of the obstacles on his way to general relativity (Stachel 1989, p. 64). In a Lakatosian reconstruction, this would translate into a sharp discontinuity in the heuristics of the programme. There will be many more of such discontinuities.

[14] Rather than accusing Lorentz of being irrational for continuing to believe in absolute simultaneity and a classical ether for the rest of his life (something for which we have unequivocal textual evidence), Zahar has Lorentz convert to relativity around 1914 (Zahar 1978, p. 50).

the rationality of the scientists in the episode under consideration. Let me quote another passage from Lakatos in this context:

> *In writing a historical case study, one should, I think, adopt the following procedure: (1) one gives a rational reconstruction; (2) one tries to compare this rational reconstruction with actual history and to criticize both one's rational reconstruction for lack of historicity and the actual history for lack of rationality*. (Lakatos 1970, p. 53; italics in the original)

Recall that I am assuming that Zahar has successfully completed step (1) of his case study. Let us now compare the result with the historical record. Did Lorentz give up his opposition to special relativity when word reached him of Einstein's successful prediction of the perihelion advance of Mercury? He did not. Does this reflect a lack of rationality of the actual history or a lack of historicity of the rational reconstruction? It will be clear how I want to answer this question. The rationality of preferring Einstein's interpretation of Lorentz invariance over Lorentz's, I think, has as little to do with the perihelion motion of Mercury as white shoes with black ravens.

So, even if we grant Zahar that all the truely formidable problems of his account could somehow be solved, it still looks as if the result would not be a rational reconstruction of the history of relativity, but a counter-example to his own methodology. As I mentioned at the outset, the problem seems to be that the episode does not fit the basic picture of competing research programmes.

I want to take a step back at this point. I have shown how, with Kuhn and Lakatos carrying the day in philosophy of science, a few footnotes by Popper and Lakatos (triggered by Grünbaum's observation that the Lorentz-FitzGerald contraction hypothesis is not *ad hoc*, at least not in the sense Popper claimed it was) eventually led to a substantial debate in the literature over the issue of Lorentz versus Einstein construed as a case study in the philosophy of theory change. As far as I have been able to tell, the basic premise that the issue should indeed be approached in this way was never called into question in this debate.

This is nicely illustrated by three papers by Schaffner (1970, 1974, 1976), in which the author proposed his own model for theory change and argued that the Lorentz versus Einstein case study fits his mold. These studies have not been as influential as Zahar's, and I will not discuss them in any detail. However, I do want to explain why I think Zahar's study is far more important than Schaffner's. This is related to my understanding of the difference between Lakatos and Kuhn.

Lakatos and Kuhn motivate their models for theory change in very different ways. Lakatos arrives at his model via an impressive philosophical argument in which he examines the pros and cons of the various traditional attempts to give a philosophical account of scientific rationality. From this point of view, Zahar's study is very important. It shows that the

Lakatosian methodology, despite a considerable measure of philosophical plausibility, fails to account for one of the most important episodes in the history of science. This need not be fatal to the methodology. One could simply accept that the methodology of scientific research programmes does not apply to the dispute between Einstein and Lorentz over the interpretation of the Lorentz invariance of their shared formalism.

Kuhn's approach is different. His model of theory change is based on some very strong historical intuitions. It does not start from a philosophical analysis and it turns out to be very hard to provide a philosophical underpinning for the model (see, e.g., Masterman 1970, Hoyningen-Huene 1993). *The structure of scientific revolutions* has nevertheless helped shape the intuitions of several generations of historians by now. Personally, I consider Kuhn to be one of the most perceptive historians of modern physics. However, I do not think there are important philosophical morals to be drawn from a match or a mis-match between a historical episode and Kuhn's model. Since the model does not seem to have a terribly convincing philosophical basis, a match simply means one more illustration of the intuitions, while a mis-match simply means that the intuitions did not pan out in this particular case. In this context, it is very telling, I think, that Kuhn, quite unlike Lakatosians such as Zahar, does not even talk about the categories of his general model when he is writing history.

It is fair to say, I think, that Schaffner's "logic of comparative theory evaluation," despite its Lakatosian name, falls into the same category as Kuhn's model, although it obviously has not had the tremendous impact of Kuhn's work. In the papers I mentioned, one does not find anything approaching the philosophical rigor of Lakatos and Zahar. Schaffner basically restricts himself to offering some plausibility arguments for his model. The proper attitude to take toward Schaffner's model, it seems to me, is simply to take advantage of the intuitions encoded in it. For instance, even though I strongly disagree with Schaffner's understanding of Lorentz's work, I share his intuition that the case of Lorentz versus Einstein turns on, as he calls them, "trans-empirical considerations" (Schaffner 1974, p. 73).

The tendency to think of the dispute between Lorentz and Einstein in terms of competing research programmes etc. can be traced back, I think, to the myth of the Michelson-Morley experiment, which glorifies Einstein to the exclusion of everybody else. It is against this background, that Whittaker's often quoted put-down of Einstein's 1905 paper, as a "paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention" (Whittaker 1953, II, p. 40) must be seen. However, if Whittaker indeed tried to restore some balance in this way, he achieved just the opposite of what he intended. For years, historians writing on Lorentz and Poincaré understandaby felt the need to distance themselves from Whittaker's preposterous remarks, often inadvertently giving Lorentz and Poincaré less than their fair share of the credit in the process. It is my impression that this situation is finally changing. Compare the papers of Scribner (1964) and Goldberg (1967) on Poincaré to the forthcoming paper by Darrigol (1994b) that I quoted at several points in part one. Scribner's and Goldberg's main concern seems to have been to convince their readers that Poincaré's accomplishments, as impressive as they were, fell far short of Einstein's. Darrigol is interested only in presenting Poincaré's contributions and does not feel the need to reassure the reader at every turn that the real intellectual giant was Einstein. This trend is related, I think, to the merciful decline in popularity of the proverbial case study that defined the discipline of History and Philosophy of Science in the seventies, so that the historian of relativity in the nineties is no longer conditioned to think in terms of competing research programmes etc. Instead, historians of relativity seem to have adopted the model of a reception study of Einstein's 1905 paper in which one examines how Einstein's work made previous results of Lorentz, Poincaré, and Abraham appear in a new light and how scientists such as Planck, Minkowski, Laue, Sommerfeld, Ehrenfest, Bucherer, and, of course, Einstein himself, approaching the problems from various different angles, worked out the ramifications of Einstein's new insights and thus contributed to the process that would ultimately give us special relativity in its canonical modern form.[15] I welcome this trend, and I will adopt its basic premise. For one thing, this means that I can do what, with the ghost of Whittaker still hovering over the field, would have been considered a mortal sin, viz. to grant Lorentz a few points against Einstein.

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[15] Personally, I owe this insight to John Stachel, but it can already be discerned, I think, in the books Miller (1981) and Goldberg (1984) wrote on the history of special relativity in the eighties.