# Chapter Three: Setting the record straight on the theorem of corresponding states

## 3.0 Common misconceptions about the theorem of corresponding states and the contraction hypothesis

In his well-known book of 1895, Lorentz proved that, to first order in $v/c$, the source free Maxwell equations are invariant under a transformation we now recognize as the Lorentz transformation for the space-time coordinates and the electromagnetic field, to first order in $v/c$ (Lorentz 1895, p. 84). This result is known as *the theorem of corresponding states*. The phrase "corresponding states" refers to pairs of solutions of the source free Maxwell equations, one describing a situation in a frame at rest in the ether, the other describing a situation in a frame in uniform motion through the ether. The solutions in such pairs are related to each other via the embryonic Lorentz transformation. With the help of the theorem of corresponding states, Lorentz could offer a very general account of the negative results of first order ether drift experiments in optics.

In an important paper published in 1899, both in a Dutch and a slightly different English version (Lorentz 1899a, 1899b), Lorentz introduced a new transformation under which the source free Maxwell equations are exactly invariant. Up to an undetermined constant, which Lorentz would eventually set equal to unity (Lorentz 1904b), this new transformation is what we now recognize as the exact Lorentz transformation. Lorentz made the exact version of the theorem of corresponding states the basis for a theory that predicts negative results for almost any ether drift experiment in optics, both first and second order experiments.

There is an important difference between the theory based on the first order version of the theorem of corresponding states and the theory based on the exact version. To understand where this difference is coming from, a few words need to be said about the ontology of Lorentz's theory.[1] Lorentz held a dualistic ontology of ether and matter. He subscribed to a stationary ether and an atomistic view of matter. The only interaction between ether and matter in his theory is via charged particles—originally called 'ions,' later called 'electrons'—generating disturbances in the ether corresponding to electric and magnetic fields and experiencing forces (the so-called Lorentz force) from these fields. So, two corresponding

[1] The ontology of Lorentz's theory remained pretty much the same throughout the development of the theory, as can be seen by comparing Lorentz 1895 (introduction, pp. 1–8) to Lorentz 1916 (section 5, pp. 8–10).

states, i.e., a pair of electromagnetic field configurations, are always associated with two corresponding configurations of material particles, interacting via unknown intermolecular forces and carrying the charge distributions responsible for the electromagnetic field configurations. Lorentz used the term 'corresponding states' strictly for electromagnetic field configurations. It will be convenient to use the terms in a somewhat wider sense, in which it includes both the electromagnetic field configurations and the configurations of particles carrying the charge distributions generating these fields. I trust no confusion will arise because of this somewhat wider usage of the term.

To derive negative results for ether drift experiments on the basis of the theorem of corresponding states, Lorentz has to assume that, when given a certain velocity with respect to the ether, a material system initially at rest in the ether and generating a particular electromagnetic field configuration will change into the material system generating the corresponding state of that electromagnetic field configuration in the co-moving frame. It is at this juncture that Lorentz's first order theory, i.e., the theory based on the first order theorem of corresponding states, is very different from Lorentz's exact theory, i.e. the theory based on the exact theorem of corresponding states.

In the first order theory, we can simply assume that the configuration of the material system will not change at all when it is set in motion (except obviously for the addition of an overall velocity). Lorentz does not even mention this assumption explicitly. The assumption is just part of the tacit assumption of pre-relativistic physical theory that as a rule physical phenomena satisfy a Galilean principle of relativity, electromagnetic phenomena being the exception to that rule. As a consequence, Lorentz's first order theory predicts negative results of ether drift experiments without adding any new physical hypotheses to the purely mathematical theorem of corresponding states. In the exact theory, however, the configuration of a material system at rest in the ether will have to change upon setting the system in motion if it is to generate the electromagnetic field configuration in the moving frame that is the corresponding state of the electromagnetic field configuration generated by the system at rest in the ether.

Lorentz largely focused on optical experiments that eventually boil down to the observation of patterns of light and darkness. From the first order theorem of corresponding states, it immediately follows that patterns of light and darkness in two corresponding states are the same. From a modern point of view, one would *prima facie* expect complications, even to first order, having to do with the relativity of simultaneity, but patterns of light and darkness are by their very nature stationary situations, so the relativity of simultaneity does not come into play. Lorentz's explanation of first order ether drift experiments in optics is then simply that a certain configuration of optical components producing a certain pattern of light and darkness while at

rest in the ether will produce that same pattern of light and darkness when it is in uniform motion through the ether.

From the exact theorem of corresponding states, it follows that patterns of light and darkness in corresponding states, though similar, are no longer the same. The pattern in the moving frame will be shortened in the direction of motion (as well as rescaled by the undetermined factor mentioned above) compared to the pattern in the corresponding state in the frame at rest in the ether. In order to predict negative result in all experiments probing such patterns of light and darkness in a moving frame, we have to make the assumption that the material part of the system under consideration, the configuration of optical components, will be shortened (and rescaled) in the same way as the patterns of light and darkness themselves. This assumption, of course, is strongly reminiscent of the contraction hypothesis Lorentz and FitzGerald had (independently) introduced earlier to account for the negative result of the Michelson-Morley experiment.

I will call the assumption that needs to be added to the exact theorem of corresponding states in order to predict negative results for all optical experiments that eventually boil down to the observation of some pattern of light and darkness the *generalized contraction hypothesis*. It generalizes the original Lorentz-FitzGerald contraction hypothesis in two ways. First, the hypothesis accounts not only for the Michelson-Morley experiment but for *all* experiments that eventually boil down to the observation of patterns of light and darkness (such as, say, the Kennedy-Thorndike experiment, that I will discuss in sections 3.2 and 3.3). Second, the generalized contraction hypothesis involves much more than a change in the dimensions of a material system when set in motion through the ether, the only effect for which the original contraction hypothesis gave a definite prediction.

Given my usage of the term 'corresponding states,' the generalized contraction hypothesis can be formulated as the hypothesis that *corresponding states physically transform into one another*. If one uses the term 'corresponding states' strictly for the electromagnetic field configurations, as Lorentz did, we have to be more careful. The generalized contraction hypothesis says that a material system (i.e., a particular configuration of particles) generating a particular electromagnetic field configuration in a frame at rest in the ether turns into the material system generating the corresponding state of that electromagnetic field configuration in a frame in motion through the ether when it is given the velocity of that frame. The reader, I trust, will already see the advantage of using the term 'corresponding states' in the somewhat broader sense in which I use it.

Comparing two corresponding states of a system that are assumed to physically transform into one another upon the appropriate change in the system's velocity we see that the generalized contraction hypothesis entails far more than the Lorentz-FitzGerald contraction

effect. In the course of this chapter, we will see that it entails (not surprisingly, I may add, since the generalized contraction hypothesis turns Lorentz's theory into a Lorentz invariant theory) the relativistic velocity dependence of the characteristic frequency of a light source, the relativistic expressions for aberration and Doppler effect, and (assuming that Newton's second law holds in the limit of low velocities) the relativistic transformation equations for force and mass. The relations between frequencies, forces, and masses are explicitly mentioned in the dazzling final section of Lorentz's 1899 paper that I will analyze in great detail in section 3.3. To my knowledge, Lorentz never mentioned the results for aberration and Doppler effect, even though the relevant calculations are a completely straightforward generalization of proofs in his 1895 book that the classical formulae for aberration and Doppler effect drop out of his first order theorem of corresponding states (as I will show in sections 3.1 and 3.3).

As we will see, Lorentz did not simply add the generalized contraction hypothesis to his exact theorem of corresponding states. His strategy was to introduce and to argue for more specific assumptions from which the generalized contraction hypothesis could then be derived. The two assumptions Lorentz actually made are the relations for forces and masses that I listed above among the consequences of the generalized contraction hypothesis. In the context of observations of patterns of light and darkness, these two relations are not just necessary but sufficient conditions for the generalized contraction hypothesis.

In sections 3.1, 3.3, and 3.5, I will work out the details of the picture just sketched. In doing so, I will pay special attention to some common misconceptions about Lorentz's theory in the extensive secondary literature on the subject.[2] Let me mention what I consider to be the two most serious points of confusion. The confusion on the first point has been recognized before, notably by Rynasiewicz (1988) and Darrigol (1994b). For the second point, however, I have not found a source which unambiguously gets this right.

(1) Many authors (e.g., Goldberg, Miller, Schaffner) have claimed that Lorentz looked upon the Lorentz transformed quantities playing a role in the theorem of corresponding states as the quantities *measured* by the moving observer. It is true that, under the influence of Einstein's work, Lorentz would eventually come to adopt that interpretation, but it is completely foreign to his thinking prior to 1905. Before 1905, the Lorentz transformed quantities were no more than mathematical auxiliaries for Lorentz. I want to suggest that Poincaré is the main culprit for this widespread misunderstanding of Lorentz's work. Around 1900, Poincaré already adopted the physical interpretation of the quantities in the theorem of corresponding states that Lorentz himself would only embrace after 1905 (see Darrigol 1994b). However, Poincaré writes about

[2] I am grateful to Ofer Gal for suggesting to importing some material in overly lengthy footnotes discussing such misconceptions into the body of the text. This greatly improved the overall structure of this chapter.

this interpretation as if it were Lorentz's, and Lorentz, it seems, never took exception to Poincaré's misrepresentation of his views.

(2) Many authors (e.g., McCormmach, Miller, Pais, Schaffner, Scribner) have suggested that with the exact theorem of corresponding states, the Lorentz-FitzGerald contraction somehow got absorbed into the Lorentz transformation equations. As I explained above, this is not the case. The successors to the original contraction hypothesis (either the generalized contraction hypothesis or the more specific assumptions from which it can be derived) still need to be added as physical assumptions to the purely mathematical theorem of corresponding states.

This last remark naturally leads me to an issue I address in section 3.2, the section dealing with the original contraction hypothesis. The issue is the alleged *ad-hoc*-ness of Lorentz's theory that I already discussed in the introduction to part two. Amplifying a well-known argument by Grünbaum, I will argue that neither the original contraction hypothesis nor its successors added to the exact theorem of corresponding states are *ad hoc* in the usual falsificationist sense of that term. However, a clear distinction needs to be made between these two cases. The testability of the original contraction hypothesis is severely limited by the fact that the hypothesis does not make any definite predictions about the effect the changes in the dimensions of moving systems have on other properties of those systems. In the case of the generalized contraction hypothesis (or of the hypotheses from which it is derived) we have no such limitations. The statement that corresponding states physically transform into one another amounts to very definite predictions of the effect of the contraction on other phenomena, thus opening up the possibility of many different tests of the hypothesis (or conjunction of hypotheses).

It should be noted that these definite predictions are always of the same kind: the effects are such that ether drift can not be detected. I do not want to deny that this is a very unsatisfactory situation. My point is that what is unsatisfactory about it is *not* that it compromises the theory's testability. The task at hand, as I see it, is to articulate what is. A promising way to find out, it seems to me, is to carefully re-analyze the charges of *ad-hoc*-ness leveled at Lorentz's theories by Einstein, Poincaré, and others, keeping in mind that it is extremely unlikely that there is a close connection between those charges and Popper's later charge that the contraction hypothesis is not falsifiable. This is a project beyond the scope of this dissertation, but I will make some remarks (in chapter four) about the direction in which I would want to look.

Let me say a few words about section 3.4 and about some subsections of section 3.5 that I have not touched upon so far. In section 3.4, I consider the electron model that Lorentz proposed in 1904. I want to emphasize that Lorentz's main motivation in developing this model was to provide an underpinning for the velocity dependent concept of mass he needed in the

context of what I called the generalized contraction hypothesis. As we already saw in chapter two, Lorentz's original purely electromagnetic model was inconsistent. As I will show in section 3.4, this inconsistency presented Lorentz and his contemporaries with a dilemma: either give up the electromagnetic view of nature that held out the promise of eventually being able to derive the generalized contraction hypothesis from electrodynamics or accept that some ether drift experiments ought to give positive results, which, in modern terms, of course would be tantamount to giving up the relativity principle. In the secondary literature, there is no clear statement of this basic dilemma, mainly because of the lack of understanding I mentioned of the role the generalized contraction hypothesis plays in Lorentz's mature theory. In the face of the dilemma, Lorentz unambiguously chose to keep what we now recognize as the principle of relativity and to give up the electromagnetic view of nature. So, it came to pass that in the early years of special relativity, in the context of the important debate over Kaufmann's experiments on the velocity dependence of high-speed electrons in β-radiation, the important dichotomy was not Lorentz versus Einstein, but Lorentz-Einstein versus Abraham, the champion of the electromagnetic view of nature.[3]

In section 3.5, I turn to Lorentz's reaction to Einstein's 1905 paper, as documented in his lectures at Columbia University in New York in 1906, published as *The theory of electrons*, first in 1909 and again in 1916 with numerous new and interesting footnotes (Lorentz 1916). In this context, I will also examine Lorentz's lectures on relativity in Leiden from the period 1910–1912 (Lorentz 1922). I will show how Lorentz took from Einstein the important insight that the Lorentz transformed quantities of his theorem of corresponding states are the quantities measured by the moving observer. At first, Lorentz did not take over Einstein's expressions for the transformation of charge and current density, (correctly) objecting to Einstein's derivation of these expressions. In one of the footnotes in the second edition of *The theory of electrons*, Lorentz provided a more satisfactory derivation of these relations, thus finally achieving full invariance of Maxwell's equations, including the source terms, in the context of his own theory. I will also briefly examine the two arguments, one good, one bad, that Lorentz routinely offered for sticking to his own theory, despite his admiration for Einstein's work.

I will also look at a curious twist in the history of the issue concerning the transformation of charge and current density, a twist that illustrates a general point that I made earlier. In 1906, Poincaré had already given a derivation of the transformation of charge and current densities which is mathematically equivalent to Lorentz's derivation of 1915 (Poincaré 1906; Lorentz

[3] As can be inferred from a remark by Sommerfeld in the discussion following an important lecture by Planck in 1906, many physicists felt that the Einstein-Lorentz approach, favored by Planck, was conservative, even old-fashioned, whereas the Abraham approach, favored by Sommerfeld and by Minkowski for instance (Galison 1979), was still considered to be on the cutting edge at that time (see Planck 1906 and section 3.4).

1916). In accordance with his understanding of the auxiliary quantities in Lorentz's theorem of corresponding states as the quantities measured by a moving observer, Poincaré had claimed that the auxiliary charge and current densities that Lorentz had used in 1904 were wrong and that the quantities he derived were the correct ones. Since Lorentz looked upon the quantities he introduced as mathematical auxiliaries, they obviously can not be wrong. As Rynasiewicz (1988, p. 73) has put it: "One is free to stipulate definitions as one pleases." Lorentz's definitions were just clumsy, as he himself later admitted (see Holton 1969, p. 321). However, he did not straighten out Poincaré in 1906. Neither did he cite Poincaré's derivation when he published his own mathematically equivalent derivation in 1915. This is quite uncharacteristic for Lorentz. What I want to emphasize is that the fact that Lorentz never challenged Poincaré's creative misreading of his work has probably contributed strongly to the subsequent misinterpretation of his work by historians of science.

I want to make one last general remark before I get down to business. As will be clear from the title of chapter three and from what has been said both in this introduction and in the introduction to part two, I am engaged in what Ted McGuire would call "revisionist history."

To use one of McGuire's favorite metaphors, one does not find historical facts 'as pebbles by the seashore.' McGuire's point is sometimes understood as throwing into doubt whether historical events really took place at all. This is to trivialize the point. Of course, historical events did happen.[4] So, the surviving texts and artifacts, the tangible evidence that certain events actually did happen, are, in a sense, no different from pebbles by the seashore and other debri left on the beach by the changing tides. McGuire's point is that these pebbles do not constitute the historical facts the historian works with. To pursue the analogy, the historian has to bear in mind that he or she is not the first to go for a stroll on the beach to collect some pebbles. Generations of historians have done so before and it is obviously an illusion to think that such beachcombers have left the pebbles in their pristine state. In at least one way, this is a good thing. If it were not for the beachcombers, the elements would long since have reclaimed the pebbles altogether. But the interference can also do considerable harm. The historian will only preserve the pebbles that seem particularly interesting, while others are allowed to simply disappear underneath the sand. Moreover, the pebbles deemed worthy of preservation will be rearranged in ways pleasing to the eye of the preserver. These two effects clearly distort our view of the beach as it was before the beachcombers arrived upon the scene.

A pessimist may say that the distortions will be so drastic that we can never hope to reconstruct what the beach looked like before it became a beach resort. I am an optimist. I think

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[4] As Keith Parsons likes to say, borrowing a phrase from Bertrand Russell, this is so obvious that it takes an extremely intelligent person to deny it.

there are cases—and I think that the case of the development of Lorentz's theory is one of them—in which we can correct for these distortions. In other words, I believe that there are cases where we have enough clues to reconstruct the original configuration of the pebbles that have been preserved. The reconstruction may then give us some clues where to look for additional pebbles. If we actually find such pebbles in the places where we expect to find them on the basis of our reconstruction, we can take this as a sign that we are on the right track.

In retelling the story of the development of Lorentz's theorem of corresponding states, I will focus almost exclusively on the first step in the process I just described, i.e., on undoing some of the misinterpretations of the texts that have traditionally been deemed the milestones in this development: a few sections of the so-called *Versuch* of 1895 (Lorentz 1895), the final section of the 'Simplified theory ...' of 1899 (Lorentz 1899a, 1899b, 1902), the famous 1904 paper (Lorentz 1904b), reflecting the state of the theory just before the advent of special relativity and widely read because of its inclusion in the Teubner anthology (Lorentz et al. 1952), and some sections of *The theory of electrons*, the book based on Lorentz's lectures in New York in 1906 (Lorentz 1916).[5] I do not want to deny that these documents are crucial for understanding the development of the theory. But they are not the only ones. Especially if we lift the arbitrary restriction to optics, as I have urged we should, other documents need to be examined as well. In the context of the Trouton-Noble experiment and the Kaufmann experiments, for instance, one should look at Lorentz's writings on the energy-momentum tensor and on the mass-energy equivalence. The most serious limitation of the present study is that I have not systematically gone through these additional documents. I hope to rectify this at some point, preferably in collaboration with Lorentz expert A. J. Kox.

Even in its present form, my analysis illustrates the relevance of three documents that, I think, have not received sufficient attention so far. The first is the contribution of Lorentz to the *Encyklopädie der Mathematischen Wissenschaften* (Lorentz 1904a) that I cited in section 1.4. The other two documents are the published versions of two lecture series, the first given in Leiden in 1910–1912 (Lorentz 1922), the second given at Caltech in 1922 (Lorentz 1927). Reading these lectures in conjunction with the famous 1906 lectures of *The theory of electrons*, one gets a clear sense of, on the one hand, Lorentz's deepening appreciation of the theory of relativity—both the special and the general theory (see Kox 1988, Janssen 1992)—and his tenacity in clinging to absolute simultaneity and a classical ether on the other. If these lectures are cited at all, it is to illustrate this tenacity (see, e.g., Nersessian 1984, p. 117; 1986, p. 232). To see the other side—Lorentz's superb mastery of the formalism of the theory—we have to

[5] In addition to these documents, one typically looks at Lorentz 1886, 1892a, 1892b as setting the stage for the development of Lorentz's theory for the electrodynamics of moving bodies based on the theorem of corresponding states.

start looking beyond optics, and consider, for instance, Lorentz's lucid treatment of issues concerning energy-momentum and $E = mc^2$.

## 3.1 The first order theorem of corresponding states (1895)

**3.1.1 What is the theorem of corresponding states?** The theorem of corresponding states is a mathematical tool for solving problems in electrodynamics in a Galilean frame of reference moving through the ether. Since Lorentz's theory is based on the notion of a stationary ether,[6] a terrestrial observer will always use such a frame to describe the experiments in optics and electrodynamics he or she performs in the laboratory.

The first version of Lorentz's theorem of corresponding states, which can be found in his book *Attempt at a theory for electrical and optical phenomena in moving bodies* (Lorentz 1895,[7] sections 56–58, pp. 81–85), is restricted in two ways. First, it works only for those problems in electrodynamics that can be solved with the source free Maxwell equations. Second, terms of order $v^2/c^2$ and smaller are systematically neglected. These restrictions are directly related to the application Lorentz had in mind when he derived the theorem, which was to account in a general way for the negative results of first order ether drift experiments in optics.[8]

In 1899 and 1904, Lorentz generalized the theorem of corresponding states so as to remove the restrictions mentioned above, a task he would complete only in 1915, with the second edition of his book *The theory of electrons* (Lorentz 1916). For a proper understanding of the conceptual development of Lorentz's theory, it is of paramount importance to carefully trace the

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[6] See Hirosige 1966, 1969 and Darrigol 1994a for a discussion of the ontology of Lorentz's theory and its development. See McGuire 1974 for a discussion of the tradition of ether theories in the context of which this ontology should be evaluated.

I want to quote from Einstein's assessment of the importance of Lorentz's work in this area in an essay he wrote at the request of the director of what would become the Museum Boerhave in Leiden on the occasion of an exhibition commemorating the Dutch Nobel laureates Lorentz and Kamerlingh Onnes in 1953. Einstein wrote: "Most of the younger generation are no longer fully aware of the decisive role played by H. A. Lorentz in shaping the fundamental concepts of theoretical physics. The reason for this curious fact is that they have absorbed Lorentz's basic ideas so completely that they are virtually incapable of comprehending the boldness of these ideas and the simplification they brought about in the foundations of physics" (Einstein 1953, p. 22). Einstein goes on to sketch the complexity of electromagnetic theory at the start of Lorentz's career, with four fields instead of two, carried both by the ether and by ponderable matter. Einstein then writes: "It was at this point that H. A. Lorentz performed his act of intellectual liberation [*erlösende That*]. With great logical consistency, he based his research on the following hypotheses: The seat of the electromagnetic field is empty space [...] This field is produced by atomistic electric charges, [on which the field in turn exerts forces]. The only link between the electromotoric field and ponderable matter is the fact that elementary electric charges are intimately connected with the atomistic constituents of matter" (ibid., pp. 22–23; the part I paraphrased is inaccurately translated). Einstein emphasizes the crucial importance of these steps for the development of special relativity: "Indeed, the essential step forward was precisely the reduction to Maxwell's equations [in] empty space, or—as it was then called—the ether" (ibid.).

[7] This book is commonly referred to as the *Versuch*. It is the most important work of Lorentz that Einstein is known to have read before 1905 (see Holton 1969, p. 318; Stachel et al. 1989, pp. 259–260).

[8] Many of these experiments are discussed in Lorentz 1886. This article also discusses Michelson's second order experiment of 1881. Lorentz's discussion partly inspired the Michelson-Morley experiment of 1887, a repetition of Michelson's 1881 experiment with improved accuracy (see, e.g., Swenson 1972, p. 88).

transition from the first order version to the exact version of the theorem. For my purposes, the transformation of the theorem from a theorem in optics in 1895 to a theorem in electrodynamics in general in 1899 and 1904 is less important. For a proper understanding of the perfection of the theorem after 1905, it will be convenient to start dealing with the full Maxwell equations, including the source terms, right away, which is what I will do.

Consider the diagram in Fig. 3.1. We have two (Galilean) inertial frames of reference, $S_0$ and $S$, the former at rest in the ether, the latter in uniform motion through the ether at a velocity $\mathbf{v}$ (ignore the third frame $S'$ for the time being). The $x$-axes of $S_0$ and $S$ are chosen in the direction of $\mathbf{v}$, so the components of $\mathbf{v}$ are $(v, 0, 0)$. The coordinates of $S_0$ are written as $\mathbf{x}_0$, the coordinates of $S$ are written as $\mathbf{x}$. Likewise, I write $t_0$ for the time in $S_0$ and $t$ for the time in $S$, even though $t_0$ and $t$ refer to the same absolute Newtonian time. $(\mathbf{x}_0, t_0)$ and $(\mathbf{x}, t)$ are related to each other via the Galilean transformation

$$\mathbf{x} = \mathbf{x}_0 - \mathbf{v}\, t_0, \qquad t = t_0. \tag{3.1}$$

In $S_0$, the electric field $\mathbf{E}$ and the magnetic field $\mathbf{B}$ satisfy Maxwell's equations:

$$\begin{aligned} &\mathrm{div}_0\, \mathbf{E} = \rho/\varepsilon_0, \qquad \mathrm{curl}_0\, \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t_0}, \\ &\mathrm{div}_0\, \mathbf{B} = 0, \qquad \mathrm{curl}_0\, \mathbf{B} = \mu_0 \rho\, \mathbf{u}_0 + \frac{1}{c^2}\frac{\partial \mathbf{E}}{\partial t_0}, \end{aligned} \tag{3.2}$$

where $\mathbf{E}$, $\mathbf{B}$, the charge density $\rho$, and the current density $\rho\mathbf{u}_0$ (the subscript '0' to indicate that $\mathbf{u}_0$ is a velocity with respect to $S_0$) are all functions of $(\mathbf{x}_0, t_0)$. So, the derivatives in Eq. 3.2 are with respect to $t_0$ and $\mathbf{x}_0$, as is indicated by the subscript '0' on the differential operators $\partial/\partial t_0$, $\mathrm{div}_0$ and $\mathrm{curl}_0$. In 1895, Lorentz, as I already mentioned, actually considered the simpler case where $\rho = \mathbf{u}_0 = 0$.

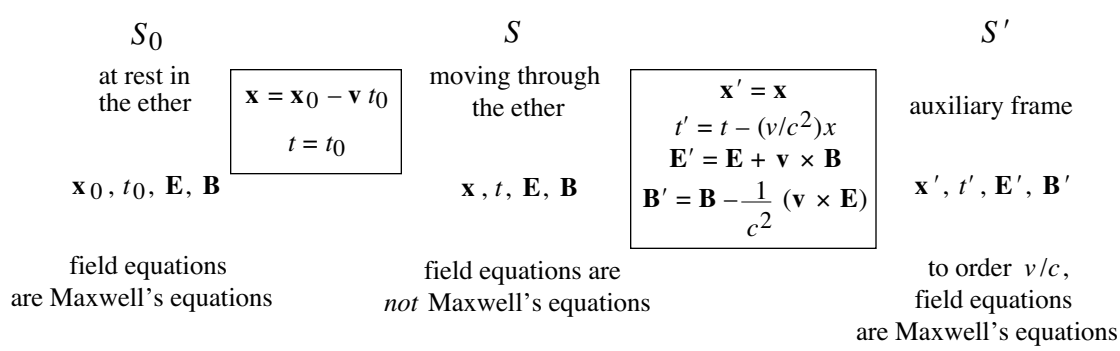


Figure 3.1: Diagram to illustrate Lorentz's first order theorem of corresponding states.

Understood as functions of ($\mathbf{x}$, $t$), the fields $\mathbf{E}$ and $\mathbf{B}$ satisfy a different set of field equations which are obtained from Maxwell's equations by replacing the time derivative $\partial/\partial t_0$ with the differential operator $\partial/\partial t - v\ \partial/\partial x$ and by replacing $\mathbf{u}_0$ with $\mathbf{u} + \mathbf{v}$, where $\mathbf{u}$ is a velocity with respect to $S$. So, in $S$ we have the equations

$$\begin{aligned} &\operatorname{div} \mathbf{E} = \rho/\varepsilon_0, \qquad \operatorname{curl} \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} + v\,\frac{\partial \mathbf{B}}{\partial x}, \\ &\operatorname{div} \mathbf{B} = 0, \qquad \operatorname{curl} \mathbf{B} = \mu_0 \rho(\mathbf{u} + \mathbf{v}) + \frac{1}{c^2}\left(\frac{\partial \mathbf{E}}{\partial t} - v\,\frac{\partial \mathbf{E}}{\partial x}\right). \end{aligned} \tag{3.3}$$

At this point, Lorentz introduced a set of auxiliary quantities with the help of which Eq. 3.3 can, if terms of order $v^2/c^2$ are neglected, be written in the form of Maxwell's equations in the source free case, and in a form very close to Maxwell's equation if the source terms are non-vanishing. Lorentz replaced the real time $t$ or $t_0$ with a parameter $t'$ which he called the "local time" (*Ortszeit*, Lorentz 1895, p. 81). Moreover, instead of the real fields $\mathbf{E}$ and $\mathbf{B}$, he introduced the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$. The spatial coordinates stay the same. The primed quantities are defined in terms of the real unprimed quantities belonging to the moving frame $S$ through

$$\mathbf{x}' \equiv \mathbf{x}, \qquad t' \equiv t - (v/c^2)\,x, \tag{3.4}$$

$$\mathbf{E}' \equiv \mathbf{E} + \mathbf{v} \times \mathbf{B}, \qquad \mathbf{B}' \equiv \mathbf{B} - \frac{1}{c^2}\,\mathbf{v} \times \mathbf{E}. \tag{3.5}$$

The primed quantities are thus stipulations and not assumptions about the time or the fields measured by a moving observer. For a concise and unusually clear statement of this basic yet widely misunderstood point, see Rynasiewicz 1988. Although around 1900 Poincaré already interpreted these primed quantities, in particular the local time, as the quantities measured by the moving observer (see, e.g., Darrigol 1994b, pp. 2–3, 49–50, 51, 58, 61), Lorentz would only start using this interpretation, under the influence of Einstein, after 1905 (see section 3.5).[9]

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[9] The following quotations from some of the older secondary literature will serve to illustrate the confusion on this point. In a discussion of the theorem of corresponding states in the *Versuch*, Goldberg writes: "The new transformation concerned the measurement of time in frames of reference moving with respect to the ether frame [...] Such a transformation equation no longer left the measurement of time an invariant. Though this was a very bold and radical step to take, Lorentz had surprisingly little to say about the sharp departure such a transformation equation represented [...] he made little comment on the meaning of the transformation save that he clearly intended it to be little more than an aid to calculation" (Goldberg 1969, p. 986). What I find most astonishing about this passage is the wild incongruence of this last remark with the preceding sentences. Goldberg is not the only one being confused. He actually cites a draft of Schaffner 1969 in a footnote appended to this passage. I will quote from two later papers by Schaffner. Schaffner is actually talking about the 1904 version of the theorem of corresponding states here, but the point is the same. "The $t'$ in equation (9) [see Eq.

I want to draw attention to one particular passage in Poincaré's writings that I suspect is responsible for a lot of the confusion surrounding the interpretation of Lorentz's work in the historical literature. The passage I have in mind occurs in the section "The principle of relativity" in Poincaré's famous 1904 lecture in St. Louis. Poincaré vividly describes the situation in ether theory around the turn of the century. While the dominant theories posit a stationary ether, the experiments aimed at detecting the earth's presumed motion through this medium consistently give negative results. The task of explaining these experimental findings theoretically, Poincaré writes "was not easy, and if Lorentz has got through it, it is only by accumulating hypotheses" (Poincaré 1904, p. 99). Starting a new paragraph, he continues: "The most ingenious idea was that of local time" (ibid.). Poincaré proceeds to explain that if an observer in uniform motion through the ether synchronizes his clocks using what a modern reader immediately recognizes as the light signaling method from Einstein 1905a, these clocks will not read the true Newtonian time, but Lorentz's local time. Poincaré does not indicate in any way that this interpretation is entirely his own and is not to be found in any of Lorentz's writings up to this point. For Poincaré, the notion of local time clearly involves a physical assumption. Poincaré assumes that the local time is, in effect, the time registered by moving observers, which helps to account for the fact that such observers do not detect ether drift. After making this point (ibid, pp. 99–100), Poincaré starts his next paragraph saying: "Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion undergo a uniform contraction in the sense of the motion" (ibid., p. 100). So, for Poincaré the notion of local time and the contraction hypothesis (to be discussed in detail in sections 3.2 and 3.3) have essentially the same status. They are both physical assumptions. This is a far cry from Lorentz's understanding of the situation. He obviously looked upon the contraction hypothesis as a physical assumption, but local time for him is no more than a convenient purely mathematical auxiliary quantity. It neither is nor involves any physical assumption. Perhaps the most clear-cut evidence for this interpretation is a well-known passage

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3.54 in section 3.3] represents the latest form of the "local time" which holds in *moving* electromagnetic systems. It is still to be distinguished from the "true" Newtonian time holding in bodies in *rest* in the ether. Transformations for **d** and **h** [read: **E** and **B**] were introduced by postulation, but they vary only slightly from what would have been expected from a standard referral of a system of charges to a moving coordinate system" (Schaffner 1970, pp. 335–336; italics in the original). Six years later, Schaffner has still not been able to pull himself out of this muddle: "It will be useful to analyze the concept of local time in some detail. Lorentz seems to treat it as a mathematical change of variable in its early forms. Even in the 1895 form (10) [i.e., Eq. 3.4], however, it entails that clocks at different points in the moving system will be out of synchronization by a factor $vx/c^2$" (Schaffner 1976, p. 472). Miller also gets this wrong: "The *hypothesis* of local time was the basis for the theorem of corresponding states that permitted systematic explanation of all optical phenomena to an accuracy of first order in $v/c$" (Miller 1981, pp. 39–40; my italics).

In the light of the discussion in the introduction to part two, it is somewhat ironic that Zahar has not fallen prey to the confusion of his critics Miller (1974) and Schaffner (1974) on this point. Zahar lucidly remarks: "Let us incidentally note the *purely mathematical origin* of this notion of local time, *which eventually led to* Poincaré's and Einstein's concepts of frame-dependent simultaneity" (Zahar 1989, p. 67; my emphasis).

in the second edition of *The theory of electrons*, where Lorentz writes that before 1905 he had been "clinging to the idea that the variable $t$ only can be considered as the true time and that my local time $t'$ must be regarded as no more than an auxiliary mathematical quantity" (Lorentz 1916, p. 321, note 72*). Reading Lorentz through Poincaré's eyes, many historians seem to have misunderstood this basic point, with disastrous consequences for their overall interpretation of Lorentz's use of corresponding states (see especially section 3.3).[10]

When terms of order $v^2/c^2$ and smaller are neglected, the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ understood as functions of $(\mathbf{x}', t')$ satisfy equations very similar to Maxwell's equations.[11]

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[10] Both Miller (1973) and Goldberg (1967) published on Poincaré before they published on Lorentz, so they may have been influenced by Poincaré's creative misreading of Lorentz's work.

[11] The easiest way to prove that these are indeed the equations for the auxiliary fields $\mathbf{E}'$ and $\mathbf{B}'$ as functions of $(\mathbf{x}', t')$ is as follows. Consider the equation for $\mathrm{div}'\mathbf{E}'$. Similar arguments can be given for the other three equations, starting with (the components of) $\mathrm{curl}'\mathbf{E}' - \partial\mathbf{B}'/\partial t'$, $\mathrm{div}'\mathbf{B}'$, and $\mathrm{curl}'\mathbf{B}' - (1/c^2)\,\partial\mathbf{E}'/\partial t'$, respectively. I will write $\mathrm{div}'\mathbf{E}'$ in terms of the unprimed quantities, and then use Eq. 3.3 to show that, when quantities of order $v^2/c^2$ are neglected, this expression is equal to $\rho/\varepsilon_0\,(1 - v\,u_x/c^2)$, as Eq. 3.6 says it is.

The inverse transformation of Eq. 3.4 is

$$\mathbf{x} = \mathbf{x}', \qquad t = t' + (v/c^2)\,x',$$

which means that the derivatives with respect to the primed variables are related to the derivatives with respect to the unprimed variables as

$$\left(\frac{\partial}{\partial t'}, \frac{\partial}{\partial x'}, \frac{\partial}{\partial y'}, \frac{\partial}{\partial z'}\right) = \left(\frac{\partial}{\partial t}, \frac{\partial}{\partial x} + (v/c^2)\frac{\partial}{\partial t}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right).$$

Inserting these relations along with Eq. 3.5 for $\mathbf{E}'$, we find

$$\mathrm{div}'\mathbf{E}' = \frac{\partial E_x}{\partial x} + (v/c^2)\frac{\partial E_x}{\partial t} + \frac{\partial E_y}{\partial y} - v\frac{\partial B_z}{\partial y} + \frac{\partial E_z}{\partial z} + v\frac{\partial B_y}{\partial z}.$$

Multiplying the first term on the right hand side by $1 - v^2/c^2$ (which we are free to do, since quantities of order $v^2/c^2$ are neglected in the end) and regrouping terms, we can write this equation as

$$\mathrm{div}'\mathbf{E}' = \left(\frac{\partial E_x}{\partial x} + \frac{\partial E_y}{\partial y} + \frac{\partial E_z}{\partial z}\right) - v\left(\frac{\partial B_z}{\partial y} - \frac{\partial B_y}{\partial z} - \frac{1}{c^2}\frac{\partial E_x}{\partial t} + \frac{v}{c^2}\frac{\partial E_x}{\partial x}\right).$$

The first three terms in parentheses are just div $\mathbf{E}$. The last four terms in parentheses are just the $x$-component of curl $\mathbf{B} - (1/c^2)\,(\partial\mathbf{E}/\partial t - v\,\partial\mathbf{E}/\partial x)$. Using Eq. 3.3 for these expressions and using the relation $\mu_0 = 1/(\varepsilon_0 c^2)$, we arrive at

$$\begin{aligned}\mathrm{div}'\mathbf{E}' &= \rho/\varepsilon_0 - \frac{v\rho}{\varepsilon_0 c^2}(u_x + v)\\ &= \rho/\varepsilon_0\left(1 - \frac{v\,u_x}{c^2} - \frac{v^2}{c^2}\right).\end{aligned}$$

The last term in parentheses can be neglected, so this is indeed the equation for $\mathrm{div}'\mathbf{E}'$ in Eq. 3.6. The other equations in Eq. 3.6 can be found in the same way.

$$\begin{aligned} &\mathrm{div}'\,\mathbf{E}' = \rho/\varepsilon_0\left(1-\frac{v\,u_x}{c^2}\right), && \mathrm{curl}'\,\mathbf{E}' = -\frac{\partial \mathbf{B}'}{\partial t'}, \\ &\mathrm{div}'\,\mathbf{B}' = 0, && \mathrm{curl}'\,\mathbf{B}' = \mu_0\rho\,\mathbf{u} + \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}, \end{aligned} \tag{3.6}$$

where $\rho$ and $\mathbf{u}$, like $\mathbf{E}'$ and $\mathbf{B}'$, are to be understood as functions of $(\mathbf{x}', t')$.

Combining Eq. 3.1 for the transformation $(\mathbf{x}_0, t_0) \rightarrow (\mathbf{x}, t)$ and Eq. 3.4 for the transformation $(\mathbf{x}, t) \rightarrow (\mathbf{x}', t')$, we find that

$$\mathbf{x}' = \mathbf{x} = \mathbf{x}_0 - \mathbf{v}\,t_0, \qquad t' = t - (v/c^2)x = t_0 - (v/c^2)x_0 + O(v^2/c^2). \tag{3.7}$$

A modern reader will immediately recognize this as the Lorentz transformation $(\mathbf{x}_0, t_0) \rightarrow (\mathbf{x}', t')$ to first order in $v/c$ (so that $\gamma = 1$). From a modern perspective, the derivation of Eq. 3.6 is simply (part of) a proof, to first order in $v/c$, that Maxwell's equations are invariant under Lorentz transformation. From such a modern point of view, the primed quantities $\mathbf{x}'$, $t'$, $\mathbf{E}'$, and $\mathbf{B}'$ all belong to the Lorentz frame $S'$ moving at a velocity $\mathbf{v}$ with respect to the frame $S_0$, just as the unprimed quantities belong to the Galilean frame $S$ moving at a velocity $\mathbf{v}$ with respect to the frame $S_0$. The four-vector $j'^{\mu} \equiv (\rho' c, \rho'\mathbf{u}')$, representing the charge and current density in $S'$, is related to the corresponding vector $j_0^{\mu}$ in $S_0$ via

$$\begin{aligned} \rho' c &\approx \rho c - \frac{v}{c}\rho u_{0x} &&\approx \rho c\left(1-\frac{v\,u_x}{c^2}\right), \\ \rho'\mathbf{u}' &\approx \rho\,\mathbf{u}_0 - \frac{\mathbf{v}}{c}\rho c &&= \rho\,\mathbf{u}, \end{aligned} \tag{3.8}$$

in accordance with Eq. 3.6.

If we are just interested in optics, we can set $\rho = \mathbf{u} = 0$ in Eq. 3.3 and Eq. 3.6. Eq. 3.6 then reduces to:

$$\begin{aligned} &\mathrm{div}'\,\mathbf{E}' = 0, && \mathrm{curl}'\,\mathbf{E}' = -\frac{\partial \mathbf{B}'}{\partial t'}, \\ &\mathrm{div}'\,\mathbf{B}' = 0, && \mathrm{curl}'\,\mathbf{B}' = \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}, \end{aligned} \tag{3.9}$$

Suppose we have a solution of the source free Maxwell equations describing some field configuration in the frame $S_0$ at rest in the ether. If we copy this solution in terms of the quantities of the auxiliary frame $S'$, we have a solution of the equations in Eq. 3.9. After all, these equations have the exact same form as the source free Maxwell equations. If we then

transform back to the quantities of $S$ and neglect terms of order $v^2/c^2$ and smaller, we obtain a new field configuration in the moving frame $S$ that is also a solution of the field equations, at least to first order in $v/c$. This field configuration in the moving frame and the field configuration in the frame at rest that we started from, are called "corresponding states" (*correspondirende Zustände*, Lorentz 1895, p. 85). The *theorem* of corresponding states is the crucial result that was invoked in this construction of a solution for a problem in $S$ from a solution in $S_0$. To paraphrase Lorentz's own statement of the theorem in the *Versuch* (Lorentz 1895, section 59,[12] p. 84):

> If there is a solution of the source free Maxwell equations in which the real fields **E** and **B** are certain functions of $\mathbf{x}_0$ and $t_0$, the coordinates of $S_0$ and the real Newtonian time, then, if we ignore terms of order $v^2/c^2$ and smaller, there is another solution of the source free Maxwell equations in which the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ are those same functions of $\mathbf{x}'$ and $t'$, the coordinates of $S$ and the local time in $S$.

**3.1.2 How the Lorentz invariance of the source free Maxwell equations to first order in *v/c* and the general nature of patterns of light and darkness account for the negative result of almost any conceivable first order ether drift experiment in optics.** How did Lorentz use this result to account for the negative result of first order ether drift experiments in optics? Before I give the correct answer to this question, let me go over an incorrect answer that trivializes the whole question. From a modern relativistic point of view, the moving observer would actually measure the Lorentz transformed quantities of the frame $S'$ rather than the Galilean transformed quantities of the frame $S$. Since we have just shown that, at least to order $v/c$ and in the free field case, these quantities satisfy Maxwell's equations, it is immediately clear that such an observer can never detect any ether drift by means of a first order ether drift experiment in optics. However, as I already emphasized, for Lorentz these primed quantities were no more than mathematical auxiliaries. Before 1905, Lorentz tacitly assumed that the moving observer would measure the Galilean transformed quantities of the frame $S$. Hence, Lorentz's explanation in 1895 of the negative results of first order optical ether drift experiments can not be this simple modern explanation.[13]

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[12] That the quantities $t'$, $\mathbf{x}'$, $\mathbf{E}'$, and $\mathbf{B}'$ were just mathematical auxiliaries for Lorentz with no physical meaning is illustrated by the last paragraph of this section, which, in my notation, can be paraphrased as: once $\mathbf{E}'$ and $\mathbf{B}'$ are known as functions of $\mathbf{x}'$ and $t'$, and thus also as functions of $\mathbf{x}$ and $t$, we can calculate **E** and **B** from Eq. 3.5.

[13] The simple explanation is suggested in the following passage. Immediately after presenting his statement of the theorem of corresponding state which is fully equivalent to my statement above, Miller writes : "The meaning of this statement, known as the theorem of corresponding states, is that the equations of electromagnetism are unchanged to order $v$ [read: $v/c$] when transformed to a system moving with uniform linear

The argument Lorentz actually did use runs as follows (Lorentz 1895, p. 86).[14] Many optical experiments—be it an experiment in geometrical optics or an experiment involving interference, diffraction, or polarization—eventually boil down to the observation of some pattern of light and darkness. Such patterns are easily described. In some regions the fields vanish, in other regions they do not (at least not on average). Consider some field configuration in $S_0$ and its corresponding state in $S$. According to the theorem of corresponding states, the same functions that give the real fields $\mathbf{E}$ and $\mathbf{B}$ as a function of the real coordinates $\mathbf{x}_0$ of $S_0$ and the real time $t_0$ for the configuration in $S_0$ will give the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ as a function of the coordinates $\mathbf{x} = \mathbf{x}'$ of $S$ and the local time $t'$ for the corresponding state of that configuration in $S$. Suppose the configuration in $S_0$ is such that at a point $P$ with coordinates $\mathbf{x}_0 = \mathbf{a}$ it is dark. That means that the fields $\mathbf{E}$ and $\mathbf{B}$ vanish at this point, not just at one instant, but over a stretch of time that is long compared to period of the light waves described by the fields $\mathbf{E}$ and $\mathbf{B}$. It follows that the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ will vanish at $\mathbf{x} = \mathbf{a}$ in the corresponding state in $S$. Since the relation between the real and the fictitious fields is linear, this means that the real fields $\mathbf{E}$ and $\mathbf{B}$ in the corresponding state will also vanish at $\mathbf{x} = \mathbf{a}$. It follows that the patterns of light and darkness in the moving frame and the patterns of light and darkness in the frame at rest are the same. If, in addition, one tacitly assumes, as Lorentz did, that the configuration of optical components producing the patterns of light and darkness does not change upon being set in motion with respect to the ether, this result amounts to a general explanation for the negative result of almost any conceivable first order ether drift experiment.

The stationary nature of patterns of light and darkness plays a crucial role in this argument. Without this property, the $x$-dependence of local time would lead to serious complications. Suppose that in two points $P_0$ and $Q_0$ of $S_0$, the fields vanish simultaneously with the respect to the real Newtonian time. In the corresponding points $P$ and $Q$ of the moving frame $S$, the fields will then vanish simultaneously with respect the local time. Since the local time depends on $x$, this means that they will *not* vanish simultaneously with respect to the real Newtonian time. This would invalidate Lorentz's conclusion with regard to patterns of light and darkness. Fortunately, patterns of light and darkness, by their very nature, are stationary situations. The concepts of light and darkness only have meaning on time scales that are large compared to the periods of the light waves used. So, when at $P$ and $Q$ it is dark at the same instant in local time, it will also be dark at both points at the same instant in real time.

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relative motion with respect to the ether. *Thus*, to order $v$, optical phenomena occur on the moving earth as if it were at rest" (Miller 1973, p. 221; my italics). As we already saw, Miller interprets the Lorentz transformed quantities as the measured quantities for the moving observer. In that case, the inference of Miller's "thus" would be completely straightforward. Under Lorentz's actual interpretation of these quantities, however, Miller's "thus" suppresses a subtle and non-trivial argument.

[14] The argument is discussed briefly in McCormmach 1970b, p. 471; and in Darrigol 1994a, p. 288. For a later statement by Lorentz himself of this argument, see Lorentz 1904a, pp. 265–268.

**3.1.3 How the Fresnel dragging coefficient and the classical formulae for aberration and Doppler effect drop out of Lorentz's first order theorem of corresponding states.** Lorentz presumably did not fully appreciate the complications arising from the $x$-dependence of local time. Apart from the fundamental result on patterns of light and darkness, however, the *Versuch* contains a few striking applications of the theorem of corresponding states in which the $x$-dependence of local time does play an essential role. It turns out that the theorem of corresponding states allows a very simple derivation of the Fresnel dragging coefficient[15] (Lorentz 1895, sections 68–69, pp. 95–97) and of the classical formulae for aberration and the Doppler effect (ibid., sections 60–61, pp. 87–89).[16]

Suppose a plane light wave of frequency $\nu$ is traveling in the positive $x$-direction of a frame $S_0$ at rest in the ether through some transparent object with a refractive index $n$ at rest with respect $S_0$. The fields **E** and **B** describing this wave will only depend on the coordinates $\mathbf{x}_0$ and the time $t_0$ of $S_0$ via

$$2\pi\nu\left(t_0 - \frac{x_0}{c/n}\right). \tag{3.10}$$

From the theorem of corresponding states, it follows that, to first order in $\beta$, the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ for a similar light wave traveling through the same object when it is moving through

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[15] The Fresnel dragging effect, introduced by Fresnel in 1818 in a famous letter to Arago (see, e.g., Whittaker 1953, Vol. 1, pp. 108–113), ensures that light, when it strikes the surface of a transparent object moving through a stationary ether, is refracted in accordance with Snell's law of refraction ($\sin i = n \sin r$, with $i$, $r$, and $n$ the angle of incidence, the angle of refraction, and the index of refraction, respectively) *applied in a frame of reference moving with the object*. Without the dragging effect, the law of refraction would hold in a frame at rest in the ether even for refraction of light striking moving transparent objects, and, as a consequence, terrestrial observers would be able to detect the earth's presumed motion through the ether in refraction experiments. The example usually given of a first order ether drift experiments on refraction whose negative result is explained by the Fresnel dragging effect is an experiment suggested by Boscovich in 1776 and carried out by Airy in 1871 (see, e.g., Lorentz 1895, p. 89), in which stellar aberration is measured with a telescope whose tube is filled with water. For an elementary discussion of how the Fresnel dragging effect explains that the water in the tube does not affect the observed aberration angle, see Goldberg 1984, pp. 443–448. Parenthetically, I may add that neither Boscovich, obviously, nor Airy, apparently, were motivated by the search for ether drift.

Goldberg also gives an elementary discussion of the famous Fizeau experiment of 1851, in which the Fresnel dragging effect in moving water was measured directly, an experiment repeated with greater accuracy in 1886 by Michelson and Morley and by Zeeman in 1914–1915 (see, e.g., Miller 1981, p. 281, note 3).

In 1892, Lorentz had already replaced Fresnel's model of the dragging effect, involving excess ether present in transparent material, by a more satisfactory model in terms of bound charged particles absorbing and re-emitting radiation. Fresnel's original model is easy to ridicule. The amount of excess ether depends on the index of refraction of the material. The index of refraction, in turn, depends on the frequency of the refracted light (dispersion). It follows that transparent matter must carry different amounts of excess ether for different wavelengths! Such criticism misses the point of Fresnel's achievement. In modern terms, Fresnel showed that the dragging effect is necessary if one wants to make the law of refraction, which is Lorentz invariant and not Galilean invariant in the context of a wave theory, compatible with a Galilean principle of relativity.

[16] These calculations are also discussed in Miller 1981, pp. 36–39.

the ether at a velocity $\mathbf{v} = (v, 0, 0)$ will only depend on the coordinates $\mathbf{x}' = \mathbf{x}$ and the local time $t'$ in the moving frame $S$ via

$$2\pi\nu\left(t' - \frac{x'}{c/n}\right). \tag{3.11}$$

Otherwise, a terrestrial observer, for instance, could use the light traveling through, say, a piece of glass on earth to create a pattern of light and darkness already differing in first order of $\beta$ from the one we would expect to find if the earth were at rest in the ether. In other words, a terrestrial observer would be able to detect the earth's presumed motion through the stationary ether in a first order ether drift experiment that eventually amounts to the observation of some pattern of lightness and darkness. As we saw above, it follows from Lorentz's theorem of corresponding states that such experiments must give a negative result.

Since the real fields $\mathbf{E}$ and $\mathbf{B}$ are linear combinations of the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$, it follows that the $(\mathbf{x}', t')$-dependence of the real fields is also via the expression in Eq. 3.11 only. Using the transformation from $(\mathbf{x}, t)$ to $(\mathbf{x}', t')$ in Eq. 3.4, we can find the $(\mathbf{x}, t)$-dependence of the fields describing the light wave in the moving object from their $(\mathbf{x}', t')$-dependence given in Eq. 3.11:

$$2\pi\nu\left(t' - \frac{x'}{c/n}\right) = 2\pi\nu\left(t - \frac{v}{c^2}x - \frac{n}{c}x\right). \tag{3.12}$$

From this equation, we read off that the light wave has velocity

$$\frac{1}{\frac{v}{c^2} + \frac{n}{c}} = \frac{c/n}{1 + \frac{v}{nc}} \approx c/n - v/n^2 \tag{3.13}$$

in the $x$-direction with respect the frame $S$. Since $S$ has velocity $v$ in the positive $x$-direction with respect to the frame $S_0$ at rest in the ether, it follows that the light wave in the moving object has a velocity

$$\frac{c}{n} + \left(1 - \frac{1}{n^2}\right)v \tag{3.14}$$

in the $x$-direction with respect to the ether. In other words, the light wave is dragged along by the moving transparent object at a fraction $1 - 1/n^2$ of that object's own velocity through the ether. The factor $1 - 1/n^2$ is just the famous Fresnel dragging coefficient.[17]

[17] Cf. Miller 1981, pp. 278–279. As Miller explains, Lorentz's 1895 derivation is mathematically equivalent to Laue's famous derivation in 1907 of the Fresnel dragging coefficient from Einstein's relativistic addition theorem for velocities. Conceptually, however, the two derivations are very different. The point of Laue's

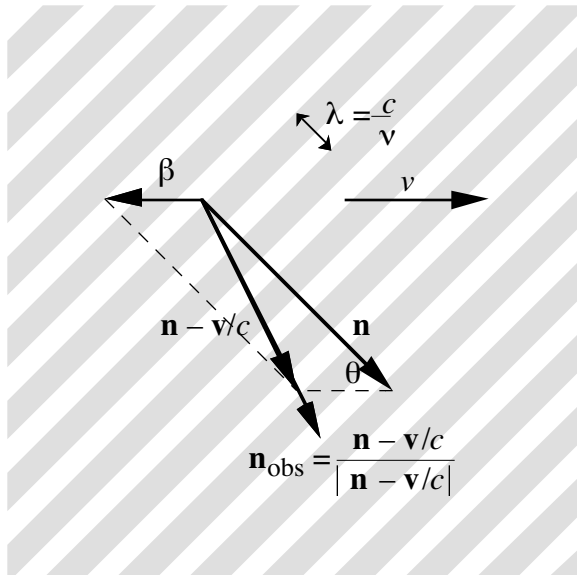


Figure 3.2: The classical Doppler and aberration effects.

The expressions for the classical Doppler and aberration effects drop out of Lorentz's theorem of corresponding states in pretty much the same way the Fresnel dragging coefficient dropped out. Consider a plane light wave with frequency $\nu$ traveling through the ether in the direction of the unit vector $\mathbf{n}$, the normal on its wave fronts (see Fig. 3.2).

According to the classical formulae for the Doppler and aberration effects this wave will have a frequency

$$\nu_{\text{obs}} = \frac{c - \mathbf{v} \cdot \mathbf{n}}{\lambda} = \left(1 - (\mathbf{v} \cdot \mathbf{n})/c\right) \nu \tag{3.15}$$

for an observer moving through the ether with a velocity $\mathbf{v}$, where I used that the wavelength $\lambda$ is equal to $c/\nu$, and it will be in the direction of the unit vector

$$\mathbf{n}_{\text{obs}} = \frac{\mathbf{n} - \mathbf{v}/c}{|\mathbf{n} - \mathbf{v}/c|} \approx \frac{\mathbf{n} - \mathbf{v}/c}{1 + (\mathbf{n} \cdot \mathbf{v})/c}, \tag{3.16}$$

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derivation is to show that the Fresnel dragging effect is essentially a kinematical effect in special relativity. Given Snell's law of refraction, it is a direct consequence of the relativity of simultaneity. This shows that *any* Lorentz invariant model of refraction giving Snell's law in the rest frame of the refracting material will automatically yield the Fresnel dragging coefficient. For Lorentz, his 1895 derivation was simply a shortcut for the derivation of the Fresnel dragging coefficient in the *specific* Lorentz invariant model of refraction he had introduced in 1892 and for which he had shown explicitly that it yields the Fresnel dragging coefficient.

I have ignored a slight complication in both Lorentz's and Laue's calculations. With a Lorentz invariant model for refraction in the rest frame of the refracting material that also takes into account dispersion, there will automatically be another term in the dragging coefficient (see, e.g., Miller 1981, p. 281, note 3). For an elegant and elementary discussion of this complication see Montanus 1992, section 2, pp. 402–404.

where I used the result of simple calculation for the norm of the vector $\mathbf{n} - \mathbf{v}/c$.[18]

Suppose the light wave is coming from a source which is moving along with the observer at a velocity $\mathbf{v}$ with respect to the ether. In that case, the Doppler and aberration effects in Eqs. 3.15–3.16, due to the motion of the observer, are canceled exactly by the Doppler and aberration effects due to the motion of the source.[19] So, $\nu_{\text{obs}}$ and $\mathbf{n}_{\text{obs}}$ in Eqs. 3.15–3.16 would just be the frequency and the direction the observer would measure if the experiment were repeated with both the source and the observer at rest in the ether.

This means that if the light wave shown in Fig. 3.2 comes from a source moving with the observer, the fields describing that light wave—both the fictitious field $\mathbf{E}'$ and $\mathbf{B}'$ and the real fields $\mathbf{E}$ and $\mathbf{B}$—can only depend on $(\mathbf{x}', t')$ via

$$\nu_{\text{obs}}\left(t' - \frac{\mathbf{n}_{\text{obs}} \cdot \mathbf{x}'}{c}\right). \tag{3.17}$$

Otherwise, the light waves could be used to the detect the earth's motion through the ether (see the argument I gave above in the context of the Fresnel dragging effect).

The $(\mathbf{x}', t')$-dependence of the fields describing a light wave is, of course, independent of the state of motion of the source. Eq. 3.17 therefore must be true of any light wave, not just of light waves coming from a co-moving source.

What needs to be shown at this point is that Eqs. 3.15–3.17 give an accurate description of a light wave of frequency $\nu$ in the direction $\mathbf{n}$. After all, nothing said so far guarantees that

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[18] With the help of the vector diagram in Fig. 3.2 we can write

$$|\,\mathbf{n} - \mathbf{v}/c\,|^2 = (1 - \beta\cos\theta)^2 + \beta^2\sin^2\theta = 1 - 2\,\beta\cos\theta + \beta^2.$$

Inserting that $-v\cos\theta = \mathbf{n}\cdot\mathbf{v}$ and neglecting terms of order $\beta^2$, we find

$$|\,\mathbf{n} - \mathbf{v}/c\,|^2 \approx\ 1 + (\mathbf{n}\cdot\mathbf{v})/c\ .$$

[19] Picture Wes Salmon enjoying the music of a small combo on an open deck of a cruise ship, and, say, a saxophone player in this combo. So, Wes and the saxophone player are both moving with respect to the air carrying the sound waves the saxophone player generates on his saxophone. Notice that the sound from the saxophone reaching Wes's ears does not come from the position of the saxophone player at that instant, but from the position of the saxophone player a split second earlier, the time difference being equal to the time it took the sound to cover the distance from the saxophone to Wes's ears. However, since Wes has the same velocity with respect to the medium carrying the sound waves as the saxophone player, he will have the impression that the sound does come from the position of the saxophone the moment he actually hears its sound. Unlike many other members in the audience, Wes is not puzzled by this at all, for he (and the physics students on semester at sea) understand perfectly well that if source and observer have the same velocity with respect to the medium, the effect due to the motion of the source is exactly compensated by the effect due to the motion of the observer. A similar and perhaps more familiar argument applies to the *frequency* of the saxophone sound. There will be a Doppler effect due to the motion of the source and a Doppler effect due to the motion of the observer, but since source and observer have the same velocity with respect to the medium in this case, the two effects cancel. In short, on a calm day, the combo on the open deck will sound exactly the way it sounds when it is playing indoors.

Lorentz's theory sanctions the expressions for $\nu_{\text{obs}}$ and $\mathbf{n}_{\text{obs}}$ in Eqs. 3.15–3.16 based on the classical formulae for aberration and Doppler effect. What we do know is that the fields describing a plane light wave of frequency $\nu$ in the direction $\mathbf{n}$ only depend on $(\mathbf{x}_0, t_0)$ via

$$\nu\left(t_0 - \frac{\mathbf{n}\cdot\mathbf{x}_0}{c}\right) \tag{3.18}$$

Inserting Eq. 3.7 for the transformation from $(\mathbf{x}_0, t_0)$ to $(\mathbf{x}', t')$ into Eq. 3.17, one does indeed obtain Eq. 3.18 as can easily be verified. Using Eq. 3.7, we can write:

$$\begin{aligned} t' - \frac{\mathbf{n}_{\text{obs}}\cdot\mathbf{x}'}{c} &= t_0 - \frac{\mathbf{v}\cdot\mathbf{x}_0}{c^2} - \frac{(\mathbf{n}-\mathbf{v}/c)\cdot(\mathbf{x}_0 - \mathbf{v}\,t_0)}{c\left(1+(\mathbf{n}\cdot\mathbf{v})/c\right)} + O(\beta^2) \\ &= t_0 - \frac{\mathbf{v}\cdot\mathbf{x}_0}{c^2} - \left(1-(\mathbf{n}\cdot\mathbf{v})/c\right)\left(\frac{\mathbf{n}\cdot\mathbf{x}_0}{c} - \frac{\mathbf{v}\cdot\mathbf{x}_0}{c^2} - \frac{\mathbf{n}\cdot\mathbf{v}}{c}t_0\right) + O(\beta^2) \\ &= \left(1-(\mathbf{n}\cdot\mathbf{v})/c\right)\left(t_0 - \frac{\mathbf{n}\cdot\mathbf{x}_0}{c}\right) + O(\beta^2) \end{aligned} \tag{3.19}$$

Inserting this expression into Eq. 3.17 and using Eq. 3.16 for $\nu_{\text{obs}}$, we arrive at Eq. 3.18:

$$\begin{aligned} \nu_{\text{obs}}\left(t' - \frac{\mathbf{n}_{\text{obs}}\cdot\mathbf{x}'}{c}\right) &= \left(1+(\mathbf{n}\cdot\mathbf{v})/c\right)\nu\left(1-(\mathbf{n}\cdot\mathbf{v})/c\right)\left(t_0 - \frac{\mathbf{n}\cdot\mathbf{x}_0}{c}\right) + O(\beta^2) \\ &= \nu\left(t_0 - \frac{\mathbf{n}\cdot\mathbf{x}_0}{c}\right) + O(\beta^2). \end{aligned} \tag{3.20}$$

This shows that Lorentz's first order theory of the *Versuch* does indeed give the classical formulae for Doppler effect and aberration. In sections 3.3 and 3.5, I will show that Lorentz's exact theory gives the relativistic expressions for these effects.

## 3.2 The original contraction hypothesis (1892/1895)

**3.2.1 The Michelson-Morley experiment, the Lorentz-FitzGerald contraction, and electrostatics in moving frames of reference.** In the last chapter of the *Versuch*, entitled "Experiments whose results do not allow explanation without further ado" (translation taken from Holton 1969, p. 319), Lorentz turns to second order ether drift experiments, i.e., experiments with an accuracy of $v^2/c^2$. The most important experiment in this category is, of course, the experiment of Michelson and Morley.[20] Lorentz (1895, sections 89–92, pp. 119–124[21]) essentially repeats what he had said in a short article on the Michelson-Morley experiment published three years earlier (Lorentz 1892b). His discussion of the experiment consists of two parts. First, he shows how a contraction of moving bodies can explain the negative outcome of the experiment. He then presents a plausibility argument for this contraction hypothesis. If, Lorentz argues, it is assumed that the molecular forces holding Michelson's interferometer together are affected by the earth's motion through the ether in the same way as Coulomb-forces are affected, the interferometer will experience a contraction of the kind needed to explain the negative result of the Michelson-Morley experiment. I will examine these two parts of Lorentz's account of the experiment in turn.

**3.2.2 How the Lorentz-FitzGerald contraction accounts for the negative result of the Michelson-Morley experiment.** I will look at the experimental set-up of the Michelson-Morley experiment in somewhat greater detail in section 3.3, but for my purposes in this section it suffices to consider the situation illustrated in Fig. 3.3.

Consider light traveling back and forth along an arm of length $L$ of a Michelson interferometer moving through the ether at a constant velocity $\mathbf{v}$ making an angle $\theta$ with that velocity. The velocity of the light with respect to the interferometer is the sum of the light's velocity with respect to the ether (the vectors of length $c$ in Fig. 3.3) and the ether's velocity $-\mathbf{v}$ with respect to the interferometer.

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[20] See Swenson 1972 for a detailed discussion of the history of experiments of Michelson, Morley, and Miller, including references to the extensive literature on this topic and facsimile reproductions of the classic papers of Michelson from 1881 (the original second order ether drift experiment in Potsdam) and of Michelson and Morley from 1886 (the repetition of the Fizeau experiment to measure the Fresnel dragging coefficient) and 1887 (the classic experiment in Cleveland). Additional material can be found in a special issue of *Physics Today* of May 1987 (Vol. 40, No. 5) on the occasion of the centennial of the Michelson-Morley experiment, and in Goldberg and Stuewer 1988, based in part on a symposium commemorating the centennial of the experiment. For those who read Dutch, see Janssen 1988 for a concise summary of the importance of the experiment in the context of 19th century ether theory.

[21] I will refer to the translation of these three sections in Lorentz et al. 1952, pp. 3–7. Holton has rightfully commented on the selection of these three sections (and nothing else) from the *Versuch* for the infamous Teubner collection (Blumenthal 1913): "Thus do scissor-wielding and extract-prone editors distort the appearance of history" (Holton 1969, p. 319).

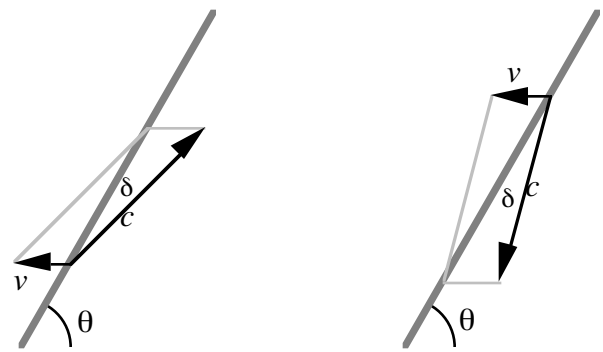


Figure 3.3: Light traveling back and forth in the arm of a moving interferometer.

For both situations shown in Fig. 3.3, we have

$$c^2\cos^2\delta + v^2\sin^2\theta = c^2, \tag{3.21}$$

which shows that the angle $\delta$ on the left is equal to the angle $\delta$ on the right. Eq. 3.21 gives the following expression for the cosine of this angle:

$$\cos\delta = \sqrt{1 - \beta^2\sin^2\theta}\,. \tag{3.22}$$

With the help of Fig. 3.3, we can compute the time $\tau$ it takes light to travel back and forth for this particular orientation of the interferometer arm

$$\tau = \frac{L}{c\cos\delta - v\cos\theta} + \frac{L}{c\cos\delta + v\cos\theta} = \frac{2cL\cos\delta}{c^2\cos^2\delta - v^2\cos^2\theta}. \tag{3.23}$$

Inserting Eq. 3.22 into Eq. 3.23, we can eliminate $\cos\delta$ from the expression for $\tau$:

$$\tau = \frac{2L}{c}\frac{\sqrt{1 - \beta^2\sin^2\theta}}{1 - \beta^2}. \tag{3.24}$$

This equation shows that $\tau$ depends on $\theta$, unless $L$ also depends on $\theta$ in just the right way. Basically, the rotating interferometer in the Michelson-Morley experiment was designed to detect this $\theta$-dependence of $\tau$. After our detailed discussion of the geometry of the condenser in the Trouton-Noble experiment in section 1.2, it is easily seen that the Lorentz-FitzGerald contraction gives $L$ just the right $\theta$-dependence to make $\tau$ $\theta$-independent.

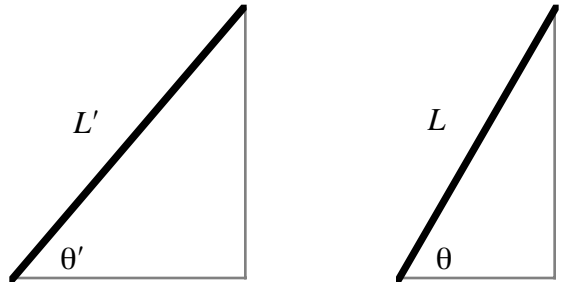


Figure 3.4: interferometer arm at rest in the ether (left);
and contracted arm moving through the ether (right).

On the right in Fig. 3.4 , we have an interferometer arm of length $L$ moving through the ether at an angle $\theta$ with its velocity **v**; on the left, we have an arm of length $L'$ at rest in the ether that would turn into the arm on the right if, upon giving it the velocity **v**, it were to contract in the direction of motion by a factor $\gamma$. The relation between the $\theta'$-independent length $L'$ of the arm at rest in the ether and the $\theta$-dependent length $L$ of the arm in motion through the ether is given by (cf. the expression for $a/a'$ in Eq. 1.11 in section 1.2):

$$L = L' \sqrt{\frac{1-\beta^2}{1-\beta^2 \sin^2\theta}} \,. \tag{3.25}$$

Inserting Eq. 3.25 into Eq. 3.24, we see that this does indeed make $\tau$ $\theta$-independent, thus accounting for the negative result of the Michelson-Morley experiment.

We can, of course, multiply the right hand side of Eq. 3.25 with an arbitrary function $f(\beta)$ without changing the prediction of a negative result in the Michelson-Morley experiment. This amounts to assuming that objects moving through the ether at a velocity **v** not only contract by a factor $\gamma$ in the direction of motion, but are also stretched out by a factor $f(\beta)$ in all three directions. Lorentz was well aware of this possibility.[22] If we assume that objects do indeed

[22] In 1892, Lorentz wrote: "... the question would still remain whether the earth's motion shortens the dimensions in one direction, as assumed above, or lengthens those in directions perpendicular to the first, which would answer the purpose equally well" (Lorentz 1892b, p. 223). In the *Versuch*, Lorentz elaborates on this comment: "If, for example, the dimensions parallel to this direction [of motion] were changed in the proportion of 1 to $1 + \delta$, and those perpendicular in the proportion of 1 to $1 + \varepsilon$, then we should have the equation $\varepsilon - \delta = \frac{1}{2} v^2/c^2$ in which the value of one of the quantities $\delta$ and $\varepsilon$ would remain undetermined" (Lorentz et al. 1952, p. 5). In my notation $1 + \delta = f(\beta)/\gamma$ and $1 + \varepsilon = f(\beta)$. From

$$\frac{1+\varepsilon}{1+\delta} = \gamma \approx 1 + \frac{1}{2} v^2/c^2,$$

it follows that $\varepsilon - \delta \approx \frac{1}{2} v^2/c^2$, the equation Lorentz gives.

contract in this way when set in motion with respect to the ether, the time $\tau$ it takes light to travel back and forth in an arm of a moving interferometer is given by

$$\tau = \frac{2L' f(\beta)}{c\sqrt{1-\beta^2}} = \gamma f(\beta)\frac{2L'}{c}. \tag{3.26}$$

The assumption of such a contraction of objects moving through the ether, Lorentz tells us in 1892, is the only way in which he has been able to reconcile the negative result of the Michelson-Morley experiment with the basic assumption of a stationary ether he took from Fresnel:

> This experiment has been puzzling me for a long time, and in the end I have been able to think of only one means of reconciling its result with Fresnel's theory [of a stationary ether]. It consists in the supposition that the line joining two points of a solid body, if at first parallel to the direction of the earth's motion, does not keep the same length when it is subsequently turned through 90°. (Lorentz 1892b, p. 221)

At the corresponding juncture in the *Versuch*, Lorentz added a footnote saying that he meanwhile found out that FitzGerald had independently come up with the same idea (Lorentz et al. 1952, p. 4).[23]

**3.2.3 The problem of the empirical testability of the contraction hypothesis.** Before I move on to Lorentz's plausibility argument for the contraction hypothesis, I want to look at one other element of Lorentz's 1892 and 1895 discussions of the contraction hypothesis. This element, I think, is the main culprit in creating the false impression that the charge of *ad-hoc*-ness that has often been leveled at the contraction hypothesis is simply to be understood as the charge that it is impossible to subject the hypothesis to an empirical test.[24] Lorentz concludes his discussion of the contraction hypothesis in 1892 with the following observation:

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[23] For a detailed account of the origins of FitzGerald's idea, see Hunt 1991, pp. 185–195. At the end of his detailed historical analysis, Hunt acknowledges that Lorentz independently arrived at the same idea as FitzGerald. In the very next paragraph, he gives his overall conclusion: "What does this account of the origin of FitzGerald's contraction hypothesis tell us, not just about the evolution of an important physical idea but about the operation of the Maxwellian group? First, it shows how important personal interactions and seemingly extraneous circumstances can be in the birth and dissemination of scientific ideas. Had it not been for Heaviside's campaign for recognition, for instance, or FitzGerald's tiff with the Royal Dublin Society, the story of the FitzGerald contraction might have been very different" (Hunt 1991, p. 195). Hunt—and Warwick for that matter (see section 1.3)—rightfully insists on the importance of looking at scientific ideas in their proper context. However, it strikes me as odd to reiterate that point at this juncture, where what we have, by Hunt's own admission, is a clear-cut case of independent discovery.

[24] My diagnosis of the situation was partly inspired by the following comment by Leplin: "It is interesting to note that Lorentz speaks in an adjacent paragraph of the difficulty of testing the hypothesis, suggesting that an interferometer would be necessary, rendering the test circular. But no connection between the problem of testability and the *ad hoc* character of the hypothesis is suggested" (Leplin 1975, p. 314). Leplin's "adjacent paragraph" is referring to the passage in the *Versuch* quoted below (Lorentz et al. 1952, p. 6)

> Since $p/V$ [Lorentz's notation for $v/c$] is equal to 1/10000, the value of $p^2/2V^2$ [the fraction by which an object in the direction of motion is shorter than in directions perpendicular to the direction of motion] becomes one two hundred millionth. A shortening of the earth's diameter to the extent of this fraction would amount to 6 cm. There is not the slightest possibility, when comparing standard measuring rods, of noticing a change in length of one part in two hundred million. Even if the methods of observation permitted, one would never detect by a juxtaposition of two rods anything of the change mentioned, if these occurred to the same extent for both rods. The only way would be to compare the lengths of two rods at right angles to each other, and if one wished to do this by means of observing an interference phenomenon, in which one beam of light travels to and fro along the first rod and the other beam along the second, the result would be a reproduction of Michelson's experiment. But then the influence of the desired change in length would again be compensated by the change in phase differences determined by expression (3) [i.e., in my notation, $T(\theta=0) - T(\theta=\pi/2) = l\beta^2$, where I used Eq. 3.24 for $\tau$]. (Lorentz 1892b, p. 223)

In the *Versuch*, we find essentially the same observation:

> ... the lengthenings and shortenings in question are extraordinarily small. We have $v^2/c^2 = 10^{-8}$, and thus, if $\varepsilon = 0$ [i.e., $f(\beta) = 1$], the shortening of the one diameter of the Earth would amount to about 6.5 cm. The length of a meter rod would change, when moved from one principal position into the other, by about 1/200 micron. One could hardly hope for success in trying to perceive such small quantities except by means of an interference method. We should have to operate with two perpendicular rods, and with two mutually interfering pencils of light, allowing the one to travel to and fro along the first rod, and the other along the second rod. But in this way we should come back once more to the Michelson experiment, and revolving the apparatus we should perceive no displacement of the fringes. (Lorentz et al. 1952, p. 6).

So, Lorentz in the period 1892–1895 thought that the Michelson-Morley experiment provided the only feasible empirical test of the contraction hypothesis. These passages, moreover, suggest that it is the only conceivable test. This would make the contraction hypothesis a paradigm example of an *ad hoc* hypothesis in the falsificationist sense of that term (see Popper 1959, p. 83). However, as has been emphasized by Grünbaum (1959) in response to Popper, the situation is not quite that dire. To be sure, there is only a very small number of conceivable empirical tests of the hypothesis as part of Lorentz's theory in 1892 and 1895, not to mention tests that would have been feasible at the time. However, the possibilities of testing the hypothesis are not limited to the one experiment for which it was introduced, and the possibilities are limited for a very different reason than the one given by Lorentz in the passages quoted above. I will argue in section 3.4 that the problem of the limited testability of the contraction hypothesis completely disappears in subsequent versions of Lorentz's theory.[25]

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[25] Recall Popper's reply to Grünbaum that I quoted in the introduction to part two: "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). I will show that "special relativity" could be replaced by "later versions of Lorentz's theory" in this statement.

Despite the possibility, at least in principle, of empirical tests of the contraction hypothesis as part of Lorentz's theory in 1892 and 1895, and despite the dramatic increase in the number of conceivable tests of the hypothesis as part of Lorentz's theory of 1899 and 1904, the strong intuition lingers, as has been argued powerfully by Holton (1969), that the hypothesis is *ad hoc*. Whatever feature of the theory is responsible for sustaining this intuition, it can not be a lack of falsifiability of the hypothesis.[26] In section 3.3, I will offer a suggestion as to which feature of the theory is responsible for this intuition instead.

Grünbaum has gone to great lengths to argue that the contraction hypothesis is not *ad hoc*. Holton has passionately argued that it is. As will be clear from what has been said above, the views of these two eminent scholars—one a philosopher, the other a historian—on this particular issue are perfectly compatible with one another.[27] Holton (1969, section 8, "Against an *ad hoc* physics," pp. 322–334) rightfully insists on the *historical* fact that many of Lorentz's contemporaries shared Einstein's intuition that the contraction hypothesis and Lorentz's theory in general were *ad hoc*. Grünbaum (1959; 1973, pp. 386–396, 721–724, 834–839 [Grünbaum's response to Holton 1969]; 1976) rightfully insists on the *philosophical* fact that the contraction hypothesis is not *ad hoc* in the falsificationist sense in which philosophers typically use that term.[28]

The task at hand, as I see it, is to articulate what made Einstein and others denounce Lorentz's theory as *ad hoc* if not for falsificationist reasons. This approach, it seems to me, is more fruitful than arguing over whether or not the contraction hypothesis and/or Lorentz's theory in general are *ad hoc* under some alternative definition of *ad-hoc*-ness that philosophers

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[26] By the time of the experiments of Rayleigh (1902), Brace (1904), and Trouton and Noble (1903) at the latest, it was clear that the contraction hypothesis, despite Lorentz's suggestions to the contrary in 1892 and 1895, can actually be subjected to empirical tests.

[27] For a discussion of the by now ancient dispute between Holton and Grünbaum over the history of special relativity, specifically over the importance of the Michelson-Morley experiment for Einstein's path to special relativity, and, more generally, over the prospects of giving a rational reconstruction of the genesis of the theory, see Gutting 1972. Grünbaum lost the battle over the Michelson-Morley experiment. It is generally accepted by now that Einstein definitely knew about the experiment before 1905 (see, e.g., Holton 1988, pp. 477–480). However, there also seems to be complete consensus that this does not seriously affect the conclusion in Holton 1969 that the Michelson-Morley experiment did not play a role of any great significance in Einstein's development of special relativity (see, e.g., Stachel 1982). That does not mean, however, that Grünbaum has lost the war (see, e.g., Earman, Glymour, and Rynasiewicz 1982).

[28] Miller, in my view, was mistaken when he wrote in his critique of Zahar 1973: "Zahar [1973, p. 220] permits Grünbaum to easily dispose of Popper's case for the *ad hocness* of the $L.F.C.$ [Lorentz-FitzGerald contraction]" (Miller 1974, p. 41), to which he added in a footnote: "if one wishes to analyze the status of the $L.F.C.$ *c*. 1900, a goal to which Zahar aspires, then Grünbaum's argument is quite beside the point. The reason is that Grünbaum's argument turns upon the Kennedy-Thorndike experiment of 1932" (ibid.). I agree with this last remark. However, Zahar was quite right to quickly grant Grünbaum his point against Popper. As much as Miller, following Holton, might want to see the contraction hypothesis condemned as *ad hoc*, it is not *ad hoc* in a falsificationist sense.

have no trouble conjuring up.[29] I completely agree with Grünbaum's reaction to Leplin 1975: "I do not see why all the logical features of Einstein's *ad hoc* charge against the LFC [Lorentz-FitzGerald contraction] have to be paradigmatic for that concept" (Grünbaum 1976, pp. 360–361). Grünbaum goes on to quote (approvingly) a list given by Holton (1969, p. 327) of the different usage various scientists have made of the phrase *ad hoc*.

Without wanting to get bogged down in the fine points of *ad-hoc*-ery, I do want to add that Zahar's work, for all its flaws, provides at least an outline of a very strong argument for the claim that the contraction hypothesis and similar hypotheses *in later versions of Lorentz's theory* are not *ad hoc*$_1$ or *ad hoc*$_2$ (i.e., that they do give novel predictions and that these predictions are verified). Grünbaum based his argument on what is clearly no more than a toy model of Lorentz's mature theory. This lends his argument a striking simplicity, but at the price of an undeniable precariousness. What saves the contraction hypothesis from being *ad hoc*, in Grünbaum's argument, are no more than two experiments, the experiment of Kennedy and Thorndike (1932) and the experiment of Ives and Stillwell (1938) on the so-called transverse Doppler effect. As Zahar has shown, a much stronger (but far more complicated) case can be made on the basis of a more realistic model of Lorentz's mature theory. One of my goals in this chapter is to actually make the case along the lines of Zahar, without running afoul of the many historical inaccuracies in his account of the development of Lorentz's theory (see sections 3.3 and 3.5).

**3.2.4 Variations on the Michelson-Morley experiment (Kennedy-Thorndike, Liénard).** The most famous test of the Lorentz-FitzGerald contraction hypothesis in the philosophical literature is the experiment of Kennedy and Thorndike that I already mentioned above. It was brought to the attention of philosophers of science by Grünbaum (1959) and plays an important role in the work of Zahar (1973, pp. 219–220; 1989, p. 48). Consider Eq. 3.26. The time $\tau = \gamma f(\beta)(2L'/c)$ it takes light to travel back and forth in the arm of an interferometer is independent of the arm's orientation with respect to the "ether wind," but it still differs from the time $2L'/c$ it would take if the arm were at rest in the ether. Rotating an interferometer while $\mathbf{v}$ remains essentially constant, as Michelson and Morley did, we do not expect to find an effect. However, observing the interference pattern in an interferometer over a period of time in which we have reason to believe that the velocity of the earth with respect to the ether (or rather its component in the plane of the interferometer) changes, we would expect to find an effect. Kennedy and Thorndike tried to measure this effect. Not surprisingly, the result of their

[29] See, e.g., Zahar's "*ad hoc*$_1$," "*ad hoc*$_2$," and "*ad hoc*$_3$" (Zahar 1973, p. 216; 1989, p. 12); and Leplin's (1975) manifold conditions of *ad-hoc*-ness.

experiment, like that of Michelson and Morley, was negative. Otherwise, we would have a violation of the principle of relativity.

The negative result of Kennedy and Thorndike can be accounted for in various ways in the context of Lorentz's theory of 1892–1895. The simplest solution would be to choose $f(\beta) = 1/\gamma$, making $\tau$ independent of $\mathbf{v}$ as well as of $\theta$. However, by 1904–1905, Lorentz had compelling reasons, as we will see in sections 3.4 and 3.5, for setting $f(\beta) = 1$. The explanation of the Kennedy-Thorndike experiment licensed by Lorentz's theory from 1899 onward is, in fact, somewhat more complicated. The explanation is that the period of light emitted by the light source in the interferometer has the exact same velocity dependence as the time $\tau$, so that their quotient, i.e., the phase difference actually measured in both the Michelson-Morley experiment and the Kennedy-Thorndike experiment, is velocity independent. I will return to this phenomenon and to the Kennedy-Thorndike experiment in section 3.3.

Here I want to draw attention to another test of the Lorentz-FitzGerald contraction hypothesis, proposed by Liénard (1898; discussed in Hirosige 1966, p. 24). As we will see in section 3.3, this proposal played an important role in the development of Lorentz's theory. Consider Eq. 3.24 for $\tau$ and Eq. 3.25 for $L$. Inserting the latter equation into the former, we found that $\tau$ is $\theta$-independent under the contraction hypothesis, which accounts for the negative result of the Michelson-Morley experiment. However, imagine that we repeat the experiment with water or glass in the arms of the interferometer. It looks as if this experiment ought to give a positive result. After all, it would seem that $\beta = v/c$ in Eq. 3.24 for $\tau$ gets replaced by $n\beta$, where $n$ is the index of refraction of the material through which the light is made to travel in the arms of the interferometer, whereas Eq. 3.25 for $L$ stays the same. For $\tau$, we would then find

$$\tau = \gamma f(\beta) \frac{2l'}{c} \sqrt{\frac{1 - n^2\beta^2 \sin^2\theta}{1 - \beta^2 \sin^2\theta}}\,, \qquad \text{(Eq. 3.27)}$$

which would make $\tau$ $\theta$-dependent. There are two complications. First, we have to take into account the Fresnel dragging effect. Second, the index of refraction of the material we put in the arms of the interferometer might be affected by the Lorentz-FitzGerald contraction (cf. Rayleigh (1902) and Brace (1904)). Nonetheless, taking these complications into consideration, if need be with the help of additional hypotheses, we can work out the prediction of Lorentz's theory for the experiment for Liénard and empirically check the result. This clearly constitutes a test of the contraction hypothesis.

The Kennedy-Thorndike experiment and the suggestion by Liénard provide valuable hints for understanding exactly why it is that the testability of the contraction hypothesis as part of Lorentz's theory of 1892 and 1895 is severely limited. The Lorentz-FitzGerald contraction may

affect a wide range of phenomena, thus making it possible in principle to subject the contraction hypothesis to a wide range of empirical tests, but Lorentz's theory does not give definite predictions for such experiments without additional assumptions about further effects of motion through the ether on the relevant phenomena (on the period of light emitted by a moving source in the case of the Kennedy-Thorndike experiment, on the index of refraction of moving water or glass in the case of Liénard's suggestion). This problem, already present in the relatively minor variations on the Michelson-Morley experiments considered above, can only get worse if we start considering tests of the contraction hypothesis that are very different from the Michelson-Morley experiment, such as, say, the Trouton-Noble experiment. The problem that Lorentz's theory does not give definite predictions without additional assumptions is a direct consequence of the fact that, in Lorentz's preliminary[30] theory of 1892 and 1895, first order effects and second order effects are treated separately. As I will show in section 3.3, the problem disappears once this feature is removed in the more mature versions of Lorentz's theory of 1899 and 1904.

**3.2.5 A "corresponding states"-like treatment of electrostatics in moving frames of reference.** In section 1.2, I already mentioned that Lorentz gave a plausibility argument for the Lorentz-FitzGerald contraction hypothesis based on the relation $\mathbf{F} = \mathrm{diag}\,(1, 1/\gamma, 1/\gamma)\,\mathbf{F}'$ between the force $\mathbf{F}$ on a charge $q$ in a static charge distribution in uniform motion and the force $\mathbf{F}'$ on that same charge $q$ if that charge distribution were to be stretched out by a factor $\gamma$ and at rest in the ether. Since the relation is important in the context of both the Trouton-Noble experiment and the Michelson-Morley experiment, as well as for the subsequent development of Lorentz's theorem of corresponding states, it will be worth our while to give a reconstruction of the derivation of the result in the *Versuch* (Lorentz 1895, sections 19–23, pp. 31–37; see also Zahar 1989, pp. 59–61).

Although the problem of electrostatics in moving frames is treated separately from the problem of optics in moving frames in the *Versuch*, Lorentz proceeds in very much the same way.[31] With the help of some auxiliary quantities, he recasts the problem in the moving frame in the form of a problem in a frame at rest in the ether for which the solution is already known. The original problem can then be solved by taking the known solution for the problem in the frame at rest in the ether, rewriting it in terms of the auxiliary quantities for the problem in the frame moving through the ether, and, finally, performing a transformation from the auxiliary quantities to the real quantities.

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[30] Recall that his 1895 book is called "Attempt at a theory ..."

[31] This may be responsible for a curious misconception by Zahar (1989, sections 2.3–2.4, pp. 58–66), who associates the theorem of corresponding states in the *Versuch* with these calculations in electrostatics rather than with the calculations in optics. For a careful analysis of this point, which unfortunately was completely lost on Zahar, see Miller 1974, pp. 33–37.

In the case of electrostatics in the moving frame $S$, Lorentz wants to recover the following equations that govern electrostatics in the frame $S_0$ at rest in the ether:

$$\begin{aligned} &\text{Poisson equation:} && \Delta_0 \phi = -\rho/\varepsilon_0, \\ &\text{Lorentz force on a test charge } q \text{ at } \mathbf{x}_0 = \mathbf{a}: && \mathbf{F} = -q\, \nabla_0 \phi \big|_{\mathbf{x}_0 = \mathbf{a}} \end{aligned} \tag{3.28}$$

where $\Delta_0$ is the Laplacian in terms of the coordinates $\mathbf{x}_0$ of the $S_0$ frame,

$$\Delta_0 \equiv \frac{\partial^2}{\partial x_0^2} + \frac{\partial^2}{\partial y_0^2} + \frac{\partial^2}{\partial z_0^2}, \tag{3.29}$$

and where $\nabla_0$ is the gradient operator in terms of $\mathbf{x}_0$. The charge density $\rho$ and the potential $\phi$ in Eq. 3.28 are both functions of $\mathbf{x}_0$, but not of the time $t_0$.

Suppose we have a static charge distribution in a moving frame, i.e., we have a charge density $\rho$ that is a function of the spatial coordinates $\mathbf{x}$ of the moving frame $S$, but not of the time $t = t_0$. Lorentz manages to cast the problem of finding the force a test particle at rest in $S$ experiences from the field of such a charge distribution in a form that, though not quite the same, is very similar to Eq. 3.28. He introduces the potential function $\omega$ that satisfies the Poisson-type equation

$$\left(\Delta - \beta^2 \frac{\partial^2}{\partial x^2}\right) \omega = -\rho/\varepsilon_0, \tag{3.30}$$

where $\Delta$ is the Laplacian in the frame $S$, and where $\omega$ and $\rho$ are functions of $\mathbf{x}$. He shows that the force on a test charge $q$ at some arbitrary point $\mathbf{x} = \mathbf{a}$ can be obtained from this potential function $\omega$ through

$$\mathbf{F} = -q\left(1 - \beta^2\right) \nabla \omega \big|_{\mathbf{x} = \mathbf{a}}. \tag{3.31}$$

The derivation of Eqs. 3.30–3.31 is considerably more complicated than the two-line derivation of Eq. 3.28. A static charge distribution in a moving frame will give rise to both an electric and a magnetic field. Both these fields will contribute to the Lorentz force $\mathbf{F}$. To write $\mathbf{F}$ as a gradient of a potential function $\omega$, as is done in Eq. 3.31 above, we therefore need to express the components of both fields as derivatives of $\omega$.

For an electrostatic problem in the moving frame $S$ (i.e., $\rho = \rho(\mathbf{x})$, $\mathbf{E} = \mathbf{E}(\mathbf{x})$, $\mathbf{B} = \mathbf{B}(\mathbf{x})$, and $\mathbf{u} = 0$), the basic equations for $\mathbf{E}$ and $\mathbf{B}$ in Eq. 3.3 (i.e., Maxwell's equations transformed to the moving frame $S$) reduce to

$$\begin{aligned} &\text{div } \mathbf{E} = \rho/\varepsilon_0, \qquad \text{curl } \mathbf{E} = v\,\frac{\partial \mathbf{B}}{\partial x}, \\ &\text{div } \mathbf{B} = 0, \qquad \text{curl } \mathbf{B} = \mu_0 \rho\, \mathbf{v} - \frac{v}{c^2}\frac{\partial \mathbf{E}}{\partial x}. \end{aligned} \tag{3.32}$$

To recast these equations in the form of Eq. 3.30, we use the well-known relation

$$\text{curl curl } \mathbf{A} = \text{grad (div } \mathbf{A}) - \Delta\, \mathbf{A}, \tag{3.33}$$

from vector calculus (where $\mathbf{A}(\mathbf{x})$ is some arbitrary well-behaved vector field), plus the fact that we are free to change the order of differentiation in all equations we will encounter. Taking the curl of the equation with curl $\mathbf{E}$ in Eq. 3.32, we find:

$$\text{curl curl } \mathbf{E} = \text{curl}\left(v\,\frac{\partial \mathbf{B}}{\partial x}\right). \tag{3.34}$$

Using Eq. 3.33 for the left hand side, and Eq. 3.32 for curl $\mathbf{B}$ on the right hand side, we can rewrite Eq. 3.34 as

$$\text{grad (div } \mathbf{E}) - \Delta\, \mathbf{E} = v\,\frac{\partial}{\partial x}\left(\mu_0 \rho\, \mathbf{v} - \frac{v}{c^2}\frac{\partial \mathbf{E}}{\partial x}\right). \tag{3.35}$$

Using that div $\mathbf{E} = \rho/\varepsilon_0$ (see Eq. 3.32) and that $\mu_0 = 1/(c^2\varepsilon_0)$, and rearranging the various terms, we can rewrite Eq. 3.35 as

$$\left(\Delta - \beta^2\,\frac{\partial^2}{\partial x^2}\right)\mathbf{E} = \frac{1}{\varepsilon_0}\begin{pmatrix} (1-\beta^2)\,\partial\rho/\partial x \\ \partial\rho/\partial y \\ \partial\rho/\partial z \end{pmatrix}. \tag{3.36}$$

A similar equation can be derived for $\mathbf{B}$. Taking the curl of the equation with curl $\mathbf{B}$ in Eq. 3.32, we find:

$$\text{curl curl } \mathbf{B} = \text{curl}\left(\mu_0 \rho\, \mathbf{v} - \frac{v}{c^2}\frac{\partial \mathbf{E}}{\partial x}\right). \tag{3.37}$$

Using Eq. 3.33 for the left hand side, and Eq. 3.32 for curl **E** on the right hand side, we can rewrite Eq. 3.34 as

$$\text{grad (div } \mathbf{B}) - \Delta\, \mathbf{B} = \mu_0 \text{ curl } \rho \mathbf{v} - \frac{v}{c^2} \frac{\partial}{\partial x} \left( v \frac{\partial \mathbf{B}}{\partial x} \right). \tag{3.38}$$

Using that div **B** = 0 (see Eq. 3.32), that $\mu_0 = 1/(c^2 \varepsilon_0)$, that

$$\text{curl } \rho \mathbf{v} = \left( 0,\ v \frac{\partial \rho}{\partial z}, - v \frac{\partial \rho}{\partial y} \right), \tag{3.39}$$

and rearranging the various terms, we can rewrite Eq. 3.38 as

$$\left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \mathbf{B} = \frac{1}{\varepsilon_0} \frac{v}{c^2} \begin{pmatrix} 0 \\ -\partial\rho/\partial z \\ \partial\rho/\partial y \end{pmatrix}. \tag{3.40}$$

At this point, Lorentz introduced the potential function ω, defined as the solution of the Poisson-type equation

$$\left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \omega = -\rho/\varepsilon_0, \tag{3.41}$$

which is just Eq. 3.30. Using Eq. 3.41 to eliminate ρ from Eq. 3.36 for **E** and Eq. 3.40 for **B**, and once again using the fact that we can change the order of differentiation at will, we find:

$$\left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \mathbf{E} = - \left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \begin{pmatrix} (1 - \beta^2)\ \partial\omega/\partial x \\ \partial\omega/\partial y \\ \partial\rho/\partial z \end{pmatrix}, \tag{3.42}$$

$$\left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \mathbf{B} = \frac{v}{c^2} \left( \Delta - \beta^2 \frac{\partial^2}{\partial x^2} \right) \begin{pmatrix} 0 \\ \partial\omega/\partial z \\ -\partial\omega/\partial y \end{pmatrix}. \tag{3.43}$$

A solution of Eqs. 3.42–3.43 is given by

$$\mathbf{E} = \left( -\left(1 - \beta^2\right) \frac{\partial \omega}{\partial x}, - \frac{\partial \omega}{\partial y}, - \frac{\partial \omega}{\partial z} \right), \quad \mathbf{B} = \left( 0,\ \frac{v}{c^2} \frac{\partial \omega}{\partial z}, - \frac{v}{c^2} \frac{\partial \omega}{\partial y} \right). \tag{3.44}$$

One can verify directly, as Lorentz (1895, p. 36) points out, that **E** and **B** in Eq. 3.44 satisfy the original equations for these fields in Eq. 3.32. Inserting Eq. 3.44 into the various components of $\mathbf{F} = q\left(\mathbf{E} + \mathbf{v} \times \mathbf{B}\right)$, we arrive at Eq. 3.31:

$$\begin{aligned} F_x &= q\,E_x = -q\left(1-\beta^2\right)\frac{\partial\omega}{\partial x}, \\ F_y &= q\,(E_y - vB_z) = -q\left(1-\beta^2\right)\frac{\partial\omega}{\partial y}, \\ F_z &= q\,(E_y + vB_y) = -q\left(1-\beta^2\right)\frac{\partial\omega}{\partial z}. \end{aligned} \tag{3.45}$$

To summarize the result so far: with the help of the function ω, Lorentz has been able to cast the problem of electrostatics in a moving frame in the form of the following two equations, which are very similar to the equations governing electrostatics in a frame at rest in the ether (see Eq. 3.28)

$$\begin{aligned} &\text{Poisson-type equation:} && \left(\Delta - \beta^2\frac{\partial^2}{\partial x^2}\right)\omega = -\rho/\varepsilon_0, \\ &\text{Lorentz force on a test charge } q \text{ at } \mathbf{x} = \mathbf{a}: && \mathbf{F} = -q\left(1-\beta^2\right)\nabla\omega\big|_{\mathbf{x}=\mathbf{a}}. \end{aligned} \tag{3.46}$$

Lorentz now introduces some auxiliary quantities to give the equation for the potential function exactly the form of the Poisson equation:

$$\begin{gathered} x' = \gamma x, \quad y' = y, \quad z = z' \\ \rho' = \rho/\gamma, \quad \omega' = \omega/\gamma. \end{gathered} \tag{3.47}$$

Notice that

$$\left(1-\beta^2\right)\frac{\partial^2}{\partial x^2} = \frac{\partial^2}{\partial x'^2}, \tag{3.48}$$

and that

$$\nabla\omega = \mathrm{diag}\left(\gamma^2, \gamma, \gamma\right)\nabla'\omega'. \tag{3.49}$$

So, the first equation in Eq. 3.46 can be rewritten in the form of the Poisson equation

$$\Delta' \omega' = -\rho'/\varepsilon_0, \tag{3.50}$$

where $\Delta'$ is the Laplacian in terms of the auxiliary coordinates $\mathbf{x}'$. The second equation in Eq. 3.46 can be rewritten as

$$\mathbf{F} = -q \operatorname{diag}(1, 1/\gamma, 1/\gamma)\, \nabla' \omega' \big|_{\mathbf{x}' = \operatorname{diag}(\gamma, 1, 1)\, \mathbf{a}}. \tag{3.51}$$

Since Eq. 3.50 has exactly the form of the Poisson equation, we can look upon it as giving the equation for the potential of the field generated by the charge distribution $\rho(\mathbf{x}_0) = \rho'(\mathbf{x}_0 = \mathbf{x}')$ at rest in the ether. It follows from the relations $x' = \gamma x$ and $\rho' = \rho/\gamma$ in Eq. 3.47 that this charge distribution at rest in the ether differs from the moving one we are interested in only in that it is stretched out by a factor $\gamma$ in the direction of the $x$-axis. The force $\mathbf{F}'$ a test charge $q$ experiences in the field generated by $\rho(\mathbf{x}_0) = \rho'(\mathbf{x}_0 = \mathbf{x}')$ in $S_0$ is given by (see Eq. 3.28)

$$\mathbf{F}' = -q\, \nabla' \omega' \big|_{\mathbf{x}' = \operatorname{diag}(\gamma, 1, 1)\, \mathbf{a}}. \tag{3.52}$$

Inserting Eq. 3.52 into Eq. 3.51, we find

$$\mathbf{F} = \operatorname{diag}(1, 1/\gamma, 1/\gamma)\, \mathbf{F}'. \tag{3.53}$$

**3.2.6 Lorentz's plausibility argument for the Lorentz-FitzGerald contraction.** In section 1.2, I already explained how Eq. 3.53 can be turned into a general strategy for dealing with electrostatics in moving frames (see the discussion following Fig. 1.6). Here, I want to look at how the result was used by Lorentz to make the Lorentz-FitzGerald contraction hypothesis plausible.

Lorentz argued that we obtain a contraction of the type needed to account for the negative result of the Michelson-Morley experiment (more specifically, a contraction with $f(\beta) = 1$), if we assume that Eq. 3.53 not only holds for Coulomb forces but also for the intermolecular forces holding a Michelson interferometer together. This argument can be found in virtually identical form in Lorentz 1892b (p. 222) and in Lorentz 1895 (see Lorentz et al. 1952, p. 7). I will quote the earlier version, mainly to show that Lorentz had already derived Eq. 3.53 in 1892, even though he only published the result in 1895 (cf. Miller 1974, pp. 33–37).

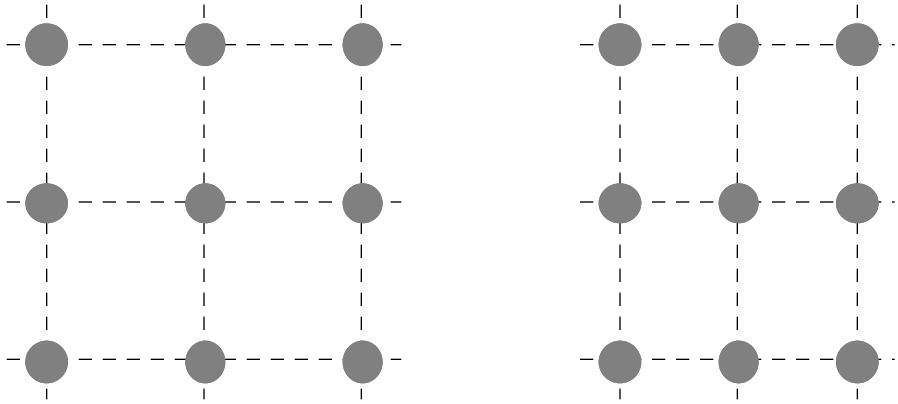

Figure 3.5: Crude static model of the molecular structure of an arm of a Michelson interferometer; on the left: at rest in the ether ("system *C*"); on the right: moving through the ether ("system *B*")

Fig. 3.5 shows the systems *C* and *B* playing a role in the third paragraph of this quotation, where the result for electrostatic forces mentioned in the first two paragraphs are applied to molecular forces. Lorentz writes:

> Let *A* be a system of material points carrying certain electric charges and at rest with respect to the ether; *B* the system of the same points while moving in the direction of the *x*-axis with the common velocity *p* through the ether. From the equations developed by me[1)][32], one can deduce which forces the particle[s] in system *B* exert on one another. The simplest way to do this,[33] is to introduce still a third system *C*, which just as *A*, is at rest but differs from the latter as regards the location of the points. System *C*, namely, can be obtained from system *A* by a simple extension by which all dimensions in the direction of the *x*-axis are multiplied by the factor $(1 + p^2/2V^2)$[34] and all dimensions perpendicular to it remain unaltered.
>
> Now the connection between the forces in *B* and *C* amounts to this, that the *x*-components in *C* are equal to those in *B* whereas the components at right angles to the *x*-axis are $1 + p^2/2V^2$ times large[r] than in *B*.[35]
>
> We will apply this to molecular forces. Let us imagine a solid body to be a system of material points kept in equilibrium by their mutual attractions and repulsions and let system *B* represent such a body whilst moving through the ether. The forces acting on any of the material points of *B* must in that case neutralize. From the above, it follows that the same can not then be the case for system *A* whereas for system *C* it can; for even though a transition from *B* to *C* is accompanied by a change in all forces at right angles to the axis, this cannot disturb the equilibrium, because they are all changed in the same proportion. In this way it appears that if *B* represents the state of equilibrium of the body during a shift through the ether

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[32] The corresponding footnote gives a reference to p. 498 of a lengthy memoir he published earlier that year. Presumably, the reference is to Eq. (61) on this page (Lorentz 1892a, p. 238), which gives an expression for the Lorentz force a charge distribution experiences from its self-field (cf. Eq. 3.94 below).

[33] This method is not to be found in Lorentz 1892a and was published only in Lorentz 1895.

[34] Recall that in 1892 Lorentz wrote $p/V$ for $\beta$. Hence, $(1 + p^2/2V^2)$ is just $\gamma$ to first order in $\beta$.

[35] The Dutch original of this short paragraph contains a misprint, which may explain why it was reworded in the translation. The literal translation of the Dutch is: "The relation between the forces in *B* and *C* comes down to this, that the components in the direction of the *x*-axis in *B* are the same as in *A* [this clearly should be *C*] while the components perpendicular to the *x*-axis are $1 - p^2/2V^2$ as large as in *C*" (Lorentz 1892b, p. 77 in the original Dutch version).

> then $C$ must be the state of equilibrium when there is no shift.[36] But the dimensions of $B$ in the direction of the $x$-axis are $(1 - p^2/2V^2)$ times the corresponding dimensions of $C$ whereas the dimensions along directions at right angles to the $x$-axis are the same in both systems. One obtains, therefore, exactly an influence of the motion on the dimensions equal to the one which, as appeared above, is required to explain Michelson's experiment. (Lorentz 1892b, p. 222)

Lorentz made it very clear, both in 1892 and in 1895, that this remarkable result should be seen as a plausibility argument for the Lorentz-FitzGerald contraction, not as a derivation of the contraction from a new hypothesis concerning the effect of ether drift on molecular forces.[37] The paragraphs immediately preceding and immediately following the passage quoted above contain numerous disclaimers to this effect:

> Now, some such change in the length of the arms in Michelson's first experiment and in the dimensions of the slab[38] in the second one is so far as I can see not inconceivable. What determines the size and shape of a solid body? Evidently the intensity of the molecular forces; any cause which would alter the latter would also influence the shape and dimensions. Nowadays we may safely assume that electric and magnetic forces act by means of the intervention of the ether. It is not far-fetched to suppose the same to be true of the molecular forces. But then it may make all the difference whether the line joining two material particles shifting together through the ether, lies parallel or crosswise to the direction of that shift. It is easily seen that an influence of the order of $p/V$ is not to be expected, but an influence of the order of $p^2/V^2$ is not excluded and that is precisely what we need.
>
> Since the nature of the molecular forces is entirely unknown to us, it is impossible to test the hypothesis. We can only calculate—with the aid of more or less plausible suppositions, of course—the influence of the motion of ponderable matter on the electric and magnetic forces. It may be worth mentioning that the result obtained in the case of electric forces yields, when applied to molecular forces, exactly [a contraction in the direction of motion of the required magnitude]
>
> [The three paragraphs quoted above]
>
> One may not of course attach much importance to this result;[39] the application to molecular forces of what was found to hold for electric forces is too venturesome for that. (Lorentz 1892b, pp. 221–223)

In 1895, Lorentz added very similar disclaimers to the presentation of his plausibility argument

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36 Notice that Lorentz tacitly assumes here that the equilibrium state is unique.

37 On this issue, I am in complete agreement with Miller's (1974, p. 39) and Schaffner's (1974, p. 50) criticism of Zahar (1973, p. 221; 1989, pp. 62–65) who claims that Lorentz wanted to derive the "LFC" from the "MFH" (Zahar's abbreviations for "Lorentz-FitzGerald Contraction" and "Molecular Force Hypothesis," respectively). Zahar also claims that "in his deduction of the LFC, Lorentz made use of his famous transformation" (Zahar 1973, p. 221). This is probably related to Zahar's identification (mentioned above) of the theorem of corresponding states in the *Versuch* with the calculations in electrostatics rather than in optics.

In his efforts to convince Zahar of the error of his ways, Miller, I think, denounces Lorentz's contraction hypothesis of 1892 and 1895 a little more than necessary. I hope my comments plus an important passage I will quote from Lorentz's unpublished papers (a document not available to Miller in 1974) will serve both to underscore the validity of Miller's points against Zahar and to restore some balance in the assessment of Lorentz's achievement in 1892 and 1895 (see also Nersessian 1988)

38 The Dutch original simply has "stone" (*steen*, Lorentz 1892b, p. 76).

39 A less ambiguous translation of the original Dutch would be: "Of course, not much weight *can* be attached to this result" (Lorentz 1892b, p. 78 in the original Dutch version; my italics).

> Surprising as this hypothesis may appear at first sight, yet we shall have to admit that it is by no means far-fetched, as soon as we assume that molecular forces are also transmitted through the ether, like the electric and magnetic forces of which we are able at the present time to make this assertion definitely. If they are so transmitted, the translation will probably affect the action between the two molecules or atoms in a manner resembling the attraction or repulsion between charged particles. Now, since the form and dimensions of a solid body are ultimately conditioned by the intensity of molecular actions, there cannot fail to be a change of dimensions as well.
>
> [...]
>
> It is worth mentioning that we are led to just the same changes of dimensions [needed for the explanation of the negative result of the Michelson-Morley experiment] if we, *firstly*, without taking molecular movement into consideration, assume that in a solid body left to itself the forces, attractions or repulsions, acting upon any molecule maintain one another in equilibrium, and, *secondly*—though to be sure, there is no reason for doing so—if we apply to these molecular forces the law which in another place [i.e. section 23 of Lorentz 1895] we deduced for electrostatic actions. (Lorentz et al. 1952, pp. 5–7; italics in the original)

To underscore that Lorentz's hesitation to apply the result found for Coulomb forces to molecular forces was entirely appropriate and not an instance of Lorentz being overly cautious, I want to draw attention to a serious problem for Lorentz's argument.[40] According to an elementary theorem in electrostatics named after Earnshaw who found it in 1831 (Pais 1988, p. 181), a system of charges can not be in stable static equilibrium unless there are other forces present, besides the Coulomb forces, to prevent the charges from moving.[41] The basic premise of Lorentz's argument is: if a system is in stable static equilibrium at rest in the ether, then a contracted version of that system is in stable static equilibrium in uniform motion through the ether (cf. Lorentz 1895, pp. 37–38). Earnshaw's theorem shows that it is impossible to have a system in stable static equilibrium under the influence of Coulomb-type forces only. This reduces Lorentz's premise to a trivial and empty truth. It is reasonable to expect that the implication will not change much when molecular motion is taken into account, but Lorentz does not make any attempt to reassure his readers that this expectation is well founded.[42] Once

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[40] I am grateful to Jon Dorling for drawing my attention to this problem.

[41] Landau and Lifschitz (1984, p. 104) outline a very simple proof of this theorem. Suppose we want to build up a charge distribution in stable static equilibrium. Consider adding the charge $q$ to the total charge $Q$ already present at that moment. For the system to be in stable static equilibrium on the basis of its Coulomb attractions and repulsions alone, $q$ has to be put in at a point $P$ where the potential $\phi$ of the electric field due to $Q$ has an extremum. Hence, the first order derivatives of $\phi$ at $P$ must be zero, while the second order derivatives must be non-zero (otherwise we would have a point of inflection rather than an extremum) and must have the same sign (otherwise we would have a saddle point rather than an extremum). It follows that $\Delta\phi \neq 0$ at $P$. But that contradicts that $\phi$ is an electrostatic potential satisfying the Poisson equation $\Delta\phi = -\rho/\varepsilon_0$. The point $P$ cannot be a point already occupied by one of the charges constituting $Q$. Hence $\rho = 0$ at $P$. That means that $\Delta\phi = 0$. So, we have a contradiction. It follows that $\phi$ cannot have any extrema. This, in turn, implies Earnshaw's theorem.

[42] Although Lorentz does not mention Earnshaw's theorem anywhere in either Lorentz 1892b or Lorentz 1895, there are some clear indications that he realized that there is no such thing as static equilibrium. In the last passage I quoted, he emphasizes this assumption along with the basic assumption that $\mathbf{F} = \mathrm{diag}\,(1, 1/\gamma, 1/\gamma)\,\mathbf{F}'$ holds for Coulomb forces and molecular forces alike. In the final paragraph of his discussion of the Michelson-Morley experiment in the *Versuch*, he writes: "In reality the molecules of a body are not at rest, but in every "state of equilibrium" there is a stationary movement. What influence this circumstance may have in the

again, this shows that Lorentz saw his argument from electrostatics as a plausibility argument for the contraction hypothesis, not as a derivation. In the latter reading, we would have to attribute a serious logical blunder to Lorentz. I see no grounds for such an uncharitable interpretation of his text.

Having put Lorentz's plausibility argument for the contraction hypothesis in proper perspective, I want to emphasize that Lorentz appears to have been very confident in 1892 and 1895 to have hit upon the correct explanation of the Michelson-Morley experiment.[43] This can be gathered, for instance, from the following remarks in the concluding paragraph of Lorentz 1892b:

> But for all that [i.e., all the reservations Lorentz expresses with respect to his plausibility argument for the contraction hypothesis], it seems undeniable that changes in the molecular forces and, consequently, in the dimensions of a body are possible of the order of $p^2/2V^2$. This being so, Michelson's experiment can no longer furnish any evidence for the question for which it was undertaken [i.e., to decide between Stokes' theory of an ether dragged along by the earth and Fresnel's theory of a stationary ether (see Swenson 1972; Janssen 1988)]. Its significance—if one accepts Fresnel's theory—lies rather in the fact, that it can teach us something about the changes in the dimensions. (Lorentz 1892b, p. 223)

To conclude this section I want to quote from a document written years later in which Lorentz looks back upon his introduction of the contraction hypothesis. The document is the draft of a letter from Lorentz to Einstein dated January 1915,[44] in which Lorentz responds to some disparaging remarks by Einstein concerning the contraction hypothesis. Einstein had recently written: "This type and manner of accounting for experiments with negative results through hypotheses invented *ad hoc* is very unsatisfactory" (Einstein 1915, p. 707[45]). Lorentz quotes this remark and adds the following interesting comments:

> That is what Poincaré also said,[46] and I myself have agreed to *that*; I felt the need for a more *general* theory, as I tried to develop later, and as has actually been developed by you (and to a lesser extent by Poincaré). However, my approach was not so terribly unsatisfactory. Lacking a general theory, one can derive some pleasure from the explanation of an isolated fact,[47] as

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phenomenon which we have been considering is a question which we do not here touch upon" (Lorentz et al. 1952, p. 7).

[43] This point, in my opinion, does not get enough emphasis in Miller 1974.

[44] This document—of paramount importance in this context—was discovered by A. J. Kox, to whom I am indebted for alerting me to it and for providing me with his transcription of it. The document is now part of the Archief H.A. Lorentz, Rijksarchief Noord-Holland, Haarlem, The Netherlands. Portions of this document are quoted in Nersessian 1984, pp. 118–119, 172–173; 1986, p. 225, pp. 232–233; 1988, pp. 74–75.

[45] As far as I have been able to tell, Einstein first made this point in print in the introduction of his 1907 review article on relativity (Einstein 1907c, pp. 412-413)

[46] In his lecture at the *Congrès international de Physique* in Paris in 1900, Poincaré had sharply criticized what he perceived to be an accumulation of hypotheses in Lorentz's theory, sarcastically pointing out that "hypotheses are what we lack the least" (Poincaré 1900a, p. 172)

[47] Larry Laudan has argued that this is a perfectly respectable position to take: "If some theory $\boldsymbol{T}_2$ has solved more empirical problems than its predecessor—*even just one more*—then $\boldsymbol{T}_2$ is clearly preferable to $\boldsymbol{T}_1$, and, *ceteris paribus*, represents cognitive progress with respect to $\boldsymbol{T}_1$" (Laudan 1977, p. 115; italics in the original).

> long as the explanation is not artificial. And the interpretation given by me and FitzGerald was not artificial. It was more so that it was the only possible one, and I added the comment that one arrives at the [contraction] hypothesis if one extends to other forces what one could already say about the influence of a translation on electrostatic forces. Had I emphasized this more,[48] the hypothesis would have created less of an impression of being invented *ad hoc*.[49] (Draft of a letter from Lorentz to Einstein, January 1915; emphasis in original)

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Laudan goes on to raise the question "why the admittedly ad hoc character of the Lorentz contraction constitutes a decisive handicap against it in comparing it with special relativity. If the empirical problem-solving capacities of the two theories are, so far as we can tell, equivalent, then they are empirically on a par; defenders of the view that the adhocness of $\boldsymbol{T}_2$ [read: Lorentz's theory] makes it distinctly inferior to $\boldsymbol{T}_n$ [read: special relativity] must spell out why, in such cases, the comparable problem-solving abilities and equivalent degrees of empirical support can be thrown to the winds simply by stipulating that ad hoc theories are intrinsically otiose" (ibid., p. 116). The passage from which these two quotations are taken is quoted in full (and approvingly) in Grünbaum 1976, pp. 358–359. I will take up Laudan's challenge in chapter four and show that there are good reasons, *in this particular case*, for preferring "$\boldsymbol{T}_n$" over "$\boldsymbol{T}_2$," reasons that (although I will not pursue the point in this dissertation) can be seen as a new articulation of the *ad hoc* charge leveled against Lorentz's theory.

[48] At this point Lorentz added a note with an important confession: "I must admit, however, that I only noticed this, *after* [emphasis in the original] I had found the hypothesis." This is hard to reconcile with Zahar's interpretation of Lorentz's argument in support of the contraction hypothesis. I will just quote two passages from Zahar's work that are clearly refuted by the letter Kox discovered. "*... the MFH arose out of considerations which had nothing to do with Michelson's experiment*" (Zahar 1973, p. 22; italics in the original). "Lorentz did nothing about the 'crucial' experiment [of Michelson and Morley] until he discovered his transformation equations [by which Zahar means Eq. 3.47 for electrostatics, not Eq. 3.4–3.5 for optics]; the latter were not discovered under the impact of Michelson's result of which they are independent" (Zahar 1989, p. 65). Of course, the 'independence'-claim in the last sentence is still correct.

[49] At this point, Lorentz adds what I think is a very perceptive comment (see section 3.3). Recall that Einstein had written that "this type and manner of accounting for experiments with *negative* results through hypotheses invented *ad hoc* is very unsatisfactory" (my emphasis). Lorentz writes: "whether it is the explanation of a negative or a positive result that is at issue, hardly makes a difference, it seems to me."

## 3.3 The exact theorem of corresponding states and the "generalized contraction hypothesis" (1899/1904)

**3.3.1 The generalization of the theorem of corresponding states.** As is clear from the title of the book, the theory for optics in moving frames set forth in Lorentz's *Versuch* of 1895 is still provisional. The trouble is that the theory consists of two totally unrelated parts: the theorem of corresponding states providing a general explanation for all first order ether drift experiments and a set of special hypotheses to account for a handful of second order experiments. Since the outcome of both first and second order experiments was always the same, viz. that the earth's motion through the ether has no influence whatsoever on the observed phenomena, this distinction is highly artificial (cf. Poincaré 1900a, p. 172). Not surprisingly, Lorentz set out to construct a general theory, valid to all orders of $v/c$, based on an exact version of the theorem of corresponding states, that would give a unified account of the negative results of both first and second order ether drift experiments.

The canonical exposition of this theory is Lorentz 1904b. In section 1.4, I already discussed the explanation of the experiments of Trouton and Noble given in this paper. My exposition of the theory in this section will follow Lorentz's discussion in an earlier paper, entitled "Simplified theory of electrical and optical phenomena in moving bodies" (Lorentz 1899a, 1899b, 1902[50]). It was in the final section of this paper, that Lorentz laid the foundations of his 1904 theory.

In the last section of this 1899 paper, Lorentz turns to the Michelson-Morley experiment, the contraction hypothesis, and the experiment proposed by Liénard in 1898 (see section 3.2):

> Some time ago Liénard [1898] has emitted the opinion that, according to my theory, the [Michelson-Morley] experiment should have a positive result, if it were modified in so far that the rays had to pass through a solid or a liquid dielectric.

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[50] Miller (1973, p. 222) lists the following discussions of this paper in the secondary literature: Hirosige 1966, pp. 24–27; Schaffner 1969, pp. 505–506; McCormmach 1970b, pp. 473–474. For (brief) more recent discussions of this paper see Pais 1982, pp. 125–126, Zahar 1989, pp. 69–70, and Darrigol 1994a, pp. 294–295. Zahar missed the crucial last section of the paper; Darrigol did take notice of it, but failed to appreciate its importance.

The publication history of Lorentz's 1899 paper is rather complicated. It was first published in the Dutch version of the Proceedings of the Amsterdam Academy (Lorentz 1899a). For the translation in the English version of the Proceedings (Lorentz 1899b), Lorentz made some changes in the presentation of his results, especially in the crucial final section 9 containing what we now recognize as the exact Lorentz transformation up to an undetermined factor. A French translation was published as Lorentz 1902. This translation follows the Dutch original rather than the emended English translation. There are some minor differences between the French and the Dutch versions as well: section 6 in the Dutch version is broken up into two sections for the French version, so the last section is now section 10. Moreover, the term 'ion' Lorentz still used in 1899 is replaced by the new term 'electron.' I will follow the English version, but I will also quote (in translation) from the Dutch original at places where the English deviates from the Dutch in an interesting way.

> It is impossible to say with certainty what would be observed in such a case, for, if the explication of Michelson's result which I have proposed is accepted, we must also assume that the mutual distances of the molecules of the transparent media are altered by the translation.
>
> Besides, we must keep in view the possibility of an influence, be it of the second order, of the translation on the molecular forces.
>
> In what follows I shall sh[o]w, not that the result of the experiment must necessarily be negative, but that this might very well be the case. At the same time it will appear what would be the theoretical meaning of such a result. (Lorentz 1899b, p. 268)

In the remaining three and a half pages of the article, Lorentz proceeds to develop the essential structure of his 1904 theory which predicts a negative result for the experiment proposed by Liénard as well as for any other optical experiment, no matter how accurate, that eventually boils down to the observation of some pattern of light and darkness.

The basis of this theory is an exact version of the theorem of corresponding states. Lorentz introduced new auxiliary quantities to give the equations for the electromagnetic field in a moving Galilean frame $S$ (see Eq. 3.3) the form of Maxwell's equations, not just in the source free case (as in the *Versuch*) but in two important special cases with sources present as well. The first case is that of a static charge distribution at rest in $S$; the second that of bound electrons whose oscillations around some fixed point of $S$ are responsible in Lorentz's theory for the emission of light from a source at rest in $S$.

The new auxiliary quantities of the last section of Lorentz's 1899 paper are a slight modification of the auxiliary quantities that are used in the rest of the paper. These, in turn, are essentially a combination of the auxiliary quantities used separately in the *Versuch,* viz. the local time from the first order theorem of corresponding states in optics (see Eq. 3.14) and the stretched out $x$-coordinate from the calculations in electrostatics (see Eq. 3.47). The auxiliary quantities in the final section of the 1899 paper, which are also the ones used in Lorentz 1904b, are defined as[51]

$$\mathbf{x}' \equiv \mathrm{diag}(l\gamma,\, l,\, l)\,\mathbf{x}, \qquad t' \equiv l\left(t/\gamma - \gamma(v/c^2)\,x\right), \tag{3.54}$$

$$\begin{aligned} \mathbf{E}' &\equiv \mathrm{diag}(1/l^2,\, \gamma/l^2,\, \gamma/l^2)\left(\mathbf{E} + \mathbf{v}\times\mathbf{B}\right), \\ \mathbf{B}' &\equiv \mathrm{diag}(1/l^2,\, \gamma/l^2,\, \gamma/l^2)\left(\mathbf{B} - \frac{1}{c^2}\,\mathbf{v}\times\mathbf{E}\right). \end{aligned} \tag{3.55}$$

The quantity $l$ is an undetermined factor that is only allowed to differ from 1 by a term in the order of $v^2/c^2$ (Lorentz 1899b, p. 269; 1904, p. 176).[52]

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[51] The primed quantities in Eq. 3.54–3.55 are the double primed quantities of Eqs. (6), (8), and (9) in Lorentz 1899b, p. 269 (I denote as $\gamma$ what Lorentz denotes as $k$); they are also the primed quantities of Eqs. (4)–(6) in Lorentz 1904b, pp. 175–176.

[52] This is the notation of Lorentz 1904b (pp. 175–176). In 1899, this factor is written as $1/\varepsilon$. In 1904, as we will see in section 3.4, Lorentz concluded that $l = 1$, albeit in a rather roundabout manner. In the English

The fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ understood as functions of $(\mathbf{x}', t')$ satisfy equations very similar to Maxwell's equations without neglecting terms of any order of magnitude (cf. Lorentz 1899b, p. 269, Eqs. (Ie)–(IVe);[53] Lorentz 1904b, p. 176, Eqs. 7–9)[54]

---

version of his 1899 paper, he wrote: "I see, however, no means to determine it" (Lorentz 1899b, p. 270). In the original Dutch version, he was not quite as pessimistic: "ε [or $l$] will have some determinate value, which it will only be possible to ascertain through a further insight in the phenomena" (Lorentz 1899a, p. 522).

[53] In the last section of his 1899 paper, Lorentz wrote down these equations for the special case where the source terms describe the oscillating electrons in a moving light source. I will discuss this special case in a somewhat simplified form at the end of this section.

[54] As with the first order theorem of corresponding states (see section 3.1, Eq. 3.6), I will derive the equation for $\mathrm{div}'\mathbf{E}'$ in Eq. 3.56. Similar arguments can be given for the other three equations, starting from (the components of) $\mathrm{curl}'\mathbf{E}' - \partial\mathbf{B}'/\partial t'$, $\mathrm{div}'\mathbf{B}'$, and $\mathrm{curl}'\mathbf{B}' - (1/c^2)\,\partial\mathbf{E}'/\partial t'$, respectively. I will write $\mathrm{div}'\mathbf{E}'$ in terms of the unprimed quantities, and then use Eq. 3.3 to show that this expression is equal to $\rho/(\varepsilon_0\gamma\, l^3)\,(1 - \gamma^2 v\, u_x/c^2)$, as Eq. 3.56 says it is.

The inverse transformation of Eq. 3.54 is

$$\mathbf{x} = \mathrm{diag}\left(\frac{1}{l\gamma}, \frac{1}{l}, \frac{1}{l}\right)\mathbf{x}', \qquad t = \frac{1}{l}\,\gamma\,\left(t' + (v/c^2)\,x'\right),$$

which means that the derivatives with respect to the primed variables are related to the derivatives with respect to the unprimed variables as

$$\left(\frac{\partial}{\partial t'}, \frac{\partial}{\partial x'}, \frac{\partial}{\partial y'}, \frac{\partial}{\partial z'}\right) = \frac{1}{l}\left(\frac{\partial}{\partial t}, \frac{1}{\gamma}\frac{\partial}{\partial x} + \gamma\,(v/c^2)\frac{\partial}{\partial t}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right).$$

Inserting these relations along with Eq. 3.55 for $\mathbf{E}'$, we find

$$\mathrm{div}'\mathbf{E}' = \frac{1}{l^3}\left(\frac{1}{\gamma}\frac{\partial E_x}{\partial x} + \gamma\,(v/c^2)\frac{\partial E_x}{\partial t} + \gamma\left(\frac{\partial E_y}{\partial y} - v\frac{\partial B_z}{\partial y}\right) + \gamma\left(\frac{\partial E_z}{\partial z} + v\frac{\partial B_y}{\partial z}\right)\right).$$

Substituting $\gamma\,(1 - v^2/c^2)$ for $1/\gamma$ in the first term on the right hand side and regrouping terms, we can write this equation as

$$\mathrm{div}'\mathbf{E}' = \frac{1}{l^3}\left(\gamma\left(\frac{\partial E_x}{\partial x} + \frac{\partial E_y}{\partial y} + \frac{\partial E_z}{\partial z}\right) - v\,\gamma\left(\frac{\partial B_z}{\partial y} - \frac{\partial B_y}{\partial z} - \frac{1}{c^2}\frac{\partial E_x}{\partial t} + \frac{v}{c^2}\frac{\partial E_x}{\partial x}\right)\right).$$

The first three terms in parentheses are just div $\mathbf{E}$. The last four terms in parentheses are just the $x$-component of curl $\mathbf{B} - (1/c^2)\,(\partial\mathbf{E}/\partial t - v\,\partial\mathbf{E}/\partial x)$. Using Eq. 3.3 for these expressions and using the relation $\mu_0 = 1/(\varepsilon_0 c^2)$, we arrive at

$$\begin{aligned}
\mathrm{div}'\mathbf{E}' &= \frac{1}{l^3}\left(\gamma\,\rho/\varepsilon_0 - \gamma\,\frac{v\rho}{\varepsilon_0 c^2}\,(u_x + v)\right) \\
&= \frac{\gamma\,\rho}{l^3\,\varepsilon_0}\left(1 - \frac{v\,u_x}{c^2} - \frac{v^2}{c^2}\right) \\
&= \frac{\gamma\,\rho}{l^3\,\varepsilon_0}\left(\frac{1}{\gamma^2} - \frac{v\,u_x}{c^2}\right) \\
&= \frac{\rho}{\gamma\,l^3\,\varepsilon_0}\left(1 - \gamma^2\,\frac{v\,u_x}{c^2}\right).
\end{aligned}$$

$$\begin{aligned} &\mathrm{div}'\,\mathbf{E}' = \frac{\rho}{\gamma l^3 \varepsilon_0}\left(1-\gamma^2\frac{v\,u_x}{c^2}\right), && \mathrm{curl}'\,\mathbf{E}' = -\frac{\partial \mathbf{B}'}{\partial t'}, \\ &\mathrm{div}'\,\mathbf{B}' = 0\,, && \mathrm{curl}'\,\mathbf{B}' = \mu_0\rho\,\mathrm{diag}(\gamma/l^3,\,1/l^3,\,1/l^3)\,\mathbf{u} + \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}. \end{aligned} \tag{3.56}$$

Combining the Galilean transformation $(\mathbf{x}_0, t_0) \to (\mathbf{x}, t)$ in Eq. 3.1 with the transformation $(\mathbf{x}, t) \to (\mathbf{x}', t')$ in Eq. 3.54, we find:

$$\begin{aligned} x' &= l\,\gamma\,(x_0 - v t_0), & t' &= l\left(t_0/\gamma - \gamma(v/c^2)\,(x_0 - v\,t_0)\right) \\ y' &= l\,y_0, & &= l\left(t_0\left(\frac{1}{\gamma}+\gamma\frac{v^2}{c^2}\right) - \gamma(v/c^2)\,x_0\right) \\ z' &= l\,z_0, & &= l\,\gamma\left(t_0 - (v/c^2)\,x_0\right). \end{aligned} \tag{3.57}$$

If we set $l = 1$, Eq. 3.57 just gives the Lorentz transformation $(\mathbf{x}_0, t_0) \to (\mathbf{x}', t')$ as we know it today. So, from a modern perspective, the derivation of Eq. 3.56 is simply (part of) a proof that Maxwell's equations are invariant under Lorentz transformation. From such a modern perspective, the primed quantities $\mathbf{x}'$, $t'$, $\mathbf{E}'$, and $\mathbf{B}'$ all belong to the Lorentz frame $S'$ moving at a velocity $\mathbf{v}$ with respect to the frame $S_0$, just as the unprimed quantities belong to the Galilean frame $S$ moving at a velocity $\mathbf{v}$ with respect to the frame $S_0$. The four-vector $j'^{\mu} \equiv (\rho' c,\ \rho'\mathbf{u}')$, representing the charge and current density in $S'$, is related to the corresponding vector $j_0^{\mu}$ in $S_0$ via

$$\begin{aligned} \rho' c &= \gamma\left(\rho c - \frac{v}{c}\rho u_{0_x}\right) = \gamma\rho c\left(1 - \frac{v\,u_x}{c^2} - \frac{v^2}{c^2}\right) = \frac{\rho c}{\gamma}\left(1 - \gamma^2\frac{v\,u_x}{c^2}\right), \\ \rho' u'_x &= \gamma\left(\rho u_{0_x} - \frac{v}{c}\rho c\right) = \gamma\rho u_x, \\ \rho' u'_y &= \rho u_{0_y} = \rho u_y, \\ \rho' u'_z &= \rho u_{0_z} = \rho u_z, \end{aligned} \tag{3.58}$$

in accordance with Eq. 3.56 for $l = 1$.

---

This is indeed the equation for $\mathrm{div}'\mathbf{E}'$ in Eq. 3.56. The other equations in Eq. 3.56 can be found in the same way.

**3.3.2 A simplified treatment of electrostatics in moving frames.** Before I turn to optics, the focus of Lorentz's considerations in the last section of his 1899 paper, I want show that Eqs. 3.54–3.56 make it possible to give a very simple treatment of electrostatic problems in the moving frame $S$. This same treatment is already possible with the help of the auxiliary quantities introduced earlier in the 1899 paper,[55] and can actually be found in section 5 of the paper (Lorentz 1899b, pp. 259–261).

In the case of electrostatics in the moving frame $S$, $\mathbf{u} = 0$, and $\mathbf{E}$, $\mathbf{B}$, and $\rho$ are functions of $\mathbf{x}$ only and not of the (real) time $t$. From Eq. 3.55, it follows that in that case $\mathbf{E}'$, $\mathbf{B}'$, and $\rho$ will be functions of $\mathbf{x}'$ only and not of the local time $t'$. So, Eq. 3.56 reduces to:

$$\begin{aligned} &\operatorname{div}' \mathbf{E}' = \frac{\rho}{\gamma l^3 \varepsilon_0}, \qquad && \operatorname{curl}' \mathbf{E}' = 0, \\ &\operatorname{div}' \mathbf{B}' = 0, && \operatorname{curl}' \mathbf{B}' = 0. \end{aligned} \tag{3.59}$$

These equations have precisely the form of the equations for electrostatics in the frame $S_0$ at rest in the ether for a charge distribution $\rho/\gamma l^3$. The equations are easy to solve. First, we can set $\mathbf{B}' = 0$. To find the solution for $\mathbf{E}'$, we introduce the potential $\omega'$ and the auxiliary charge distribution $\rho'$, defined through:

$$\mathbf{E}' \equiv -\operatorname{grad} \omega', \qquad \rho' \equiv \frac{\rho}{\gamma l^3}. \tag{3.60}$$

The charge distribution $\rho'$ is basically the same stretched out charge distribution Lorentz considered in the *Versuch* (see section 3.2). The only difference is that this charge distribution is not only stretched out by a factor $\gamma$ in the $x$-direction, but also by a factor $l$ in all three directions.

With $\mathbf{E}' = -\operatorname{grad} \omega'$, the equation $\operatorname{curl}' \mathbf{E}' = 0$ is automatically satisfied. Inserting Eq. 3.60 into the equation for $\operatorname{div} \mathbf{E}'$ in Eq. 3.59, we find

$$\Delta' \omega' = -\rho'/\varepsilon_0, \tag{3.61}$$

which is just the Poisson equation.

With the help of Eq. 3.55 for $\mathbf{E}'$, the Lorentz force on a charge $q$ at a fixed point of $S$ in the static charge distribution at rest in $S$ can be written as

[55] Except for the addition of the extra factor $\varepsilon$, only the definitions of $t'$ and $\mathbf{B}'$ are changed in the final section, and neither $t'$ nor $\mathbf{B}'$ play a role in electrostatics

$$\mathbf{F} = q(\mathbf{E} + \mathbf{v} \times \mathbf{B}) = \mathrm{diag}(l^2,\ l^2/\gamma,\ l^2/\gamma)\ q\mathbf{E}' \tag{3.62}$$

The quantity $q\mathbf{E}'$ in this equation is just the force $\mathbf{F}'$ the charge $q$ would experience at the corresponding point in the stretched out charge distribution $\rho'$ at rest in $S_0$. In other words,

$$\mathbf{F} = \mathrm{diag}(l^2,\ l^2/\gamma,\ l^2/\gamma)\ \mathbf{F}'. \tag{3.63}$$

For $l = 1$, Eq. 3.63 reduces to Eq. 3.53, which formed the basis for Lorentz's plausibility argument for the Lorentz-FitzGerald contraction hypothesis.[56] Notice the tremendous simplification of the derivation of this result achieved in 1899 as compared to the tedious derivation in the *Versuch* (see Eqs. 3.32–3.53 in section 3.2).[57]

**3.3.3 How the Lorentz invariance of the source free Maxwell equations, the hypothesis that corresponding states physically transform into one another, and the general nature of patterns of light and darkness can account for the negative result of almost any conceivable optical ether drift experiment.** I now turn to optics. I will dwell on this subject at somewhat greater length than Lorentz himself and I will draw attention to some logical features of his theory he never explicitly mentioned, neither in 1899 nor in 1904. The purpose of my detailed discussion is to elucidate the basic structure of the theory which has been widely misunderstood in the secondary literature (as I will illustrate with some quotations below). In particular, I want to clarify the relation between two fundamental elements of the theory, the purely mathematical theorem of corresponding states and the following physical assumption, for which I want to coin the phrase *generalized contraction hypothesis*:

> If a material system, i.e., a configuration of particles, with a charge distribution that generates a particular electromagnetic field configuration in $S_0$, a frame at rest in the ether, is given the velocity $\mathbf{v}$ of a Galilean frame $S$ in uniform motion through to the ether, it will rearrange itself so as to produce the configuration of particles with a charge distribution that generates the electromagnetic field

---

[56] The undetermined factor $l$ is essentially the same as the undetermined factor $f(\beta)$ in the explanation of the Michelson-Morley experiment on the basis of the contraction hypothesis (see section 3.2). Hence, the following remark by Miller, though true in the context of the 1895 version of Lorentz's theory, is no longer true in the context of the 1899 version of the theory. Commenting on Lorentz's 1892–1895 plausibility argument for the contraction hypothesis, Miller writes: "Lorentz [...] emphasizes [Lorentz et al. 1952, p.5] that $\varepsilon = 0$ [i.e., $f(\beta) = 1$] may not be the correct choice; however, it is the only one which he can argue for on the basis of electromagnetic theory" (Miller 1974, p. 39).

[57] Darrigol failed to appreciate this when he wrote: "the benefit of the new transformation was meager and the technique of corresponding states worked only for the cases that Lorentz had already been able to treat, electrostatics and first order optics" (Darrigol 1994a, p. 295). The last part of this statement is somewhat puzzling. As we will see below, and as Lorentz emphasizes in his paper (Lorentz 1899b, pp. 267–268), the point of the new transformation (i.e., Eqs. 3.54–3.55) is to be able to deal with *second* order optics.

> configuration in $S$ that is the corresponding state of the original electromagnetic field configuration in $S_0$.[58]

If we use the term 'corresponding states' to encompass both the electromagnetic field configuration and the configuration of the material system generating it, the generalized contraction hypothesis can be stated more concisely as: *corresponding states physically transform into one another*.[59] This assumption is independent of the theorem of corresponding states.[60] As we saw in the discussion of the Trouton-Noble experiment in chapter one, the theorem of corresponding states can be used with and without the contraction hypothesis. The following discussion of optics in the 1899–1904 version of Lorentz's theory was, in fact, inspired in part by Larmor's ingenious, if not unproblematic, discussion of the Trouton-Noble experiment (see sections 1.2 and 1.3).

In optics, we can set $\rho = \mathbf{u} = 0$. Eq. 3.56 then reduces to:

$$\begin{aligned} \operatorname{div}' \mathbf{E}' &= 0, \qquad & \operatorname{curl}' \mathbf{E}' &= -\frac{\partial \mathbf{B}'}{\partial t'}, \\ \operatorname{div}' \mathbf{B}' &= 0, \qquad & \operatorname{curl}' \mathbf{B}' &= \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}. \end{aligned} \tag{3.64}$$

These equations have the exact same form as the source free Maxwell equations. So, we can formulate an exact theorem of corresponding states (cf. section 3.1):

> If there is a solution of the source free Maxwell equations in which the real fields $\mathbf{E}$ and $\mathbf{B}$ are certain functions of $\mathbf{x}_0$ and $t_0$, the coordinates of $S_0$ and the real Newtonian time, then there is another solution of the source free Maxwell equations in which the fictitious fields $\mathbf{E}'$ and $\mathbf{B}'$ are those exact same functions of $\mathbf{x}'$ and $t'$, the coordinates of $S$ and the local time in $S$.

---

[58]The idea behind this hypothesis is similar to the idea behind Lorentz's plausibility argument for the Lorentz-FitzGerald contraction in 1892 and 1895 that I discussed in section 3.2. However, as we will see, Lorentz no longer invoked the dubious assumption of static equilibrium in this context (cf. Lorentz 1904b, p. 183, p. 191).

[59] Another advantage of this more concise formulation is that it leaves open the precise nature of the material system. In the historical context, the material system will always be a collection of classical particles interacting according to some Newtonian force law, but, in a modern setting, as has been urged by John Norton, one might want to consider a situation in which the material part of the system is described by, say, a Dirac spinor field. The generalized contraction hypothesis then says that the configuration of the Dirac field in the system at rest in the ether will turn into the Lorentz boosted version of that configuration upon setting the system in motion.

[60] It is not surprising that this point has been misunderstood by many modern authors. If all physical laws, not just the laws governing the electromagnetic field, are Lorentz invariant, the generalized contraction hypothesis is a direct consequence of the Lorentz invariance.

We can also formulate an exact version of the general result in the *Versuch* concerning patterns of light and darkness in corresponding states:[61]

> If there is a field configuration in $S_0$ which is a solution of the source free Maxwell equations and which describes a certain pattern of light and darkness, then its corresponding state in $S$ describes a pattern of light and darkness that differs from the pattern in $S_0$ only in that it is contracted by a factor $\gamma l$ in the $x$-direction and by factor $l$ in the $y$- and $z$-directions.

The patterns of light and darkness in two corresponding states in optics are thus related to each other in the same way the charge distributions in two corresponding states in electrostatics are related to each other. The strategy for finding the force on a charge in a moving static charge distribution, which was introduced in the *Versuch* and which I used in the discussion of the Trouton-Noble experiment in sections 1.2 and 1.3, therefore suggests a similar strategy for finding the pattern of light and darkness in optical experiments, such as the Michelson-Morley experiments and variations on it such as the Kennedy-Thorndike experiment and the experiment proposed by Liénard. As in the case of electrostatics, we need to distinguish between the case without and the case with a physical contraction of the systems under consideration.

I consider the case without a physical contraction first. Consider Fig. 3.6 (cf. Fig. 1.8 for the Trouton-Noble experiment in section 1.2). On the right, it shows a moving *un*contracted Michelson interferometer. The arm with mirror 2 makes an angle $\theta$ with the $x$-axis of the co-moving frame Galilean frame $S$, also shown in the figure.

The task before is to compute whether it will be light or dark at the point $P$ at the center of the screen as a result of the interference of light coming from mirrors 1 and 2. Assuming that the mirrors and the beam splitter reflect and refract the light in such a way that it travels along the arms of the interferometer (a dubious assumption as we will see, but one that is routinely made in discussions of the Michelson-Morley experiment), the result will depend solely on the value of the phase difference

$$\alpha = \frac{\tau_1 - \tau_2}{T} \tag{3.65}$$

between the light waves coming from the two mirrors, where $\tau_1$ ($\tau_2$) is the time it takes light to travel from the beam splitter to mirror 1 (mirror 2) and back, and where $T$ is the period of light emitted by the source (for the sake of simplicity I will assume that this light is monochromatic).

---

[61] Recall that in 1895 we had $\mathbf{x}' \equiv \mathbf{x}$ (see Eq. 3.4), whereas in 1899–1904 we have $\mathbf{x}' \equiv \mathrm{diag}(l\gamma, l, l)\,\mathbf{x}$ (see Eq. 3.54). So, if it is light (dark) in a field configuration in $S_0$ at $\mathbf{x}_0 = \mathbf{a}$, it will be light (dark) in the corresponding state in $S$ at $\mathbf{x}' = \mathbf{a}$, i.e., at $\mathbf{x} = \mathrm{diag}(1/l\gamma, 1/l, 1/l)\,\mathbf{a}$.

If $\alpha$ has a half integer value, it will be totally dark at $P$; if $\alpha$ has any other value, there will be light at $P$, the light being brightest if $\alpha$ has an integer value.

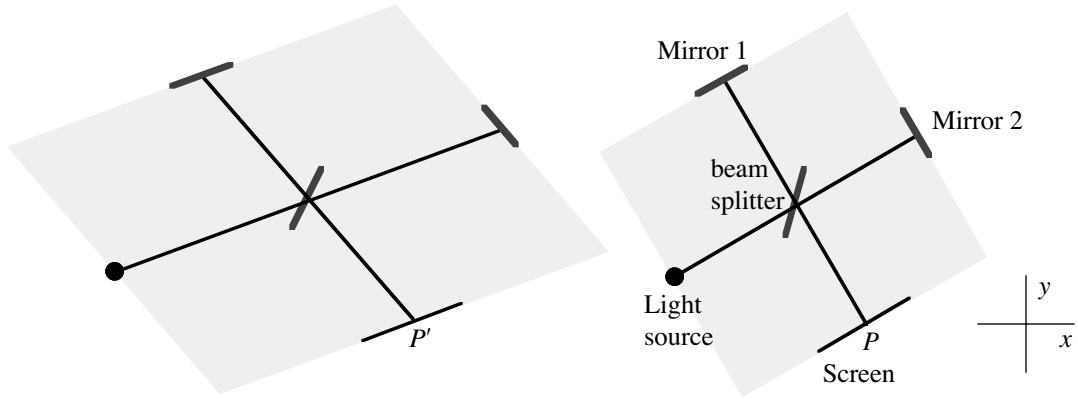


Figure 3.6: Moving interferometer and its corresponding state (without the Lorentz-FitzGerald contraction).

The field configuration in the interferometer moving along with the frame S will correspond to a stretched out field configuration in $S_0$. The stretched out version of the moving interferometer is shown on the left in Fig. 3.6. If it is light at $P$, it will be light at $P'$. If it is dark at $P$, it will be dark at $P'$. Hence, the phase difference $\alpha$ must be equal to the corresponding phase difference

$$\alpha' = \frac{\tau'_1 - \tau'_2}{T'} \tag{3.66}$$

in the stretched out interferometer at rest in the ether, where $\tau'_1$, $\tau'_2$, and $T'$ are the analogues of $\tau_1, \tau_2$, and $T$ in the case of the moving interferometer.[62] The period $T'$ will *not* be equal to the period $T$ of the light used in the moving frame. It will be equal to the period of that light in *local* time, not in *real* time. Since, at a fixed point in $S$, the local time differs by a factor $l/\gamma$ from the real Newtonian time, it follows that

$$T' = (l/\gamma)\, T = l\, T \sqrt{1 - \beta^2}\,. \tag{3.67}$$

[62] This is a slight simplification of matters. It may seem that we can only conclude that $\alpha = \alpha'$, if $\alpha$ is a half integer, so that it is totally dark at $P$ and $P'$. I have never bothered to find a rigorous derivation for this claim, but it is easy to see that the equality must hold in general. Imagine that we slowly move one of the mirrors in the moving interferometer parallel to itself, thereby changing the length of one of the arms. As a consequence, $\alpha$ and $\alpha'$ will change continuously. Every time $\alpha$ takes on a half integer value, $\alpha'$ must also take on a half integer value. That means that $\alpha$ and $\alpha'$ can only differ by some integer value. Consideration of the full interference pattern on the screen (especially of the number of rings) shows that this integer value can only be zero.

Since the stretched out interferometer is at rest in the ether, $\tau'_1 - \tau'_2$ is simply given by

$$\tau'_1 - \tau'_2 = \frac{2(L'_1 - L'_2)}{c}, \tag{3.68}$$

where $L'_1$ and $L'_2$ are the lengths of the arms of the stretched out interferometer. Using Eq. 1.11 from section 1.2, multiplied by an overall factor $l$, for the relation between $(L'_1, L'_2)$ and $(L_1, L_2)$, we find (cf. Eq. 3.25):

$$L'_1 - L'_2 = lL_1 \sqrt{\frac{1-\beta^2\sin^2(\theta+\pi/2)}{1-\beta^2}} - lL_2 \sqrt{\frac{1-\beta^2\sin^2\theta}{1-\beta^2}}\,. \tag{3.69}$$

With the help of Eqs. 3.66–3.69, the phase difference $\alpha = \alpha'$ can be written as

$$\alpha = \frac{2L_1}{cT}\frac{\sqrt{1-\beta^2\cos^2\theta}}{1-\beta^2} - \frac{2L_2}{cT}\frac{\sqrt{1-\beta^2\sin^2\theta}}{1-\beta^2} \tag{3.70}$$

This is precisely the expression for $\alpha$ that follows from Eq. 3.24, the result of my examination of the Michelson-Morley experiment in section 3.2. The conclusion is that Lorentz's exact theorem of corresponding states without assuming a physical contraction predicts a positive result for the Michelson-Morley experiment.

As I already indicated, this new derivation of the prediction for the experiment does make it clear that the standard treatment of the experiment involves some dubious tacit assumptions. Looking at the stretched out interferometer in Fig. 3.6, one clearly sees that the stretched out mirrors, even though they are at rest in the ether, do not reflect light according to the standard law of reflection from geometrical optics if the light waves are to travel along the arms of the interferometer as was assumed in the derivations of both Eq. 3.70 above and Eq. 3.24 in section 3.2.[63] Presumably, this will not affect the result to order $\beta^2$, but I, for one, have not been able to prove this.[64]

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[63] Hicks (1902) would actually criticize the standard analysis of the Michelson-Morley experiment on the grounds that it does not give a satisfactory treatment of the reflection of light by moving mirrors (see Swenson 1972, p. 142).

[64] Several years ago, I tried to derive, together with Xaveer Leijtens, an expression, valid to order $\beta^2$, for the phase difference $\alpha$ in the case of an uncontracted interferometer, using Lorentz's exact theorem of corresponding states *without* making the assumption that the light waves travel along the arms of the interferometer. First, we constructed the images of a pointlike light source in the stretched out interferometer under the various reflections occurring in the interferometer. In this way, we reduced the problem of the interference of light coming from the two mirrors to the problem of the interference of the light coming from two virtual pointlike light sources. Finally, we calculated the phase difference at the center of the interference pattern for this situation. The result of our long, tedious, and error-prone calculation deviated significantly from the second order approximation of Eq. 3.70. Despite this result, I tend to believe that Eq. 3.70 is correct to order $\beta^2$, my main reason being that a

For my purposes here, the exact expression for the phase difference expected in the Michelson-Morley experiment if there were no physical contraction is not terribly important. In fact, a purely qualitative argument would do. Without the contraction hypothesis, the corresponding state of the moving interferometer will change its shape as the moving interferometer is rotated. This means that whether it is dark or light at $P'$ (and thereby at $P$) will depend on the orientation of the moving interferometer. Without the contraction hypothesis, Lorentz's theory therefore predicts a positive result in the Michelson-Morley experiment (cf. my reconstruction in section 1.3 of the argument Larmor (1902) offered for the Trouton-Noble experiment).

I now turn to the case with a physical contraction. This case is illustrated in Fig. 3.7, which is fully analogous to Fig. 3.6, the only difference being that the moving interferometer has now undergone the Lorentz-FitzGerald contraction (cf. Fig. 1.7 for the Trouton-Noble experiment in section 1.2).

The corresponding state of the moving contracted interferometer is simply the uncontracted interferometer at rest in the ether. So, the shape of the corresponding state will not depend on the orientation of the moving interferometer with respect to its velocity. As a consequence, we now expect negative results, both in the Michelson-Morley experiment and in the variation on it suggested by Liénard.

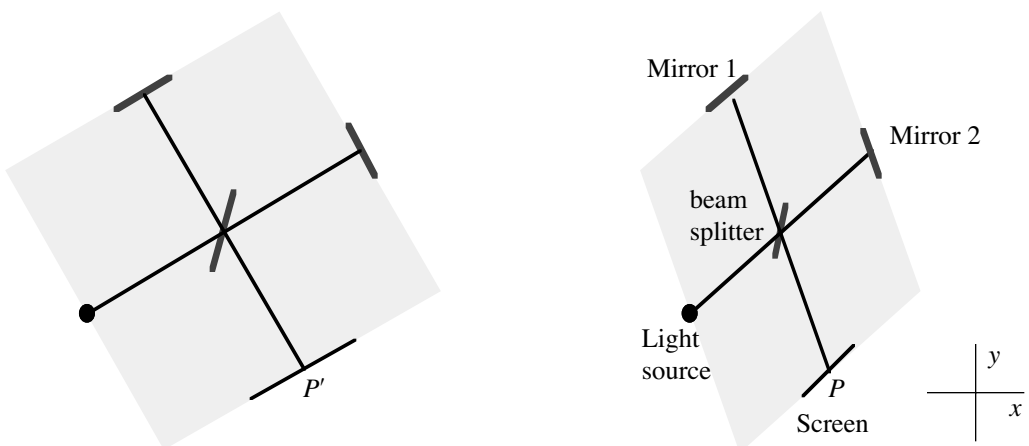


Figure 3.7: Moving interferometer and its corresponding state (with the Lorentz-FitzGerald contraction).

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similar simplification in the case of the Trouton-Noble experiment (i.e., treating the stretched out condenser as if it has the shape of a rectangle rather than a parallelogram) does not affect the expression for the turning couple of the Coulomb forces exerted on the condenser the plates to order $\beta^2$ (see section 1.2). However, even for $\theta = 0$ and $\theta = \pi/2$, when the stretched out interferometer has the shape of a rectangle, the problem of reflection still comes into play for reflections at the beam splitter, whereas the corresponding problem in the case of the stretched out condenser disappears for those two special values of $\theta$.

In this case, the phase difference $\alpha$, determining whether it is light or dark at the center $P$ of the screen in the moving interferometer, is exactly equal to the phase difference

$$\alpha' = \frac{2\left(L'_1 - L'_2\right)}{cT'} \tag{3.71}$$

we would have if the interferometer were at rest in the ether. Hence, we also expect a negative result in the Kennedy-Thorndike experiment.

Notice that the explanation of the negative result of this experiment involves a surprising new element in Lorentz's exact theory. The frequency of the light emitted by the light source in the interferometer depends on the source's velocity with respect to the ether. The frequency $\nu$ it emits while moving at a velocity $\mathbf{v}$ differs by a factor $l/\gamma$ from the frequency $\nu'$ it would emit if it were at rest in the ether.[65] This immediately shows that the generalized contraction hypothesis (which in this case tells us that the interferometer on the left of Fig. 3.7, both its material parts and its electromagnetic field configuration, turns into the interferometer on the right when it is set in motion through the ether with a velocity $\mathbf{v}$) entails more than just a change of dimensions, which was all the original contraction hypothesis told us. It entails many other effects of motion through the ether on physical phenomena, not only the effect just mentioned concerning the frequency of the light emitted by a moving source, but also, for instance, an effect on the reflection of light by moving mirrors. This last effect can be seen upon inspection of Fig. 3.7. The reflection (and refraction, I may add) is such that in the corresponding state light is reflected (and refracted) according to the standard laws of geometrical optics. That means that these laws do not hold in the Galilean frame $S$ moving with the interferometer (cf. my brief discussion of the Fresnel dragging coefficient in section 3.1). I will show below that the generalized contraction hypothesis likewise affects the phenomena of Doppler effect and aberration.

Because of its definite predictions about the effect of motion through the ether on all sorts of phenomena rather than just on the dimensions of moving bodies, the generalized contraction hypothesis does not face the problem of limited testability the original contraction hypothesis was facing (see section 3.2). There is no shortage of empirical tests to which the new hypothesis can be subjected. The new contraction hypothesis therefore passes with flying colors the *philosophical* test of falsifiability, a test on which the old contraction hypothesis earned no more than a marginal pass (thanks mainly to the Kennedy-Thorndike experiment). In whatever sense the new hypothesis may still be *ad hoc*, there can be no doubt whatsoever that it is not *ad hoc* in the usual falsificationist sense of that term. And this is true not just of the

[65] From $T' = (l/\gamma)\, T$ (see Eq. 3.67) it follows that $\nu = (l/\gamma)\, \nu'$.

generalized contraction hypothesis as such, but also for the more specific assumptions entailed by the hypothesis considered separately. This is a direct consequence of the fact that these assumptions are not introduced to account for isolated experiments but for the general experimental indication that ether drift can not be detected, at least not by optical means.

Unless Einstein was deeply mistaken about the logical status of the contraction hypothesis in Lorentz's mature theory—and what complicates matters is that, at least to some extent, he appears to have been—his denouncement of the contraction hypothesis as *ad hoc* must be related to some other feature of Lorentz's theory. The feature that immediately comes to mind is the fact that the generalized contraction hypothesis and the specific assumptions it entails are all aimed at explaining why a central element in Lorentz's ontology, the stationary ether, is completely invisible to us. As I will argue in chapter four, this is a very unsatisfactory situation indeed. However, what is unsatisfactory about it can not be cast in falsificationist terms. In terms of testability, it does not make any difference whether a theory predicts a positive or a negative result. What counts is that it predicts a definite result. This, I take it, is Lorentz's point, when he wrote to Einstein in 1915, in a passage I already quoted at the end of section 3.2: "whether it is the explanation of a negative or a positive result that is at issue, hardly makes a difference, it seems to me."

**3.3.4 The status of the generalized contraction hypothesis for Lorentz.** After this lengthy discussion of the logical structure of Lorentz's theory, I want to confront my analysis with some passages from the actual text of Lorentz's 1899 paper. Lorentz's own statement of what I called the generalized contraction hypothesis is as follows:

> We shall not only suppose that the system $S_0$ *may* be changed in this way into an imaginary system $S$,[66] but that, as soon as the translation is given to it, the transformation *really* takes place, of itself, i.e. by the action of the forces acting between the particles of the system, and the aether. Thus, after all, $S$ will be the *same* material system as $S$ [this clearly should be $S_0$].
>
> The transformation of which I have spoken, is precisely such a one as is required in my explication of Michelson's experiment. (Lorentz 1899b, p. 270; italics in the original)

In his brief discussion of this paper, Pais quotes this last sentence and comments: "Thus the reduction of the FitzGerald-Lorentz contraction to a consequence of Lorentz transformations is a product of the nineteenth century" (Pais 1982, pp. 125–126). What can be derived from the invariance of the source free Maxwell equations is, at best, the contraction of electromagnetic field configurations, such as the interference pattern in a moving Michelson interferometer. The

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[66] By the 'system $S_0$' and the 'imaginary system $S$' Lorentz means the corresponding configurations of electromagnetic field plus particles and charges generating these fields in, what I called, the *frames* $S_0$ and $S$. The use of the word 'imaginary' is confusing in this context. I assume it simply refers to the fact that the state in $S$ is most easily described in terms of the fictitious—'imaginary'—primed quantities.

contraction of the material part of the interferometer still has to be added as an extra assumption, unless we already know that all physical laws and not just Maxwell's equations are Lorentz invariant. It is a typical example of doing Whig history to attribute this insight to Lorentz in 1899.

Of course, one has to keep in mind that Pais was writing an Einstein biography, not a monograph on the development of Lorentz's theorem of corresponding states. However, I have not been able to find a clear and correct statement of the relation between the exact theorem of corresponding states and the generalized contraction hypothesis in the extensive secondary literature focusing on this narrow subject either. The statements I was able to find range from cryptic (Hirosige 1966[67]) through vague and misleading (McCormmach 1970a, 1970b[68]) to plainly wrong (Schaffner 1969, 1970;[69] Miller 1980[70,71]). This does not apply to Pais (whose

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[67] Hirosige writes that the new auxiliary quantities in the last section of Lorentz's 1899 paper "were introduced in order to show that Lorentz contraction was not a conventional fiction but a necessary requisite for the correspondence of states" (Hirosige 1966, p. 26). He continues to make some more useful remarks: "the dimensional relation between $S_0$ and S just agrees with Lorentz contraction" (ibid.) and "Lorentz explains the absence of influence of the motion of the earth not by showing the covariancy of Maxwell's equations to higher orders. He [...] satisfied himself with confirming the necessity of the contraction" (ibid., p. 27). To the extent that I can make sense of these remarks, I think I agree with Hirosige.

[68] McCormmach, in an otherwise very lucid paper, does not do much better than Hirosige. He writes: "Lorentz made the contraction hypothesis an integral part of his theory in 1899 by generalizing his corresponding-states theorem [...] The contraction relations were no longer a special assumption, but a formal "transformation" associated with an extended corresponding-states theorem" (McCormmach 1970b, p. 473). These statements may be correct but they are too vague to be of any help to a reader looking for guidance in understanding Lorentz's reasoning. A few paragraphs later, however, McCormmach starts a sentence with: "The inclusion of contraction relations in the corresponding states theorem ..." (ibid.). This strongly suggests Pais's incorrect reading of this passage, and is therefore highly misleading. In another paper published in 1970, McCormmach is more explicit about his views on this matter. The following statement confirms what one would suspect on the basis of the quotations I just gave, namely that McCormmach misunderstands Lorentz's theory on this point: "From 1899 to 1904, Lorentz unified and simplified his theory by interpreting his dimensional contraction formula as a universal coordinate transformation" (McCormmach 1970a, p. 49).

[69] Schaffner simply blunders at this juncture. After quoting the passage from Lorentz 1899b, p. 269, with the definitions of the new auxiliary space and time coordinates (see Eq. 3.54), he writes: "These modified transformations allowed Lorentz to conclude that" (Schaffner 1969, p. 506). He then quotes the paragraph starting with the sentence "The transformation of which I have spoken, is precisely such a one as is required in my explication of Michelson's experiment" (the last sentence of the quotation I gave and the sentence that was also quoted by Pais). Schaffner leaves out the paragraph immediately preceding this sentence, thus suggesting that "the transformation of which I have spoken" refers to the definitions of the new auxiliary space and time coordinates rather than to the physical transformation of corresponding states into one another which unambiguously is what "the transformation of which I have spoken" actually refers to! This was not simply a slip on Schaffner's part. A year later he writes: "Equation (6) [i.e., in my notation, $x' = \gamma\, l x$] represents the transformation of length that captures the Lorentz-FitzGerald contraction" (Schaffner 1970 p. 335). In his critique of Zahar, he makes the following even more revealing remark. Talking about the experiments of Rayleigh and Brace and of Trouton and Noble, Schaffner writes: "Without further modifications, the Lorentz electron theory would have required positive results for these experiments; with further modifications in the transformation equations the theorem of corresponding states could be saved" (Schaffner 1974, p. 48). Later on in that same paper, he speculates on why Einstein "did not move in the direction of developing a theory which would have employed both the contraction hypothesis and the second order 'local time' transformation" (ibid., p. 65). Schaffner clearly conflates the mathematical theorem of corresponding states here with the physical assumption that corresponding states actually transform into one another.

native language is Dutch), but in fairness to the other authors it has to be said that they probably did/do not read Dutch so they did not have the benefit of comparing Lorentz's English text with the Dutch original. This may be a good point to remind readers in this same predicament that the French translation of this paper (Lorentz 1902) actually follows the Dutch rather than the English version.

The statement in the original Dutch version of the paper is much shorter and occurs in the middle of a dense paragraph, whereas the expanded statement in the English version is made into a separate paragraph. Personally, however, I feel that the Dutch original is much clearer:

> We shall assume that if an initially resting system $S_0$ is brought into translation, it will, *of itself*, go over into the system $S$. (Lorentz 1899a, p. 521[72]; italics in the original)

I want to draw attention to a subtle shift that can be discerned in comparing the original Dutch statement of the hypothesis and the reworked English statement. The specific hypotheses about the effect of motion with respect to the ether on particular physical phenomena that are entailed by the generalized contraction hypothesis can, of course, be looked upon in (at least) two different ways. They can be looked upon as consequences of the hypothesis which is itself taken to be one of the basic postulates of the theory (along with Maxwell's equations, the notion of a stationary ether, the notion of electrons etc.). They can also be looked upon as necessary conditions for the hypothesis which is itself understood as a claim that needs to be established on the basis of other premises. My presentation was clearly biased toward the former alternative. This fits with the original Dutch statement of the generalized contraction hypothesis, especially with the italicized 'of itself.' In the English version, however, Lorentz qualifies 'of itself,' making it clear that the generalized contraction effect, the physical transformation of corresponding states into one another, is not a primitive notion for him, but is

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[70] Miller makes the same mistake as Schaffner: "More than ten interlocking postulates enabled Lorentz to extend the 1895 theorem of corresponding states to apply to any order of accuracy" (Miller 1980, p. 73). Once again, the "interlocking hypotheses" are needed, not for the validity of the exact theorem of corresponding states, which is a purely mathematical theorem, but in order to guarantee that corresponding states actually transform into one another. This misinterpretation by Schaffner and Miller is a natural consequence of their misinterpretation of the auxiliary quantities occurring the theorem of corresponding states as postulates rather than stipulations (see section 3.1).

[71] A similar confusion can be found in a paper on Poincaré by Scribner. Talking about Lorentz's 1904 theory, he writes: "while [Lorentz] believed that the contraction of moving bodies in the direction of [...] motion (*as implied by the* [Lorentz] *transformation*) was a real physical effect, he viewed the local time coordinates as essentially a mathematical artifice" (Scribner 1964, p. 677; my italics). Earlier in his paper, Scribner (ibid., p. 674) clearly distinguishes between Lorentz and Poincaré's interpretation of local time in the context of the *Versuch* theory (see Davis 1994, p. 3). Yet, he still conflates mathematics and physics in Lorentz's 1904 theory

[72] The Dutch reads: "Wij zullen aannemen dat, indien een eerst rustend stelsel $S_0$ in translatie gebracht wordt, het *van zelf* in het stelsel $S$ overgaat." The Dutch 'stelsel' is ambiguous between 'physical system' and 'frame of reference.' In this context, however, only the former reading makes sense. Cf. the French translation: "Nous admettrons que, quand on imprime un mouvement de translation à un système $S_0$ primitivement en repos, ce système passe *de lui même* à l'état $S$." (Lorentz 1902, p. 153; emphasis in original).

to be derived from the action of forces. This is indicative of Lorentz's general attitude toward the generalized contraction hypothesis. His strategy is to look for a subset of the consequences of the hypothesis that are not just individually necessary but jointly sufficient conditions for the generalized contraction effect. The generalized contraction hypothesis can then be derived using these conditions as premises in the derivation, premises for which Lorentz tried to find independent support.[73] When he wrote the sketchy last section of his 1899 paper, Lorentz may have vacillated between giving the overarching generalized contraction hypothesis or a necessary and sufficient subset of its consequences logical priority. In the end, however, he settled on the latter alternative.

This is especially clear in his much more systematic presentation of the new theory in his famous 1904 paper, as the following quotation from that paper demonstrates:

> [...] I shall suppose *that the forces between uncharged particles, as well as those between such particles and electrons, are influenced by a translation* [through the ether] *in quite the same way as the electric forces in an electrostatic system*. In other terms, whatever be the nature of the particles composing a ponderable body, so long as they do not move relatively to each other, we shall have between the force acting in a system ($\Sigma'$) without, and the same system ($\Sigma$) with a translation, the relation [$\mathbf{F}(\Sigma) = \mathrm{diag}(l^2, l^2/\gamma, l^2/\gamma)\ \mathbf{F}(\Sigma')$; cf. Eq. 3.63], if as regards the relative position of the particles $\Sigma'$ is got from $\Sigma$ by the deformation [diag($\gamma l$, $l$, $l$)], or $\Sigma$ from $\Sigma'$ by the deformation [diag($1/\gamma l$, $1/l$, $1/l$)].
>
> We see by this that, as soon as the resulting force is 0 for a particle in $\Sigma'$, the same must be true for the corresponding particle in $\Sigma$. Consequently, if, neglecting the effects of molecular motion, we suppose each particle of a solid body to be in equilibrium under the action of the attractions and repulsions exerted by its neighbors, and if we take for granted that there is but one configuration of equilibrium, [74] we may draw the conclusion that the system $\Sigma'$, if the velocity $w$ is imparted to it, will *of itself* change into the system $\Sigma$. In other terms, the translation will *produce* the deformation [diag($1/\gamma l$, $1/l$, $1/l$)].
>
> The case of molecular motion will be considered in § 12. (Lorentz 1904b, p. 183; italics in original)

The reader will recognize the statement of the generalized contraction hypothesis toward the end of this statement: "the system $\Sigma'$, if the velocity $w$ is imparted to it, will *of itself* change into the system $\Sigma$. In other terms, the translation will *produce* the deformation [diag($1/\gamma l$, $1/l$, $1/l$)]." In section 12, Lorentz introduces the assumption that the mass of all particles depends on their velocity with respect to the ether in a certain way. The conjunction of the assumptions about the effect of ether drift on masses and forces suffices to derive the generalized contraction hypothesis in the context of optical experiments that eventually boil down to the observation of a pattern of light and darkness.

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[73] The most important example in this category is the hypothesis of the velocity dependence of the mass of electrons that is entailed by the generalized contraction hypothesis. In 1899, Lorentz notes that this is a necessary condition for the generalized contraction effect (I will go over this part of Lorentz's argument at the end of this section). In 1904, he provides a detailed model of the electron whose mass exhibits the required velocity dependence (see section 3.4).

[74] As an aside, I want to draw attention to the fact that Lorentz now explicitly mentions the assumption that the equilibrium state is unique, an assumption he made tacitly in 1892 and 1895 (see section 3.2).

The fact that Lorentz chose to derive the generalized contraction hypothesis from other more specific hypotheses about the effect of ether drift on physical phenomena is largely responsible, I think, for the impression of a proliferation of hypotheses that modern readers have come away with after reading Lorentz 1904b (see, e.g., Holton 1969, pp. 323–324; Schaffner 1976, pp. 473–474). In fact, Lorentz and his contemporaries remained well aware of the fact that all these hypotheses served the same purpose, i.e., they explain why corresponding states physically transform into one another.[75] A clear understanding of this point is crucial for a proper assessment of Lorentz's later attitude toward special relativity (see chapter four).

The conclusion of the final section of Lorentz's 1899 paper clearly illustrates Lorentz's understanding of the status of the generalized contraction hypothesis. Implicitly referring to Liénard's proposal that motivated the discussion in the final section of the paper, Lorentz writes:

> If the hypothesis[76] might be taken for granted, Michelson's experiment should always give a negative result, whatever transparent media were placed on the path of the rays of light, and even if one of these went through air, and the other, say through glass. This is seen by remarking that the correspondence between the two motions [read: corresponding states] we have examined is such that, if in $S_0$ we had a certain distribution of light and dark (interference bands) we should have in $S$ a similar distribution, which might be got from those in $S_0$ by the dilatations (6) [i.e, in my notation (see Eq. 3.54), $\mathbf{x}' = \mathrm{diag}(l\gamma, l, l)\,\mathbf{x}$], provided however that in $S$ the time of vibration be $k\varepsilon$ times as great as in $S_0$. The necessity of this last difference follows from (9) [i.e, in my notation (see Eq. 3.54), $t' = l\,(t/\gamma - \gamma\,(v/c^2)x)$[77]]. Now the number $k\varepsilon$ would be the same in all positions we can give to the apparatus; therefore, if we continue to use the same sort of light, while rotating the instruments, the interference bands will never leave the parts of the ponderable system, e.g. the lines of a micrometer, with which they coincided at first. (Lorentz 1899b, pp. 272–273; italics in the original)

At the risk of oversimplifying matters, I want to briefly summarize the progress that is made in the transition from the 1895 theory in the *Versuch* to the 1899 theory in the final section of "Simplified theory of electrical and optical phenomena in moving bodies." In 1895 the theorem of corresponding states and the contraction hypothesis were utterly disjoint parts of the

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[75] Although I disagree with Miller's interpretation of Lorentz's theory (see above), I think his phrase "interlocking hypotheses" (Miller 1980, p. 73) nicely captures the situation.

[76] This refers to the specific hypothesis about the velocity dependence of the mass of the electron and not to the overarching generalized contraction hypothesis. The original Dutch version of this paragraph starts with "Were *all the assumptions* mentioned correct ..." (Lorentz 1899a, p. 522; my emphasis).

[77] Actually, we have to be a little more careful. The effect of motion with respect to the ether on the frequency of the light emitted by the source in the interferometer follows from $t' = l\,(t/\gamma - \gamma\,(v/c^2)x)$ *and* the generalized contraction hypothesis. Notice that, when this slight inaccuracy is removed, the effect is introduced as a consequence of the generalized contraction hypothesis. This shows that Lorentz did not intend to include this particular consequence of the hypothesis in the subset of necessary and sufficient conditions for the hypothesis from which he wanted to derive that corresponding states physically transform into one another.

As an aside, I want to mention that this is the only passage in Lorentz's pre-1905 publications that I know of, where he mentions an instance of what we now recognize as the relativistic time dilation effect (cf. Lorentz 1916, pp. 208–209).

theory. By 1899, however, the contraction has come to be firmly incorporated into the exact version of the theory, the very idea of assuming a physical contraction arising in a natural way from the mathematical theorem of corresponding states. As a consequence the 1899 theory provides a general explanation for a broad class of both first and second order ether drift experiments whereas in the 1895-theory the second order experiments had to be accounted for one by one.

**3.3.5 How the relativistic formulae for aberration and Doppler effect drop out of Lorentz's exact theorem of corresponding states and the generalized contraction hypothesis.** It is sometimes said that Lorentz's theory is not empirically equivalent to special relativity because it would fail to yield the relativistic expressions for stellar aberration and the transverse Doppler effect. Arthur Miller has repeatedly made that claim.[78] In fact, the relativistic expressions for Doppler effect and aberration drop out of Lorentz's exact theory of 1899 and 1904 in precisely the same way as the classical expressions drop out of Lorentz's first order theory of 1895 (see section 3.1). The only difference is that Lorentz, as far as I know, never went through the exact analogues of these calculations in the *Versuch*. It is true, however, that it is only after Lorentz's reinterpretation, after 1905 and under the influence of Einstein, of the primed quantities of his theorem of corresponding states that the predictions of his theory for aberration and Doppler effect become fully equivalent to the relativistic predictions.

Fig. 3.8 combines Fig. 3.2 in section 3.1 and half of Fig. 3.7. On the left, we have a plane light wave of frequency $\nu$ traveling through the ether in the direction of the unit vector **n**, the normal on its wave fronts (cf. Fig. 3.2). On the right, we have a contracted interferometer moving through the ether at a velocity **v** (cf. Fig. 3.7).

We can look upon the drawing on the left as a blown up snapshot of a small region in the arm of the moving interferometer with mirror 1, a snapshot taken as a light wave is traveling

[78] In his critique of Zahar, for instance, Miller writes: "Zahar [1973, p. 235] also makes the claim (repeats an old mistake) that Einstein's and Lorentz's theories are observationally equivalent. Special relativity is not observationally equivalent to Lorentz's theory of 1904. The reason is that a theory of light containing Lorentz's ether cannot account *exactly* for the optical Doppler effect, nor for observations of stellar aberration" (Miller 1974, p. 42; italics in original). In a footnote, Miller refers to "a forthcoming work by the author" for a full discussion of this point. Such a discussion can be found in Miller 1981, pp. 301–307. And earlier in his book, Miller writes: "Lorentz's theory did not predict a transverse Doppler effect, although Einstein made no mention of this point" (ibid., p. 224). My calculations will show that Miller's claim is wrong (thus giving a simple explanation of Einstein's taciturnity on this point!), as Miller generously conceded when, in 1988, I showed him a manuscript, in Dutch, with a clumsy version of my calculations. Miller is right to point out that special relativity is not observationally equivalent to Lorentz's 1904 theory. However, Zahar was talking about the post-1905 version of Lorentz's theory, which actually is empirically equivalent to special relativity, although, as will be clear from the discussion in chapters one and two, the equivalence required Lorentz to take over much more from special relativity than (as Zahar wants to have it) the idea that the primed quantities of the theorem of corresponding states are the quantities a moving observer actually measures.

from mirror 1 back to the beam splitter. The vector $\mathbf{n} - \mathbf{v}/c$ points in the direction of this arm (cf. Fig. 3.3 in section 3.2).

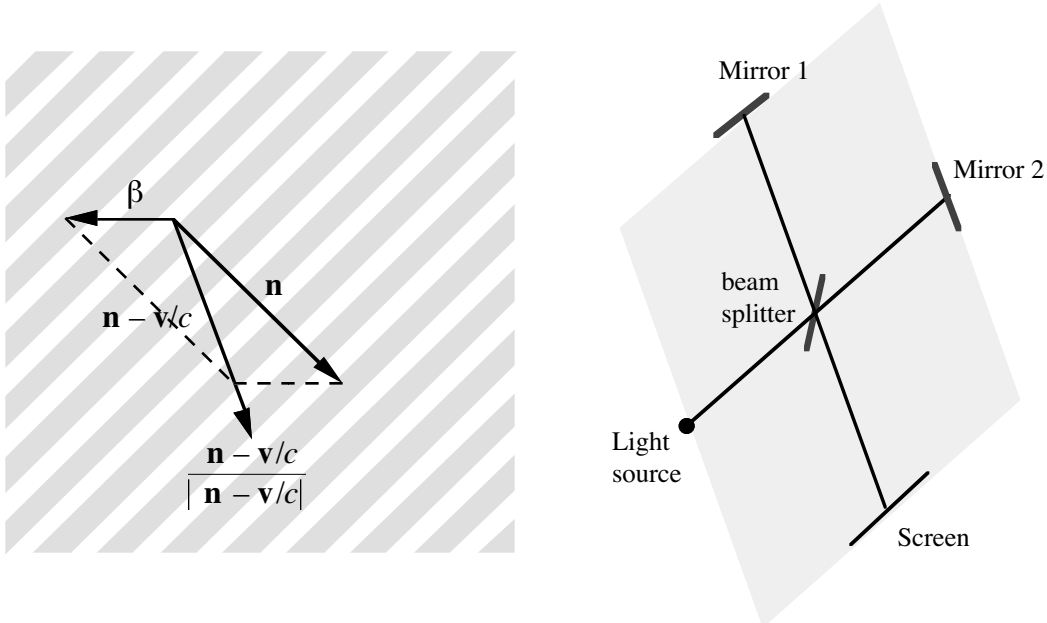


Figure 3.8: Aberration and Doppler effect in Lorentz's exact theory of 1899 and 1904.

Since Lorentz's theory—the exact theorem of corresponding states plus the generalized contraction hypothesis—predicts that for a co-moving observer the interference pattern in the moving interferometer will be no different from the interference pattern the observer would measure if the experiment were to be repeated with both the observer and the interferometer at rest in the ether, the light wave must have the direction

$$\mathbf{n}_{\text{obs}} = \frac{\text{diag}(\gamma, 1, 1)\ \left(\mathbf{n} - \mathbf{v}/c\right)}{\left|\,\text{diag}(\gamma, 1, 1)\ \left(\mathbf{n} - \mathbf{v}/c\right)\right|} \tag{3.72}$$

for the co-moving observer. After all, a light wave in the direction $\mathbf{n} - \mathbf{v}/c$ will behave as a light wave in the direction $\text{diag}(\gamma, 1, 1)\ (\mathbf{n} - \mathbf{v}/c)$ in all experiments with this light wave carried out by the co-moving observer (cf. the contracted moving interferometer on the right of Fig. 3.7 and the uncontracted interferometer at rest in the ether on the left). For the denominator in Eq. 3.72, we can write:[79]

[79] Writing **n** and **v** out in components, we find

$$\left|\,\text{diag}(\gamma, 1, 1)\left(\mathbf{n} - \mathbf{v}/c\right)\right|^2 = \gamma^2 (n_x - \beta)^2 + n_y^2 + n_z^2.$$

Inserting $n_y^2 + n_z^2 = 1 - n_x^2 = \gamma^2 (1 - \beta^2)\ (1 - n_x^2)$, we can rewrite this as

$$\left| \operatorname{diag}(\gamma, 1, 1) \ \left(\mathbf{n} - \mathbf{v}/c\right) \right| = \gamma\,(1 - \beta n_x) \tag{3.73}$$

Inserting Eq. 3.73 into Eq. 3.72, we find

$$\mathbf{n}_{\text{obs}} = \frac{1}{\gamma\,(1 - \beta n_x)} \begin{pmatrix} \gamma\,(n_x - \beta) \\ n_y \\ n_z \end{pmatrix} \tag{3.74}$$

This relation between $\mathbf{n}_{\text{obs}}$ and $\mathbf{n}$ will hold no matter what the state of motion of the source is, even though it was derived for light coming from a co-moving source. In other words, Eq. 3.74 gives the general formula for aberration we would expect on the basis Lorentz's exact theory. Notice that this formula is just the classical aberration formula corrected for the Lorentz-FitzGerald contraction effect.

A completely analogous argument leads to the formula for the Doppler effect we would expect on the basis of Lorentz's theory. Consider Fig. 3.8 again. Due to the classical Doppler effect, the light wave with frequency $\nu$ and direction $\mathbf{n}$ will have frequency $(1 - \beta n_x)\,\nu$ (see Eq. 3.16) for an observer moving through the ether at a velocity $\mathbf{v}$. On top of this effect, we need to take into account that for a moving observer the frequency will differ by a factor $\gamma/l$ from the frequency for an observer at rest in the ether. After all, as we saw earlier in this section, Lorentz's theory predicts *both* that the frequency of light emitted by a moving source differs by a factor $l/\gamma$ from the frequency of the light emitted by the same source at rest in the ether, *and* that the moving observer has no way of actually detecting this effect by observing patterns of light and darkness in his laboratory. In this way, we arrive at the relation

$$\nu_{\text{obs}} = \frac{\gamma}{l}\left(1 - \beta n_x\right)\nu. \tag{3.75}$$

As with Eq. 3.74, this equation will hold no matter what the state of motion of the source is, even though it was derived for light coming from a co-moving source.

There are two questions that need to be addressed at this point. First, does Lorentz's exact theory actually predict the formulae for aberration and Doppler effect in Eqs. 3.74–3.75?

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$$\begin{aligned} \left| \operatorname{diag}(\gamma, 1, 1) \left(\mathbf{n} - \mathbf{v}/c\right) \right|^2 &= \gamma^2\,(n_x - \beta)^2 + \gamma^2 (1 - \beta^2)\,(1 - n_x^2) \\ &= \gamma^2\,(n_x^2 - 2\beta n_x + \beta^2 + 1 - \beta^2 - n_x^2 + \beta^2 n_x^2) \\ &= \gamma^2\,(1 - \beta n_x)^2. \end{aligned}$$

Taking the square root on both sides, we arrive at Eq. 3.73.

Second, are these predictions the same as the predictions of the special theory of relativity? The answers to these two questions are 'yes' and 'yes, provided we set $l = 1$,' respectively.

To prove these claims, I will proceed the same way I did in section 3.1 to verify that Lorentz's first order theory predicts the classical formulae for aberration and Doppler effect. The $(\mathbf{x}', t')$-dependence of the fields (fictitious and real) describing a light wave of frequency $\nu$ in the direction $\mathbf{n}$ can only be through

$$\nu_{\text{obs}}\left(t' - \frac{\mathbf{n}_{\text{obs}} \cdot \mathbf{x}'}{c}\right), \tag{3.76}$$

otherwise we would run into conflicts with the general result concerning patterns of light and darkness in Lorentz's theory. On the other hand, we know that the $(\mathbf{x}_0, t_0)$-dependence of the fields describing a plane light wave of frequency $\nu$ in the direction $\mathbf{n}$ is given by

$$\nu\left(t_0 - \frac{\mathbf{n} \cdot \mathbf{x}_0}{c}\right), \tag{3.77}$$

as can be seen upon inspection of Fig. 3.8. What needs to be shown is that if we substitute Eq. 3.57 for the transformation from $(\mathbf{x}_0, t_0)$ to $(\mathbf{x}', t')$ and Eqs. 3.74–3.75 for $\mathbf{n}_{\text{obs}}$ and $\nu_{\text{obs}}$ into Eq. 3.76, we end up with Eq. 3.77. A tedious but essentially straightforward calculation shows that this is indeed the case. Fortunately, there is a shortcut for this calculation, which at the same time clarifies the relation of these calculations in Lorentz's theory to the special relativistic treatment of Doppler effect and aberration.

I define the following quantities

$$k'^{\mu} \equiv (\nu_{\text{obs}}, \nu_{\text{obs}}\mathbf{n}_{\text{obs}}), \qquad k_0^{\mu} \equiv (\nu, \nu\mathbf{n}) . \tag{3.78}$$

With the help of Eqs. 3.74–3.75, the relation between these quantities can be written as

$$\begin{aligned}
\nu_{\text{obs}} &= \frac{1}{l}\gamma\left(\nu - \beta(\nu n_x)\right), \\
\nu_{\text{obs}} n_{\text{obs}_x} &= \frac{1}{l}\gamma\left(\nu n_x - \beta\nu\right), \\
\nu_{\text{obs}} n_{\text{obs}_y} &= \frac{1}{l}\nu n_y, \\
\nu_{\text{obs}} n_{\text{obs}_z} &= \frac{1}{l}\nu n_z.
\end{aligned} \tag{3.79}$$

In modern notation, we would write this as

$$k'^{\mu} = \frac{1}{l}\Lambda^{\mu}{}_{\nu}\, k_0^{\nu}, \tag{3.80}$$

just as we would write Eq. 3.57 as

$$x'^{\mu} = l\,\Lambda^{\mu}{}_{\nu}\,x_0^{\nu}, \tag{3.81}$$

with $x'^{\mu} \equiv (ct', \mathbf{x}')$, $x_0^{\nu} \equiv (ct_0, \mathbf{x}_0)$, and

$$\Lambda^{\mu}{}_{\nu} = \begin{pmatrix} \gamma & -\gamma\beta & 0 & 0 \\ -\gamma\beta & \gamma & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{pmatrix}. \tag{3.82}$$

With the help of Eq. 3.78 and $x'^{\mu} \equiv (ct', \mathbf{x}')$, Eq. 3.76 can be rewritten as

$$\nu_{\text{obs}}\left(t' - \frac{\mathbf{n}_{\text{obs}} \cdot \mathbf{x}'}{c}\right) = \eta_{\mu\nu}\,k'^{\mu}\,x'^{\nu}/c \tag{3.83}$$

With the help of Eqs. 3.80–3.81, the right hand side of Eq. 3.83 can be rewritten as

$$\eta_{\mu\nu}\,k'^{\mu}\,x'^{\nu}/c = \eta_{\mu\nu}\left(\frac{1}{l}\,\Lambda^{\mu}{}_{\rho}\,k_0^{\rho}\right)\left(l\,\Lambda^{\nu}{}_{\sigma}\,x_0^{\sigma}/c\right) = \eta_{\rho\sigma}\,k_0^{\rho}\,x_0^{\sigma}/c, \tag{3.84}$$

where in the last step I used the defining relation $\eta_{\mu\nu}\,\Lambda^{\mu}{}_{\rho}\,\Lambda^{\nu}{}_{\sigma} = \eta_{\rho\sigma}$ of the Lorentz transformation. The last expression in Eq. 3.84 is just an alternative way of writing the expression in Eq. 3.54.

This proves that Lorentz's exact theory does indeed predict the formulae for aberration and Doppler effect in Eqs. 3.74–3.75. The fact that these equations can be rewritten in the form of Eqs. 3.79–3.80 shows that, for $l = 1$, Lorentz's predictions simply reduce to the relativistic predictions.

This still does not constitute a full proof that Lorentz's exact theory and special relativity are empirically equivalent as far as aberration and Doppler effect are concerned. From a relativistic point of view, we have only dealt so far with the case of how an observer in a frame moving with respect to the ether measures the frequency and the direction of light that has a certain (different) frequency and direction for an observer in the ether frame. We have not dealt with the "reverse case" of how an observer in the ether frame measures the frequency and the direction of light that has a certain (different) frequency and direction for an observer in the frame moving with respect to the ether. Lorentz would only consider such reverse cases after 1905, under the influence of Einstein. As we will see in section 3.5, it is clear from Lorentz's

understanding of such reverse cases in general that his theory will *always* give the same predictions for Doppler effect and aberration as special relativity.[80]

**3.3.6 The generalized contraction hypothesis and the velocity dependence of the mass of electrons.** As we have seen, the generalized contraction hypothesis, the assumption that corresponding states physically transform into one another, entails much more than a change in the dimensions of bodies when they are set in motion with respect to the ether, the only effect for which the original contraction hypothesis gave a definite prediction. Perhaps the most surprising effect entailed by the hypothesis and certainly the effect that gets most attention in the final section of Lorentz's 1899 paper is the dependence of the mass of electrons responsible for the emission of light on their velocity. The argument roughly runs as follows. Consider an oscillating electron generating an electromagnetic wave in a light source in the frame $S_0$ at rest in the ether. The oscillation will satisfy Newton's laws of motion. Now consider that same electron in the corresponding state in the Galilean frame $S$ moving through the ether at a velocity $\mathbf{v}$. In terms of the auxiliary quantities in Eq. 3.54 its motion will be exactly the same as the motion in the system at rest. This, in turn, implies that in terms of the *real* quantities, it will *not* satisfy Newton's laws of motion, unless the electron's mass depends on its velocity in a specific way.

The actual calculations are very simple. The oscillation in the light source at rest in the ether frame $S_0$ will have to satisfy Newton's second law

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[80] The relativistic expression for the Doppler effect was first verified in a celebrated experiment by Ives and Stillwell (1938). In their conclusion, the experimenters write that their results "may [...] be claimed to give more decisive evidence for *the Larmor-Lorentz theory*" (Ives and Stillwell 1938, p. 266; my emphasis; quoted and discussed in Swenson 1972, p. 237).

It is interesting to notice that the 1899 version of Lorentz's theory gives the correct predictions for the experiments of Michelson and Morley, Kennedy and Thorndike, and Ives and Stillwell. In 1949, H. P. Robertson, well-known for his work in general relativistic cosmology, especially for the (Friedman-Lemaître-) Robertson-Walker metric (Eisenstaedt 1993, p. 377, note 31), published a paper entitled "Postulate *versus* observation in the special theory of relativity," in which he proposed to derive the basic kinematics of special relativity, not from Einstein's two postulates but from the results of these three experiments. Robertson motivated this enterprise pointing to "a certain reluctance wholeheartedly to accept its necessity [i.e., the necessity of the special theory of relativity], a reluctance shared at times even by scientists whose own work paved the way to, or confirmed the predictions of, the theory" (Robertson 1949, p. 378). Robertson does not identify such scientists, but it seems safe to conclude that his list would include Lorentz, Michelson, and Ives. Kennedy and Thorndike are on Robertson's side as is clear from the inflated title of the paper in which they report the null result of their experiment: "Experimental establishment of the relativity of time" (Kennedy and Thorndike 1932). In his conclusion, Robertson writes: "The three second-order optical experiments of Michelson and Morley, of Kennedy and Thorndike, and of Ives and Stillwell, furnished empirical evidence which, within the limits of the inductive method, enabled us to conclude that [...] the kinematics *im kleinen* [remember that Robertson worked in relativistic cosmology] of physical space-time is [...] governed by the Minkowski metric [...] the background upon which the special theory of relativity and its later extension to the general theory are based" (Robertson 1949, p. 382). Even granting Robertson this conclusion, one wonders whether Robertson realized in 1949 that despite all this one can still retain the ether and Newtonian space-time, as is clear from the fact that with the 1899 version of Lorentz's theory we have no difficulty whatsoever dealing with all three experiments that form the input of Robertson's argument. (For more on Robertson, see Urani and Gale 1993.)

$$\mathbf{F}_0 = m_0\, \mathbf{a}_0. \tag{3.85}$$

In the corresponding state of the light source at rest in the Galilean frame $S$, moving through the ether at a velocity $\mathbf{v}$, the oscillation will satisfy the same equation in terms of the auxiliary quantities, i.e.,

$$\mathbf{F}' = m_0\, \mathbf{a}', \tag{3.86}$$

where $\mathbf{F}'$ is the same function of $(\mathbf{x}', t')$ as $\mathbf{F}_0$ is of $(\mathbf{x}_0, t_0)$, and where $\mathbf{a}' \equiv d^2\mathbf{x}'/dt'^2$ is the same as $\mathbf{a}_0 \equiv d^2\mathbf{x}_0/dt_0^2$. Lorentz assumes that motion through the ether affects all forces on the electron in the same way in which it affects Coulomb forces. Using the inverse of Eq. 3.63, we can then write $\mathbf{F}'$ as

$$\mathbf{F}' = \mathrm{diag}(1/l^2,\ \gamma/l^2,\ \gamma/l^2)\ \mathbf{F}. \tag{3.87}$$

For the relation between the acceleration $\mathbf{a}'$ in terms of the fictitious space and time coordinates and the real acceleration $\mathbf{a}$, Lorentz uses the relation

$$\mathbf{a}' = \mathrm{diag}(\gamma^3/l,\ \gamma^2/l,\ \gamma^2/l)\ \mathbf{a}. \tag{3.88}$$

In general, this relation is far more complicated, but when the velocity of the electron oscillating in the system at rest in the ether can be neglected (which means that $d\mathbf{x}'/dt'$ can be neglected in the system in motion), the relation actually simplifies to Eq. 3.88. A derivation of the general relation between $\mathbf{a}'$ and $\mathbf{a}$ was given by Planck (1906a) in the context of his derivation of the relativistic generalization of Newton's second law, a derivation which is, in fact, mathematically equivalent to Lorentz's 1899 derivation of the velocity dependence of mass required by the generalized contraction hypothesis, except for the fact that Planck only needs to consider the case where $l = 1$.[81]

My hunch is that Lorentz actually arrived at Eq. 3.88 through the following crude argument. If an electron oscillates around a fixed point of $S$ with a low velocity and a small amplitude, the $x$-dependent term in the expression of local time can be ignored. In that case, we only need to take into account that $\mathbf{x}'$ differs from $\mathbf{x}$ by a factor $\mathrm{diag}(\gamma l, l, l)$ and that $t'$ differs from $t$ by a factor $l/\gamma$. This gives a quick and dirty derivation of Eq. 3.88:

$$\mathbf{a}' = \frac{d^2\mathbf{x}'}{dt'^2} = \mathrm{diag}(\gamma l,\ l,\ l)\ (\gamma/l)^2\, \frac{d^2\mathbf{x}}{dt^2} = \mathrm{diag}(\gamma^3/l,\ \gamma^2/l,\ \gamma^2/l)\ \mathbf{a}. \tag{3.89}$$

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[81] For an elegant and elementary exposition of Planck's derivation, see Zahar 1989, section 7.1, pp. 227–237. The equations for the relation between $\mathbf{a}'$ and $\mathbf{a}$ can be found on p. 232 (Eqs. (2)–(4)).

Inserting Eqs. 3.87–3.88 into Eq. 3.86, we find

$$\mathrm{diag}(1/l^2,\ \gamma/l^2,\ \gamma/l^2)\ \mathbf{F} = m_0\ \mathrm{diag}(\gamma^3/l,\ \gamma^2/l,\ \gamma^2/l)\ \mathbf{a}. \tag{3.90}$$

This can be rewritten as

$$\mathbf{F} = m_0\ \mathrm{diag}(\gamma^3 l,\ \gamma l,\ \gamma l)\ \mathbf{a}. \tag{3.91}$$

From Eq. 3.91, we conclude that the oscillation of an electron in the moving source can only satisfy Newton's second law if the mass $m$ of an electron with velocity $\mathbf{v}$ with respect to the ether (remember that the velocity of the oscillation itself was assumed to be negligible) differs from the mass $m_0$ of an electron at rest in the ether, in precisely the following way:

$$m = \mathrm{diag}(\gamma^3 l,\ \gamma l,\ \gamma l)\ m_0. \tag{3.92}$$

Notice that a distinction has to be made between accelerations in the direction of motion and accelerations perpendicular to the direction of motion. At the beginning of the century when the velocity dependence of high speed electrons in β-radiation became a lively area of experimental and theoretical research (see section 3.4), the corresponding masses would come to be known as the *longitudinal mass* ($m_\mathrm{L}$) and the *transverse mass* ($m_\mathrm{T}$), respectively. From Eq. 3.92, we read off that

$$m_\mathrm{L} = \gamma^3 l m_0, \qquad m_\mathrm{T} = \gamma l m_0 \tag{3.93}$$

As Planck showed in the paper mentioned above (Planck 1906a), these relations also obtain in special relativity.[82] However, Planck's interpretation of these relations was very different from Lorentz's. For Planck, as for Einstein, the velocity dependence of mass was part of a new relativistic mechanics replacing classical Newtonian mechanics. Lorentz wanted to retain Newtonian mechanics, even after the generalized contraction hypothesis with which he wanted to account for the negative results of ether drift experiments had forced him by 1904 to assume that in nature there are no Galilean invariant Newtonian masses or forces. Consequently, he felt he had to provide an explanation for the fact that the mass of an electron is not simply a Galilean invariant Newtonian mass. For a proper understanding of Lorentz's later attitude toward special relativity, it is important to keep this in mind.

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[82] Einstein (1905a, p. 919) obtained $m_\mathrm{T} = \gamma^2 m_0$ instead of $m_\mathrm{T} = \gamma m_0$, the result obtained by Planck and Lorentz (Lorentz had set $l$ equal to 1 in 1904). Using the frames $S_0$ and $S'$ used above to represent two arbitrary Lorentz frames, we can readily see where the discrepancy is coming from. Einstein used $\mathbf{F}' = \mathbf{F}$ instead of $\mathbf{F}' = \mathrm{diag}(1, 1/\gamma, 1/\gamma)\ \mathbf{F}$, the now standard transformation law for forces used by Lorentz and Planck (cf., e.g., Zahar 1989, p. 233). Einstein was well aware of the arbitrariness of his definition of force. As a matter of fact, he already emphasized this in his first relativity paper (Einstein 1905a, p. 919).

To conclude this section, I want to quote the passage from Lorentz's 1899 paper in which he derives the velocity dependence of the mass of electrons that is required by the generalized contraction hypothesis:

> We have already seen that, in the states of equilibrium [around which the oscillations of the electron take place], the electric forces parallel to $OX$, $OY$, $OZ$ [i.e., the axes of the Galilean frame $S$], existing in $S$ differ from the corresponding forces in $S_0$[83] by the factors $1/\varepsilon^2$, $1/k\varepsilon^2$, and $1/k\varepsilon^2$.[84]
>
> From (Ve) [the equation for the Lorentz force] it appears that these same factors come into play when we consider the part of the electric forces that is due to the vibrations. If, now, we suppose that the molecular forces are modified in quite the same way in consequence of the translation, we may apply the just mentioned factors to the components of the *total* force acting on an ion. Then the imagined motion in $S$ will be a possible one, provided that these same factors to which we have been led in examining the forces present themselves again, when we treat of the product of the masses and the accelerations
>
> According to our suppositions, the accelerations in the directions of $OX$, $OY$, $OZ$ in $S$ are respectively $1/k^3\varepsilon$, $1/k^2\varepsilon$ and $1/k^2\varepsilon$ times what they are in $S_0$ [cf. Eq. 3.88]. If therefore the required agreement is to exist with regard to the vibrations parallel to $OX$, the ratio of the masses of the ions in $S$ and $S_0$ should be $k^3/\varepsilon$; on the contrary we find for this ratio $k/\varepsilon$, if we consider in the same way the forces and the accelerations in the directions of $OY$ and $OZ$.
>
> Since $k$ is different from unity, these values cannot both be 1; consequently, states of motion, related to each other in the way we have indicated, will only be possible if in the transformation of $S_0$ into $S$ the masses of the ions change; even this must take place in such a way that the same ion will have different masses for vibrations parallel and perpendicular to the velocity of translation. (Lorentz 1899b, pp. 271–272; italics in the original)

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[83] Lorentz's '$S$' and '$S_0$' are what I called the corresponding states in the frames $S$ and $S_0$.

[84] In my notation, this would be $l^2$, $l^2/\gamma$, and $l^2/\gamma$, respectively (see Eq 3.87 or Eq. 3.63).

## 3.4 Lorentz's electron model (1904)

**3.4.1 The dilemma posed by the Lorentz, Abraham, and Bucherer-Langevin electron models: the electromagnetic view of nature or the principle of relativity.** The main justification for Lorentz's 1899 hypothesis that the electron mass has just the velocity dependence required by what I called the generalized contraction hypothesis, was undoubtedly that this hypothesis would help explain the null results, both actually obtained and anticipated, of a wide variety of optical ether drift experiments. However, Lorentz was not the first to entertain the idea that mass might depend on velocity, which offered at least some hope for finding independent support for the hypothesis (cf. my remarks on Lorentz's general attitude toward the generalized contraction hypothesis in section 3.3). In the paragraph immediately following the quotation from Lorentz 1899b that I gave at the end of section 3.3, Lorentz expresses this hope:

> Such a hypothesis seems very startling at first sight. Nevertheless we need not wholly reject it. Indeed, as is well-known, the *effective* mass of an ion depends on what goes on in the aether; it may therefore very well be altered by a translation and even to different degrees for vibrations of different directions. (Lorentz 1899b, p. 272; italics in original)

Lorentz's "as is well-known" probably refers to work by Thomson in 1881 and 1889, Heaviside in 1889, and Searle in 1897 (Miller 1981, p. 46), who showed that the interaction of a charged sphere with its self-field gives the sphere additional inertia over and above its Newtonian mass.

In his proclamation of the electromagnetic world picture,[85] Wien (1900) emphasized these results. Following up on Wien's suggestion, Abraham, who would become one of the leading proponents of the electromagnetic view of nature, worked out an electron model in which the total mass of the electron comes from the interaction with its self-field (Abraham 1903, 1904, 1905; see Miller 1981, pp. 55–61). In Abraham's model, the electron is a rigid spherical charge distribution, where by 'rigid' I mean that the electron retains its spherical shape when it is set in motion with respect to the ether instead of being subject to the Lorentz-FitzGerald contraction. The expressions Abraham found for the velocity dependence of the longitudinal and transverse mass of the electron are similar to but not the same as the relations in Eq. 3.93 required by Lorentz's generalized contraction hypothesis (see, e.g., Miller 1981, p. 60, Eqs. (1.143)–(1.144)).

Stimulated by Wien's ideas about an electromagnetic world picture, Kaufmann, Abraham's experimentalist colleague in Göttingen in the years 1901–1903 (Miller 1981, p. 48, p. 66),

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[85] See McCormmach 1970b for an excellent discussion of the electromagnetic view of nature. For a more recent discussion (in Dutch), see Bosman 1987.

started experimenting with β-radiation—or 'Becquerel rays' as they were called at the time—to determine the velocity dependence of the transverse mass of high speed electrons. Kaufmann refined these experiments as he moved from Berlin to Göttingen to Bonn in the early years of the century. As I already pointed out in the introduction, the reception of Einstein's special theory of relativity took place in the context of the debate over Kaufmann's results.[86]

By 1905, there were three different predictions for the velocity dependence of the transverse mass of the electron, based on three slightly different models of the electron, the Abraham model of 1903, the Lorentz model of 1904, and the Bucherer-Langevin model of 1904–1905. In all three models, the mass of the electron is of purely electromagnetic origin. Moreover, an electron at rest in the ether is a spherical charge distribution in all three models. The models differ in their predictions about the shape of an electron in motion with respect to the ether. Unlike Abraham's model, the Lorentz model and the Bucherer-Langevin model make the electron subject to the Lorentz-FitzGerald contraction. In both models, a moving electron will be contracted by a factor $\gamma l$ in the direction of motion and by a factor $l$ in the directions perpendicular to the direction of motion. The difference between the two models for such a contractile electron, is that the factor $l$ has the value $l = 1$ in the Lorentz model and the value $l = \gamma^{-1/3}$ in the Bucherer-Langevin model. So, the Lorentz electron only contracts in the direction of motion, while the Bucherer-Langevin electron contracts by a factor $\gamma^{2/3}$ in the direction of motion and dilates by a factor $\gamma^{1/3}$ in the directions perpendicular to the direction of motion. In other words, the Bucherer-Langevin electron changes its shape, just as the Lorentz electron, but not its volume.

The reason Lorentz picked $l = 1$ is that this is the only value for which the longitudinal and transverse mass of a contractile electron satisfy Eq. 3.93 required by the generalized contraction hypothesis. As was shown by Poincaré (1905, 1906; see Miller 1973, pp. 277–279), however, $l = \gamma^{-1/3}$, the value for $l$ in the Bucherer-Langevin model, is the only value for which the contractile electron is compatible with the electromagnetic view of nature. So, we have a dilemma. There is no electron model that is both compatible with the electromagnetic view of nature and compatible with the general experimental indication that we will never be able to detect ether drift, and therefore with Einstein's relativity principle. The Lorentz electron is incompatible with the electromagnetic view of nature. The Abraham and Bucherer-Langevin electrons are incompatible with the absence of any signs of ether drift. As I mentioned in

[86] For a fascinating discussion of this debate with numerous quotations from the relevant work of the experimentalists, theorists, and mathematicians involved (including Kaufmann, Abraham, Lorentz, Poincaré, Einstein, Bucherer, Planck, Sommerfeld, and Minkowski), see Miller 1981, especially, pp. 47–54, 61–67, 225–235, 334–352.

section 2.2, Lorentz had no qualms about abandoning the electromagnetic view of nature in the face of this dilemma.[87]

Lorentz's work in 1904 on a concrete model for the electron exhibiting the properties required by the generalized contraction hypothesis is extremely important, I think, for a proper understanding of his later attitude toward aspects of Einstein's theory of relativity (see chapter four). I therefore want to give a self-contained exposition of Lorentz's model and its problems (cf. Miller 1981, especially pp. 73–75).

**3.4.2 The electromagnetic mass of spherical charge distributions subject to the Lorentz-FitzGerald contraction.** The electromagnetic mass of a charge distribution is the inertia of that charge distribution due to the interaction with its self-field. The force an arbitrary static charge distribution experiences from its self field when its moves through the ether at some constant velocity **v** is given by

$$\mathbf{F}_{\text{self}} = \int \mathbf{f}\ d^3x = \int \rho\left(\mathbf{E} + \mathbf{v} \times \mathbf{B}\right) d^3x\ , \tag{3.94}$$

where **E** and **B** are the fields generated by the charge distribution itself. The components of the force density **f** can be written as (cf. section 1.4, Eqs. 1.44–1.54 and section 2.4, Eq. 2.117 and Eq. 2.119)

$$f^i = \partial_j T^{ij} - \dot{g}^i, \tag{3.95}$$

where $T^{ij}$ is the Maxwell stress tensor and where **g** is the electromagnetic momentum density. When Eq. 3.95 is inserted into Eq. 3.94, the term with $\partial_j T^{ij}$ vanishes on account of Gauss's theorem and Eq. 3.94 reduces to[88]

$$\mathbf{F}_{\text{self}} = -\int \frac{\partial \mathbf{g}}{\partial t}\, d^3x = -\frac{d}{dt}\int \mathbf{g}\ d^3x = -\frac{d\mathbf{G}}{dt}. \tag{3.96}$$

The total force on the electron will be the sum of the force due to the electron's self-field and any external forces exerted on the electron:

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[87] I am indebted to A. J. Kox for emphasizing (in private conversations) Lorentz's early reservations about program of Wien and Abraham to develop a purely electromagnetic world view. Kox's assessment is at odds with Miller's. According to Miller (1973, p. 314), Lorentz only started to have doubts about the electromagnetic world view in 1921.

[88] Cf. the derivation of $\mathbf{T}_{\text{self}} = -\mathbf{v} \times \mathbf{G}$ from $\mathbf{T}_{\text{self}} = \int \mathbf{x} \times \mathbf{f}\, d^3x$ (see section 1.4, Eq. 1.43–1.62).

$$\mathbf{F}_{\text{tot}} = \mathbf{F}_{\text{ext}} + \mathbf{F}_{\text{self}} = \mathbf{F}_{\text{ext}} - \frac{d\mathbf{G}}{dt}. \tag{3.97}$$

According to Newton's second law, the total force can be written as

$$\mathbf{F}_{\text{tot}} = \frac{d\mathbf{p}}{dt} = m^{\text{N}}\mathbf{a}, \tag{3.98}$$

where the superscript 'N' on the mass $m$ is to indicate that it is the Newtonian mass, the "true" or "material" mass as Lorentz (1904b, p. 185) called it. This mass has to be distinguished from the electromagnetic mass $m^{\text{EM}}$, the inertia of the electron due to the interaction with its self-field. $\mathbf{F}_{\text{self}}$ can not simply be written as $m^{\text{EM}}\mathbf{a}$. The electromagnetic inertia for accelerations in the direction of the velocity is different from the inertia for accelerations perpendicular to the direction of the velocity.

As we will see shortly, the electromagnetic momentum of the electron (no matter whether we consider the model of Lorentz, Abraham, or that of Bucherer and Langevin) has the form:[89]

$$\mathbf{G} = G(v)\,\frac{\mathbf{v}}{v} \tag{3.99}$$

Differentiating this expression with respect to time, we find:

$$\frac{d\mathbf{G}}{dt} = \frac{dG}{dv}\frac{dv}{dt}\frac{\mathbf{v}}{v} + G\,\frac{d}{dt}\left(\frac{\mathbf{v}}{v}\right) = \frac{dG}{dv}\,\mathbf{a}_{//} + \frac{G}{v}\,\mathbf{a}_{\perp}. \tag{3.100}$$

From this equation, we read off the following basic equations for the electron's electromagnetic longitudinal mass $m_{\text{L}}^{\text{EM}}$ and its electromagnetic transverse mass $m_{\text{T}}^{\text{EM}}$, the electron's inertia for accelerations parallel to $\mathbf{v}$ and perpendicular to $\mathbf{v}$, respectively:

$$m_{\text{L}}^{\text{EM}} = \frac{dG}{dv}, \qquad m_{\text{T}}^{\text{EM}} = \frac{G}{v}. \tag{3.101}$$

With the help of Eqs. 3.98–3.101, Eq, 3.97 can be rewritten as:

$$\mathbf{F}_{\text{ext}} = \left(m^{\text{N}} + m_{\text{L}}^{\text{EM}}\right)\mathbf{a}_{//} + \left(m^{\text{N}} + m_{\text{T}}^{\text{EM}}\right)\mathbf{a}_{\perp}. \tag{3.102}$$

There is an alternative way of deriving an expression for the electromagnetic longitudinal mass $m_{\text{L}}^{\text{EM}}$. If an electron travels a distance $ds$ through the ether, the force the electron experiences from its self-field does a certain amount of work $dW$, unless the electron's motion is uniform. Using Eq. 3.96 and Eqs. 3.100–3.101, $dW$ can be written as

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[89] In general, i.e., for arbitrary charge distributions, the electromagnetic momentum (in what is, in effect, what I called the Laue definition in chapter two) will not have this simple form. The charge distribution on the condenser in the Trouton-Noble experiment provides a case in point (cf. section 2.3, Eq. 2.107)

$$dW = \mathbf{F}_{\text{self}} \cdot \; d\mathbf{s} \; = -\frac{d\mathbf{G}}{dt} \cdot \; d\mathbf{s} \; = -\, m_{\text{L}}^{\text{EM}} \frac{dv}{dt}\, ds \; . \tag{3.103}$$

Setting $ds/dt$ equal to $v$ and noticing that the work $dW$ will be minus the change in electromagnetic energy $U_{\text{EM}}$, we can write

$$m_{\text{L}}^{\text{EM}} = \frac{1}{v} \frac{dU_{\text{EM}}}{dv}. \tag{3.104}$$

In order to calculate the electromagnetic mass for the models of Abraham, Lorentz, and Bucherer-Langevin, we need to calculate the momentum and/or the energy of the electromagnetic fields generated by the charge distributions of the electrons in these different models. The energy and momentum of these fields are calculated in the same way as the energy and momentum of the fields of the moving condenser in the Trouton-Noble experiment, i.e., with the help of Lorentz's theorem of corresponding states (cf. section 1.4, Eqs. 1.63–1.73).

The electromagnetic momentum **G** is defined as (see, e.g., 1.35):

$$\mathbf{G} \; = \int \varepsilon_0 \, \mathbf{E} \times \mathbf{B} \;\; d^3x \; . \tag{3.105}$$

Since we are dealing with a static charge distribution, the volume element can be written as (cf. section 2.1, Eqs. 2.29–2.34 and Fig. 2.3, and Eq. 3.54)

$$d^3x = \frac{1}{\gamma l^3}\, d^3x'. \tag{3.106}$$

With the help of Eq. 3.55, the fields **E** and **B** can be expressed in terms of **E**′ and **B**′:

$$\begin{aligned} \mathbf{E} &= \text{diag}(l^2,\, l^2\gamma,\, l^2\gamma)\left(\mathbf{E}' - \mathbf{v} \times \mathbf{B}'\right), \\ \mathbf{B} &= \text{diag}(l^2,\, l^2\gamma,\, l^2\gamma)\left(\mathbf{B}' + \frac{1}{c^2}\, \mathbf{v} \times \mathbf{E}'\right). \end{aligned} \tag{3.107}$$

Since $\mathbf{B}' = 0$, and since it is always possible to choose our coordinate system such that $\mathbf{v} = (v, 0, 0)$, these equations reduce to

$$\mathbf{E} = l^2 \left(E'_x \,,\, \gamma\, E'_y,\, \gamma\, E'_z\right), \qquad \mathbf{B} \;\; = \frac{l^2 \gamma v}{c^2} \left(0 \,,\, -E'_z,\, E'_y\right). \tag{3.108}$$

So, for the cross product of **E** and **B**, we find:

$$\mathbf{E} \times \mathbf{B} = \begin{pmatrix} E_y B_z - E_z B_y \\ -E_x B_z \\ E_x B_y \end{pmatrix} = \frac{l^4 \gamma v}{c^2} \begin{pmatrix} \gamma E'^2_y + \gamma E'^2_z \\ -E'_x E'_y \\ -E'_x E'_z \end{pmatrix}. \tag{3.109}$$

The space integrals over the $y$- and $z$-components of $\mathbf{E} \times \mathbf{B}$ vanish because of the symmetry of the electron charge distributions. In all three models under consideration, the corresponding state of an electron in uniform motion through the ether is either a sphere (Lorentz, Bucherer-Langevin) or an ellipsoid of revolution around an axis parallel to $\mathbf{v}$ (Abraham). Choose a coordinate system with its $x$-axis in the direction of $\mathbf{v}$ and its origin at the center of the electron. The field of the electron will be invariant under reflection in the $xy$-, the $xz$-, and the $yz$-plane. Focus on reflection in the $yz$-plane. For arbitrary values of $a$, the value of $E'_x E'_y$ at $x = a$ in the integrand of the expression for $G_y$ will be minus the value at $x = -a$. This means that $G_y = 0$. A fully analogous argument shows that $G_z = 0$ as well. This proves the claim I made earlier (see Eq. 3.99) that the electromagnetic momentum $\mathbf{G}$ of the electron always has the direction of the electron's velocity $\mathbf{v}$. More specifically, $\mathbf{G}$ is given by

$$\mathbf{G} = \mathbf{v}\, \frac{\varepsilon_0 l \gamma}{c^2} \int E'^2_y + E'^2_z \, d^3x'. \tag{3.110}$$

This equation holds for all three electron models under consideration. I will only do the actual integration for a contractile electron, such as the Lorentz electron and the Bucherer-Langevin electron. In a moving frame a contractile electron has the shape of an ellipsoid, but its corresponding state at rest in the ether will always be spherical. In Abraham's model, a moving electron retains its spherical shape, so its corresponding state at rest in the ether has the shape of an ellipsoid. That makes it harder to perform the integration in Eq. 3.110. For a contractile electron, we can simply write:

$$\int E'^2_y + E'^2_z \, d^3x' = \frac{2}{3} \int E'^2 \, d^3x' = \frac{4}{3\varepsilon_0} U'_{\mathrm{EM}}, \tag{3.111}$$

where in the last step I used the equation $\frac{1}{2}\varepsilon_0 E'^2$ for the energy density of an electric field (see, e.g., section 2.1, Eq. 2.3), and where $U'_{\mathrm{EM}}$ is the electromagnetic energy of the electron at rest in the ether. Inserting Eq. 3.111 into Eq. 3.110, we find that the electromagnetic momentum of a contractile electron is given by

$$\mathbf{G} = \gamma\, l\, m'_{\mathrm{EM}}\, \mathbf{v}, \tag{3.112}$$

where $m'_{\mathrm{EM}}$—which, as the notation suggests, will be interpreted as the electromagnetic mass of the electron at rest in the ether—is defined as[90]

$$m'_{\mathrm{EM}} \equiv \frac{4}{3}\frac{U'_{\mathrm{EM}}}{c^2}. \tag{3.113}$$

Inserting 3.112 for **G** into Eq. 3.101, we can derive expressions for the longitudinal and transversal electromagnetic mass of a contractile electron. The transverse mass $m_{\mathrm{T}}^{\mathrm{EM}}$ is simply given by

$$m_{\mathrm{T}}^{\mathrm{EM}} = \frac{G}{v} = \gamma\, l\, m'_{\mathrm{EM}} \tag{3.114}$$

The longitudinal mass $m_{\mathrm{L}}^{\mathrm{EM}}$ takes a little more work:

$$m_{\mathrm{L}}^{\mathrm{EM}} = \frac{dG}{dv} = \frac{d}{dv}(\gamma\, v)\, l\, m'_{\mathrm{EM}} + \gamma\, \mathrm{v}\, \frac{dl}{dv}\, m'_{\mathrm{EM}}. \tag{3.115}$$

Using that $\frac{d}{dv}(\gamma\, v) = \gamma^3$,[91] this equation can be written as

$$m_{\mathrm{L}}^{\mathrm{EM}} = \gamma^3\, l\, m'_{\mathrm{EM}} + \gamma\, \mathrm{v}\, \frac{dl}{dv}\, m'_{\mathrm{EM}}. \tag{3.116}$$

With the help of Eq. 3.114 and Eq. 3.116, we can write for the total longitudinal and transverse masses of a contractile electron:

$$\begin{aligned} m_{\mathrm{L}} &= m^{\mathrm{N}} + \gamma^3\, l\, m'_{\mathrm{EM}} + \gamma\, \mathrm{v}\, \frac{dl}{dv}\, m'_{\mathrm{EM}}, \\ m_{\mathrm{T}} &= m^{\mathrm{N}} + \gamma\, l\, m'_{\mathrm{EM}}. \end{aligned} \tag{3.117}$$

Lorentz was looking for an electron model that would satisfy the following relations required by his generalized contraction hypothesis (cf. Eq. 3.93)

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[90] The reader will recognize the '$^4/_3$-puzzle' discussed in section 2.2.

[91] This can be seen as follows: $\frac{d}{dv}(\gamma\, v) = \frac{d}{d\beta}(\gamma\, \beta) = \gamma + \beta\, \frac{d\gamma}{d\beta}$. Using $\gamma = \gamma^3(1-\beta^2)$ in the first term and $\frac{d\gamma}{d\beta} = \beta\gamma^3$ in the second, one sees that this is indeed equal to $\gamma^3$.

$$m_{\mathrm{L}} = m^{\mathrm{N}} + m_{\mathrm{L}}^{\mathrm{EM}} = \gamma^3 l \left(m^{\mathrm{N}} + m'_{\mathrm{EM}}\right),$$
$$m_{\mathrm{T}} = m^{\mathrm{N}} + m_{\mathrm{T}}^{\mathrm{EM}} = \gamma l \left(m^{\mathrm{N}} + m'_{\mathrm{EM}}\right). \tag{3.118}$$

Eq. 3.117 will only be of the form of Eq. 3.118, if the factor $l$ is set to unity,[92] and if we assume that the mass of the electron is purely electromagnetic and does not have a Newtonian component (see Lorentz 1904b, section 9, pp. 184–185):

$$l = 1, \qquad m^{\mathrm{N}} = 0. \tag{3.119}$$

Eq. 3.117 then reduces to

$$m_{\mathrm{L}} = \gamma^3 m'_{\mathrm{EM}}, \qquad m_{\mathrm{T}} = \gamma\, m'_{\mathrm{EM}}. \tag{3.120}$$

Both assumptions in Eq. 3.119 were quite welcome. The first assumption fulfilled Lorentz's hope to determine the arbitrary factor $l$ in his theorem of corresponding states (Lorentz 1899a, p. 522; 1899b, p. 270). With hindsight, this is, of course, a rather round-about way to determine $l$, but it was the only way known to Lorentz in 1904. Einstein (1905) and Poincaré (1905, 1906) would show that the condition $l = 1$ is necessary if we want the transformation in Eqs. 3.55–3.54 to be reciprocal, another necessary condition the ether theory would have to satisfy if it is to predict in all generality that we can never determine our velocity with respect to the ether. The second assumption, i.e., that the mass of the electron is purely electromagnetic, fitted nicely with Wien and Abraham's electromagnetic view of nature.

**3.4.3 Abraham, Poincaré, Bucherer, Langevin, and the ambiguity in the expression for the longitudinal mass of Lorentz's electron model.** Unfortunately, Lorentz's assumption that $l = 1$ turned out to be incompatible with the electromagnetic view of nature. The origin of the problem, as Abraham pointed out (1904, 1905; see Miller 1981, pp. 75–79), is that the electromagnetic longitudinal mass of a contractile electron with $l = 1$ is different depending on whether we calculate it from the electron's electromagnetic momentum (using Eq. 3.101) or from its electromagnetic energy (using Eq. 3.104).

The electromagnetic energy of the electron can be found in exactly the same way as it electromagnetic momentum (cf. Eqs. 3.105–3.112). The energy $U_{\mathrm{EM}}$ of an arbitrary electromagnetic field is given by

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[92] Eq. 3.117 will only have the form of Eq. 3.118 if $dl/dv = 0$. Since $l(v)$ can only deviate from 1 by something in the order of $v^2/c^2$, this means that $l = 1$.

$$U_{\mathrm{EM}} = \int \tfrac{1}{2}\,\varepsilon_0\,E^2 + \tfrac{1}{2}\,\mu_0^{-1}\,B^2\,d^3x. \tag{3.121}$$

Using Eqs. 3.108 to express the fields **E** and **B** in terms of **E**′, and Eq. 3.106 for the volume element $d^3x$, we can rewrite this equation as

$$U_{\mathrm{EM}} = \int \tfrac{1}{2}\,\varepsilon_0\,l^4\left(E'^2_x + \gamma^2 E'^2_y + \gamma^2 E'^2_z\right) + \tfrac{1}{2}\,\mu_0^{-1}\,\frac{v^2}{c^4}\,l^4\left(\gamma^2 E'^2_z + \gamma^2 E'^2_y\right)\frac{1}{l^4\gamma}\,d^3x'. \tag{3.122}$$

With $\mu_0^{-1}/c^2 = \varepsilon_0$ and $\beta = v/c$, Eq. 3.122 simplifies to

$$U_{\mathrm{EM}} = \tfrac{1}{2}\,\varepsilon_0\,\frac{l}{\gamma}\int E'^2_x + \gamma^2(1+\beta^2)E'^2_y + \gamma^2(1+\beta^2)E'^2_z\,d^3x'. \tag{3.123}$$

This equation holds for all three electron models under consideration. However, I will only evaluate the integral for a contractile electron (Lorentz, Bucherer-Langevin), in which case the corresponding state always has a spherical shape. So, for a contractile electron, we can write:

$$\int \tfrac{1}{2}\,\varepsilon_0\,E'^2_x\,d^3x' = \int \tfrac{1}{2}\,\varepsilon_0\,E'^2_y\,d^3x' = \int \tfrac{1}{2}\,\varepsilon_0\,E'^2_z\,d^3x' = \tfrac{1}{3}\,U'_{\mathrm{EM}}, \tag{3.124}$$

where $U'_{\mathrm{EM}}$ is the electromagnetic energy of the electron at rest in the ether. Inserting Eq. 3.124 into Eq. 3.123, we find[93]

$$U_{\mathrm{EM}} = \tfrac{1}{3}\,U'_{\mathrm{EM}}\,\frac{l}{\gamma}\left(1 + 2\gamma^2(1+\beta^2)\right). \tag{3.125}$$

It will be convenient to eliminate $\beta^2$ from this equation with the help of the relation $\beta^2 = (\gamma^2 - 1)/\gamma^2$. After some rearrangement of terms, Eq. 3.125 then turns into

---

[93] Notice that Eq. 3.125 can be rewritten as

$$\begin{aligned} U_{\mathrm{EM}} &= \tfrac{1}{3}\,U'_{\mathrm{EM}}\,\frac{l}{\gamma}\left(\gamma^2(1-\beta^2) + 2\gamma^2(1+\beta^2)\right) \\ &= \tfrac{1}{3}\,U'_{\mathrm{EM}}\,l\gamma\left(3+\beta^2)\right) \\ &= U'_{\mathrm{EM}}\,l\gamma\left(1 + \tfrac{1}{3}\beta^2)\right). \end{aligned}$$

From this equation, one immediately sees that, to first order in $\beta$, $U_{\mathrm{EM}} = U'_{\mathrm{EM}}$, as in the case of the Trouton-Noble condenser (see section 2.4, Eq. 2.132).

$$U_{\mathrm{EM}} = \frac{1}{3} U'_{\mathrm{EM}} \frac{l}{\gamma}\left(1 + 2\gamma^2\left(2 - \frac{1}{\gamma^2}\right)\right)$$
$$= \frac{4}{3}\gamma\, l\, U'_{\mathrm{EM}} - \frac{1}{3}\frac{l}{\gamma} U'_{\mathrm{EM}}. \tag{3.126}$$

Inserting Eq. 3.126 into Eq. 3.104, we can compute the electron's electromagnetic longitudinal mass $m_{\mathrm{L}}^{\mathrm{EM}}$. If we do this for the Lorentz electron (i.e., if we set $l = 1$). it turns out that the contribution to $m_{\mathrm{L}}^{\mathrm{EM}}$ coming from the first term in Eq. 3.126 already gives the full expression for $m_{\mathrm{L}}^{\mathrm{EM}}$ calculated from the electron's electromagnetic momentum. Substituting $m'_{\mathrm{EM}}\, c^2$ for $4/3\, U'_{\mathrm{EM}}$ (Eq. 3.113) and $l = 1$ into the first term of Eq. 3.126, we find that

$$\frac{1}{v}\frac{d}{dv}\left(\gamma\, m'_{\mathrm{EM}}\, c^2\right) = \frac{1}{v} m'_{\mathrm{EM}}\, c^2 \frac{d\gamma}{dv} = \gamma^3\, m'_{\mathrm{EM}}, \tag{3.127}$$

where I used that $d\gamma/dv = (v/c^2)\,\gamma^3$, as one easily verifies. From Eqs. 3.126–3.127, it follows that

$$\frac{1}{v}\frac{dU_{\mathrm{EM}}}{dv} \neq \gamma^3\, m'_{\mathrm{EM}}. \tag{3.128}$$

In other words, the longitudinal mass calculated from the electron's electromagnetic energy is not the same as the electromagnetic mass calculated from the electron's electromagnetic momentum in Lorentz's theory (see Eq. 3.116 for $l = 1$). As it stands, Lorentz's electron model is inconsistent. In section 2.2, I already indicated how Poincaré (1905, 1906) would solve the problem.[94] Poincaré introduced the notion of ether pressure as a mechanism for stabilizing Lorentz's electron. I will not attempt to prove this here, but the ether pressure gives a contribution $U'_{\mathrm{EM}}/3\gamma$ to the energy of Lorentz's electron. This cancels the second term in Eq. 3.126 for $U_{\mathrm{EM}}$ . As we just saw, the first term in Eq. 3.126 for $U_{\mathrm{EM}}$ does give the same longitudinal mass as the electromagnetic momentum **G** in Eq. 3.112 for $l = 1$.

Abraham objected to this solution of the problem for it was at odds with his electromagnetic view of nature. It showed that Lorentz's electron could not be of purely electromagnetic origin. At first sight, it looks as if Abraham's own theory faces the exact same problem as Lorentz's. No matter whether the charge distribution representing the electron is a rigid sphere, as Abraham thought, or a sphere subject to the Lorentz-FitzGerald contraction, as Lorentz thought, it would seem that non-electromagnetic forces are needed to prevent the charge distribution from flying apart under the influence of their Coulomb repulsion. This impression is based on

[94] Poincaré had discovered the problem and had brought it to Lorentz's attention independently of Abraham (Miller 1981, p. 79).

an anachronistic view of Abraham's theory. For him the rigid spherical electron was a primitive notion. What ensured the electron's stability were not forces balancing the Coulomb forces, but 'rigid constraints' in the sense of Hertz's mechanics (Miller 1981, p. 56). For our purposes, we can think of such rigid constraints as forces of infinite magnitude so that the electron can never change its shape. This gives a perfectly consistent theory, as is illustrated by the fact that in this theory we steer clear of the problem we just encountered in Lorentz's theory. In Abraham's theory, the electromagnetic energy and the electromagnetic momentum of the electron give the same longitudinal mass without adding any extra pieces to the theory. The crucial difference between Hertz's rigid constraint mechanism stabilizing Abraham's rigid electron and Poincaré's ether pressure mechanism stabilizing Lorentz's deformable electron is that the infinite forces in the former case never do work while the finite forces in the latter case do, viz. every time the electron changes its velocity and thereby its volume.[95]

That the ambiguity in the expression for the longitudinal mass of Lorentz's electron does indeed have its origin in the energy conversions that accompany the changes of the electron's volume when it is being accelerated can be seen from the fact that the Bucherer-Langevin electron, which always retains its volume, does not suffer from this ambiguity. In the case of the Bucherer-Langevin electron, as in the case of the Abraham electron, the electromagnetic momentum gives the same longitudinal mass as the electromagnetic energy.

Inserting $l = \gamma^{-1/3}$ in Eq. 3.116, we can find $m_{\mathrm{L}}^{\mathrm{EM}}$ for the Bucherer-Langevin electron through its electromagnetic momentum:

$$m_{\mathrm{L}}^{\mathrm{EM}} = m'_{\mathrm{EM}} \left( \gamma^{8/3} + \gamma\, v\, \frac{d\gamma^{-1/3}}{dv} \right). \tag{3.129}$$

Consider the last term separately:

$$\frac{d\gamma^{-1/3}}{dv} = -\frac{1}{3}\gamma^{-4/3}\frac{d\gamma}{dv} = -\frac{1}{3}\frac{v}{c^2}\gamma^{5/3}, \tag{3.130}$$

where I used that $d\gamma/dv = (v/c^2)\,\gamma^3$. Inserting Eq. 3.130 into Eq. 3.129, we find

$$m_{\mathrm{L}}^{\mathrm{EM}} = m'_{\mathrm{EM}}\,\gamma^{8/3}\left(1 - \frac{1}{3}\beta^2\right). \tag{3.131}$$

Inserting $l = \gamma^{-1/3}$ and $^4/_3\, U'_{\mathrm{EM}} = m'_{\mathrm{EM}} c^2$ into Eq. 3.126 and substituting the result into Eq. 3.104, we can find $m_{\mathrm{L}}^{\mathrm{EM}}$ for the Bucherer-Langevin electron through its electromagnetic energy:

---

[95] From a modern relativistic point of view, the "work" done in changing the shape of the electron is a purely kinematical effect. It can be defined away in the same way we defined away the turning couples in the Trouton-Noble experiment using the Rohrlich definition of four-momentum.

$$
\begin{aligned}
m_{\mathrm{L}}^{\mathrm{EM}} &= \frac{1}{v}\, m'_{\mathrm{EM}}\, c^2 \frac{d}{d\gamma}\left(\gamma^{2/3} - \frac{1}{4}\gamma^{-4/3}\right)\frac{d\gamma}{dv} \\
&= m'_{\mathrm{EM}}\, \frac{c^2}{v}\left(\frac{2}{3}\gamma^{-1/3} + \frac{1}{3}\gamma^{-7/3}\right)\frac{v}{c^2}\gamma^3 \qquad (3.132) \\
&= m'_{\mathrm{EM}}\, \gamma^{8/3}\left(\frac{2}{3} + \frac{1}{3}\gamma^{-2}\right).
\end{aligned}
$$

Using $\gamma^{-2} = 1 - \beta^2$ , we can rewrite this equation as

$$
m_{\mathrm{L}}^{\mathrm{EM}} = m'_{\mathrm{EM}}\, \gamma^{8/3}\left(1 - \frac{1}{3}\beta^2\right). \qquad (3.133)
$$

Eq. 3.133 is the same as Eq. 3.131: the energy and momentum of the Bucherer-Langevin electron give the same longitudinal mass.

**3.4.4 Choosing between the electromagnetic view of nature and the principle of relativity: Planck, Lorentz, and Sommerfeld.** So, here we have the dilemma I mentioned at the beginning of this section. The Lorentz model of the electron as amended by Poincaré has the properties required by Lorentz's generalized contraction hypothesis and is thus compatible with the absence of any signs of ether drift. However, because of the Poincaré stresses needed to ensure its stability and to remove the ambiguity in the expression for its longitudinal mass, Lorentz's model is not purely electromagnetic and hence incompatible with the electromagnetic view of nature. The Abraham and Bucherer-Langevin models are consistent without the addition of any non-electromagnetic pieces and hence compatible with the electromagnetic view of nature. However, these models do not have the properties required by Lorentz's generalized contraction hypothesis and hence are incompatible with the absence of any signs of ether drift.

A clear statement of the dilemma was given by Planck in the discussion following his lecture entitled "The Kaufmann measurements of the deflectability of β-rays and their relevance for the dynamics of electrons," delivered at the *Versammlung Deutscher Naturforscher und Ärzte* in Stuttgart on September 19, 1906 (see Miller 1981, pp. 232–235; McCormmach 1970b, pp. 489–490; Jungnickel and McCormmach 1986, pp. 249–250). Kaufmann, Abraham, Bucherer,[96] and Sommerfeld took part in the discussion following Planck's lecture. At one point, Planck remarked:

> Abraham is right when he says that the essential advantage of the sphere theory [i.e., Abraham's theory of the electron] would be that it be a purely electrical [sic] theory. If this were feasible, it would be very beautiful indeed, but for the time being it is just a postulate.

[96] Understandably, Bucherer took exception to the fact that Planck only discussed the electron models of Lorentz and Abraham (Planck 1906, p. 760).

> At the basis of the Lorentz-Einstein theory [!] lies another postulate, namely that no absolute translation can be detected. These two postulates, it seems to me, can not be combined, and what it comes down to is which postulate one prefers. My sympathies actually lie with the Lorentzian postulate. (Planck 1906, p. 761)

In response, Sommerfeld quipped:[97]

> I suspect that the gentlemen under forty will prefer the electrodynamical postulate, while those over forty will prefer the mechanical-relativistic postulate. I prefer the electrodynamical one. (Ibid.)

The transcript in Planck 1906 of the discussion in Stuttgart also preserved the reaction of the assembled physicists to Sommerfeld's quick retort: "laughter" (*Heiterkeit*, Ibid.)

To conclude this section, I want to quote and briefly discuss a passage from Lorentz's lectures given in New York in March and April of 1906, a passage that clearly shows Lorentz's response to the dilemma outlined above:

> Abraham [1904] has raised the objection that I had not shown that the electron, when deformed to an ellipsoid by its translation, would be in stable equilibrium. This is certainly true, but I think the hypothesis need not be discarded for *this* reason. The argument proves only that the electromagnetic actions and the [Poincaré] stress of which we have spoken cannot be the only forces which determine the configuration of the electron.[98] [...]
>
> Notwithstanding all this, it would, in my opinion, be quite legitimate to maintain the hypothesis of the contracting electrons, if by its means we could really make some progress in the understanding of phenomena. In speculating on the structure of these minute particles we must not forget that there may be many possibilities not dreamt of at present; it may very well be that other internal forces serve to ensure the stability of the system, and perhaps, after all, we are wholly on the wrong track when we apply to the parts of an electron our ordinary notion of force. (Lorentz 1916, pp. 214–215; italics in the original)

This passage is from a section entitled "Optical phenomena in moving bodies," and it is safe to assume that the "phenomena" that Lorentz suggests "the hypothesis of the contracting electrons" could help to explain are simply the negative results of a wide variety of optical ether drift experiments. These comments from 1906 testify to Lorentz's reservations with respect to the electromagnetic view of nature, reservations that, one suspects, are related to his keen awareness of the limitations of his theory due to quantum phenomena.

It remains for me to explain the italicized "this" in the quotation above. The emphasis has to do with Lorentz's acknowledgment, two pages earlier, of the fact that Kaufmann's latest results seem to favor Abraham's electron model over his own:

---

[97] See McCormmach 1970b, p. 490, for a discussion of the development of Sommerfeld's attitudes toward the electromagnetic view of nature and special relativity.

[98] Unless I misunderstand Lorentz's point here, this comment seems to be wrong. The Poincaré stresses do ensure the stability of the electron (see section 2.2). Lorentz is quite right when he point out that there may be other mechanisms serving the same purpose (see section 2.2 and Laue 1911a).

> His [i.e., Kaufmann's] new numbers agree within the limits of experimental errors with the formulae given by Abraham, but [...] are decidedly unfavourable to the idea of a contraction such as I attempted to work out. (Ibid., pp. 212–213)

Shortly before his departure for New York, in a letter to Poincaré dated Leiden, March 8, 1906, Lorentz had written: "Unfortunately my hypothesis of the flattening of electrons is in contradiction with Kaufmann's results, and I must abandon it. I am therefore at the end of my rope (*au bout de mon latin*)" (quoted in Miller 1981, p. 334; the full letter is reproduced in facsimile on pp. 336–337). These passages strongly suggest that Lorentz took Kaufmann's results much more seriously than Einstein. Miller indeed draws that conclusion. A. J. Kox has pointed out to me that Lorentz's reaction was probably more ambivalent. Kox alerted me to a curious aspect of Lorentz's exposition in his 1906 lectures. Immediately after the sentence I just quoted, in which Lorentz acknowledges the discrepancy between his theory and Kaufmann's latest results, he writes:

> Yet, though it seems very likely that we shall have to relinquish this idea altogether, *it is, I think, worth while looking into it somewhat more closely*. (Ibid., p. 213; my italics)

Lorentz then proceeds to discuss his idea *at length*.

No matter how serious Lorentz's worries about Kaufmann's results were in 1906, by 1908 the tide had changed, and the experiments now favored Lorentz's—and Einstein's—prediction for the velocity dependence of the transverse mass of the electron. In the second edition of his New York lectures, Lorentz added the following somewhat triumphant footnote to the sentence citing Kaufmann's results that I quoted above:

> Later experiments [Lorentz cites Bucherer 1908,[99] Guye and Lavanchy 1915, and others] have confirmed [Eq. 3.120] for the transverse electromagnetic mass, so that, in all probability, the only objection that could be raised against the hypothesis of the deformable electron and the principle of relativity has now been removed. (Ibid., p. 339)

As Miller points out, the accuracy of the results of Kaufmann, Bucherer, Guye and Lavanchy, and others measuring the transverse mass of high speed electrons in β-radiation in the first decades of the century was grossly over-estimated at the time. Miller (1981, p. 350) cites two later experimenters, Zahn and Spees (1938), who re-analyzed some of the data obtained in these experiments and concluded that the experiments by Bucherer and others, despite significant improvements in the experimental set-up over the set-up used in Kaufmann's original experiment "proved very little, if anything more than the Kaufmann experiments, which indicated a large qualitative increase of mass with velocity. One might say that their experiments

---

[99] Notice that the title of Bucherer's paper of 1908 is: "Measurements on Becquerel rays. The experimental confirmation of the Lorentz-Einstein *theory*." The title of Guye and Lavanchy's paper of 1915 only refers to the "Lorentz-Einstein *formula* for high speed cathode rays."

gave a slight suggestion in favor of the relativity electron [i.e., Lorentz electron], but that the uncertainties of interpretation are so great as to give one very little feeling of certainty as regards a 10 percent difference" (Zahn and Spees 1938, p. 519). In other words, these experiments were totally unfit for their ostensive purpose of deciding between the various electron models that were being considered at the time.

For the theoretical epilogue to this episode, I want to quote Pais: "Special relativity killed the classical dream of using the energy-momentum-velocity relations of a particle as a means of probing the dynamical origins of its mass. The relations are purely kinematical[100]" (Pais 1982, p. 159). As we saw in chapter two, this was not immediately clear in 1905. It seems to have been clear to Einstein, but not, for instance, to Ehrenfest (see Ehrenfest 1907 and Einstein 1907a). The issue was settled only in 1911, by Laue. As we saw in chapter two, Laue (1911a, p. 153) showed that as long as the electron is a complete or closed static system, its four-momentum will transform as the four-momentum of a relativistic point particle. It immediately follows that the expressions for the longitudinal and transverse mass of a complete static system are the Lorentz-Einstein expressions in Eq. 3.120. We also saw in chapter two that the condition that the system be static can be dropped. So, these expressions would hold for the longitudinal and transverse mass of any closed system, which is just a slightly different way of making the point Pais made in the passage I just quoted.

---

[100] My impression is that Pais uses the phrase "kinematical" here as synonymous with "independent of the details of the dynamics," rather than in the sense I used it in chapter two, which would lead us to read this statement of Pais as "the relations directly reflect the Minkowskian space-time structure." The two readings are, of course, perfectly compatible with one another. The "energy-momentum-velocity relations of a particle" are independent of the details of the dynamics because they directly reflect the Minkowskian space-time structure.

## 3.5 Lorentz's interpretation of the Lorentz transformation formulae after 1905: from mathematical auxiliaries to effective coordinates and fields

**3.5.1 The state of Lorentz's theory in 1905.** I want to give a brief review of the state of Lorentz's theory for the electrodynamics of moving bodies in 1905. As I argued in section 3.3, this part of Lorentz's theory has a twofold basis, a purely mathematical result and a general physical assertion about the effect of ether drift on physical phenomena. The mathematical result is the exact theorem of corresponding states, which is essentially the claim that the source free Maxwell equations are Lorentz invariant. The physical assertion, which I dubbed the generalized contraction hypothesis, is that corresponding states physically transform into one another. With the help of these two elements, it is possible, in principle, to give a general account of the negative result of any conceivable ether drift experiment.

As I also pointed out in section 3.3, Lorentz did not make the generalized contraction hypothesis an axiom of his theory (in the sense that Maxwell's equations, for instance, are). Instead, Lorentz wanted to derive the generalized contraction hypotheses from other more specific assumptions about the effect of ether drift on physical phenomena. Which and how many of these specific assumptions are needed for such a derivation depends on the range of ether drift experiments one wants to account for through the combination of the exact theorem of corresponding states and the generalized contraction hypothesis.[101] If we focus on optical

[101] As Schaffner has emphasized, Lorentz's 1904 theory does not predict in full generality that ether drift can never be detected. Instead, Lorentz's goal was "to show, by means of certain fundamental assumptions, and without neglecting terms of one order of magnitude or another, that *many* electromagnetic actions are *entirely independent* of the motion of the system" (Lorentz 1904b, p. 174; quoted in Schaffner 1974, p. 48; Schaffner's italics). After his exposition of the new theory, Lorentz writes: "It is easily seen that the proposed theory can account for *a large number* of facts" (Lorentz 1904b, p. 189; my emphasis). These statements can be read (as Schaffner suggests) in support of the claim that Lorentz still believed that eventually ether drift would somehow be detected. However, these quotations from Lorentz 1904b do not rule out the possibility that Lorentz already had the view he would ultimately adopt, viz. that no experiment would ever detect ether drift. They should then be read as acknowledgments on Lorentz's part that his theory does not (yet) provide a general explanation of the anticipated negative results in any conceivable ether drift experiment. Recall that Lorentz's general account for the negative results of optical ether drift experiments only works for those experiments that eventually boil down to the observation of some pattern of light and darkness. Lorentz was aware of the fact that this included most but not all optical experiments. In his article for the *Encyklopädie der Mathematischen Wissenschaften* published earlier in 1904, for instance, he devotes two subsections to experiments that do not fall into this category, explicitly identifying them as such: "We now continue with the discussion of certain experiments that no longer only involve the distribution of light and darkness" (Lorentz 1904a, pp. 268–269). Lorentz discusses measurements of intensities and of radiation pressure in this context. Using the (first order) theorem of corresponding states, he shows that motion through the ether does not affect intensities (to first order in $v/c$). Hence, first order ether drift experiments involving measurements of intensities will always give negative results (ibid., p. 269). First order ether drift experiments involving measurement of radiation pressure, however, should in general give small positive results (ibid., pp. 270–271). Given that Lorentz's theory did not include $E = mc^2$ at this point, this should not come as a surprise (cf. section 2.5).

ether drift experiments that eventually boil down to the observation of some pattern of light and darkness, two assumptions will do.[102]

> (1) All forces satisfy the relation $\mathbf{F} = \mathrm{diag}(1, 1/\gamma, 1/\gamma)\,\mathbf{F}_0$ (cf. Eq. 3.63 for $l = 1$), where $\mathbf{F}_0$ is a force in a system in some state at rest in the ether and $\mathbf{F}$ is the corresponding force in the corresponding state of the system moving through the ether at a velocity $\mathbf{v}$.
>
> (2) The mass of any physical object satisfies the relation $m = \mathrm{diag}(\gamma^3, \gamma, \gamma)\, m_0$ (see Eq. 3.92 for $l = 1$), where $m_0$ is the mass of the object at rest in the ether and $m$ is the mass of the same object moving through the ether at a velocity $\mathbf{v}$.

In support of assumption (1), Lorentz could point to the fact that the relation can actually be derived from Maxwell's equations in the case of Coulomb forces. Although Lorentz did not draw attention to this, the assumption also receives support from the negative result of the Trouton-Noble experiment which directly indicates that ether drift has the same effect on the electromagnetic and the non-electromagnetic forces on a condenser.

Similarly, in support of assumption (2), Lorentz could point to the fact that the relation can actually be derived from Maxwell's equation in the case of his specific model for the electron, a spherical charge distribution subject to the Lorentz-FitzGerald contraction.[103]

The support that assumption (2) derives from Lorentz's model of the electron is clearly weakened by the discovery of Abraham and Poincaré that the model is inconsistent without the addition of some extra structure of non-electromagnetic origin. It is easy to see how this undercuts the justification of Lorentz's assumption (2). What is needed are arguments to establish that masses and forces satisfy the general expressions $m = \mathrm{diag}(\gamma^3, \gamma, \gamma)\, m_0$ and $\mathbf{F} = \mathrm{diag}(1, 1/\gamma, 1/\gamma)\,\mathbf{F}_0$. One such argument is to show that *for some special cases* these expressions can actually be derived from electrodynamics. As long as it is a live possibility that all of physics will ultimately be reduced to electrodynamics, such derivations hold out the promise that, in the end, they will work *for all cases*. However, the moment we give up the notion of a purely electromagnetic physics (and the inconsistency discovered by Abraham and Poincaré did not leave Lorentz much choice in the matter), these calculations in electrodynamics will never be more than plausibility arguments for the assumption that the relevant expressions hold in general, i.e., not only for electromagnetic but also for non-electromagnetic masses and forces.

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[102] Cf. Lorentz 1904b, p. 183, p. 191; 1916, section 175, p. 205.

[103] Whereas the contraction of macroscopic objects is derived from other assumptions in Lorentz's theory, the contraction of individual electrons is one of its primitive axioms

To be sure, Lorentz may have looked upon these derivations as mere plausibility arguments all along (cf. his plausibility argument for the original contraction hypothesis). For Lorentz, the ultimate justification for his assumptions about mass and force seems to have come from the fact that they allow the derivation of the generalized contraction hypothesis in the relevant context, which, in turn, is extremely well confirmed empirically through the negative results of numerous ether drift experiments.

From assumptions (1) and (2) and the extra assumption that both the motion of oscillating electrons generating light and the motion of atoms and molecules of solids around their equilibrium positions are very slow compared to the velocity of light, we can derive the only two results needed to account for the negative result of optical ether drift experiment that eventually boil down to the observation of some pattern of light and darkness:

> (a) The material components in an optical experiment are subject to the same contraction as the patterns of light and darkness in those experiments (the latter contraction, unlike the former, can be derived from the theorem of corresponding states).
>
> (b) The frequency of the light emitted by a moving source is a factor $\gamma$ lower than the frequency emitted by that same source at rest in the ether, in agreement with the relation between the frequencies in two corresponding patterns of light and darkness that can be derived from the theorem of corresponding states.

**3.5.2 Grünbaum's "doubly amended theory" as a model for Lorentz's mature theory.** Notice that, as long as we are dealing with observations of patterns of light and darkness (as in most optical ether drift experiments), we can use Grünbaum's "doubly amended theory" (Grünbaum 1973, p. 723) as a simplified model for Lorentz's mature theory. Grünbaum's two amendments are the Lorentz-FitzGerald contraction hypothesis and the so-called clock retardation hypothesis. These two amendments are easily seen to correspond to (a) and (b) above. From a logical point of view, it does not make much of a difference whether we supplement the theorem of corresponding states with assumptions (a) and (b), or with assumptions (1) and (2), from which (a) and (b) are then derived.

Zahar based his discussion of Lorentz's theory on Grünbaum's simplified model. Zahar identified the unassailable "hard core" of Lorentz's "research programme" as consisting of "Maxwell's equations [...], of Newton's laws of motion and of the Galilean transformation, to which Lorentz added his equation [...] for the so-called Lorentz force" (Zahar 1973, p. 215). I would like to add the basic elements of Lorentz's ontology, the stationary ether and the

electrons, the sole mediators between ether and ponderable matter.[104] Zahar then identifies "three consecutive theories belonging to Lorentz's ether programme" (ibid., p. 216):

> $\mathbf{T}_1$ consists of the hard core as defined above together with the (tacit!) assumptions (*i*) that moving clocks are not retarded and (*ii*) that material rods are not shortened by their motion through the ether.
>
> $\mathbf{T}_2$ is obtained from $\mathbf{T}_1$ by substituting the LFC [Lorentz-FitzGerald contraction] for assumption (*ii*). According to the LFC a body moving through the ether with velocity **v** is shortened by the factor $(1 - v^2/c^2)^{1/2}$.
>
> $\mathbf{T}_3$ is the conjunction of the hard core, of the LFC and of the assumption that, contrary to (i), clock moving with a velocity **v** are retarded by the factor $(1 - v^2/c^2)^{1/2}$. (Zahar 1973, p. 216)

One of Zahar's main theses is that "the shift from $\mathbf{T}_1$ to $\mathbf{T}_2$ and that from $\mathbf{T}_2$ to $\mathbf{T}_3$ were non *ad hoc*" (ibid.). When we bear in mind that $\mathbf{T}_3$ is just a toy model of the 1899–1904 version of Lorentz's theory that I analyzed in great detail above, it is clear that the case for $\mathbf{T}_2 \rightarrow \mathbf{T}_3$ not being *ad hoc* is much stronger than the case for $\mathbf{T}_1 \rightarrow \mathbf{T}_2$. In fact, Miller (1974) and Schaffner (1974) had no trouble demolishing most of the arguments Zahar offers for the latter case (see section 3.2). Not surprisingly, Miller and Schaffner have nothing to say about the $\mathbf{T}_2 \rightarrow \mathbf{T}_3$ case. As I showed in section 3.3, they completely misunderstand the theory modeled by $\mathbf{T}_3$. Unfortunately, Zahar's elaboration of $\mathbf{T}_3$ (Zahar 1973, section 1.5, pp. 231–233) is also unsatisfactory. It would be an acceptable model of Lorentz's theory after 1905, when Lorentz started to give a physical meaning to the auxiliary quantities of his theorem of corresponding states, but it does not capture the 1899–1904 version of the theory.[105] Zahar, for instance, did not identify what I called the generalized contraction hypothesis and also missed the crucial role of the special nature of patterns of light and darkness. However, if Zahar's elaboration of $\mathbf{T}_3$ is replaced by something a little more accurate (something along the lines of my presentation in section 3.4, for instance), it would seem to me that a very strong case can be made that the transition $\mathbf{T}_2 \rightarrow \mathbf{T}_3$ is not *ad hoc* in any of the three senses Zahar distinguishes. In fact, I already showed that it is not *ad hoc*$_1$ nor (allowing enough time) *ad hoc*$_2$. The 1899–1904 version of Lorentz's theory predicts negative results for a broad class of optical ether drift

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[104] This point was also made by Miller (1974, p. 32). For Zahar's response, see Zahar 1978, p. 51.

[105] As an aside, I want to point out that in Zahar's approach, in my opinion, one should distinguish between $\mathbf{T}_3$, the 1899–1904 version of Lorentz's theory, and $\mathbf{T}_4$, the post–1905 version. Equating $\mathbf{T}_3$ and $\mathbf{T}_4$ is too serious a distortion of the historical development, it seems to me, especially because it suppresses the undeniable influence of Einstein's research programme on the heuristics of Lorentz's. Before 1905, Lorentz inferred from the theorem of corresponding states to the undetectability of ether drift appealing to the special nature of patterns of light and darkness. After 1905, he made this inference appealing to the insight he had taken from Einstein, viz. that the auxiliary quantities occurring in the theorem of corresponding states are the measured quantities for the moving observer. As we have seen, this new heuristic was also developed in the context of the ether programme (by Poincaré), but the fact remains that Lorentz directly took it from Einstein.

experiments, and given the empirical success of special relativity we can rest assured that all these predictions would be confirmed if anybody bothered to put them to the test.

After this evaluation of the secondary literature on Lorentz's theory, I return to the evaluation of the theory itself.

**3.5.3 The problem of non-static charge distributions.** Lorentz appears to have been satisfied deriving the generalized contraction hypothesis in the context of optical experiments involving no more than patterns of light and darkness which by their very nature are stationary situations. If we try to generalize the corresponding states technique to non-stationary situations, we run into complications caused by the $x$-dependence of local time. Lorentz had to learn from Einstein how to deal with these complications.

By 1904, Lorentz's theory was nonetheless applicable, at least in principle, to the whole field of electrodynamics, not just to optics and electrostatics. This can be seen from the fact that in the 1904 exposition of the theorem of corresponding states, Lorentz deals with the source terms in Maxwell's equations in their most general form (see Lorentz 1904b, p. 176, Eqs. (7)–(9)). In his exposition of the theorem of corresponding states in the *Versuch* of 1895, Lorentz had simply set the charge and current densities to zero, working only with the source free Maxwell equations (see section 3.1). In the 1899 paper I discussed in section 3.3, he still only considered the charge and current densities for two special cases, electrostatic charge distributions and charge distributions representing oscillating electrons responsible for the emission of light (see section 3.3). In 1904, he finally allowed arbitrary charge and current densities. Why then is it that, even in 1904, the theory's range of application is essentially still restricted to the static and stationary situations encountered in optics and electrostatics?

Before 1905, Lorentz and others tacitly assumed that any problem to be solved by a theory for electrodynamics of moving bodies has the following generic form:

> Given some charge distribution described by the charge and current densities ($\rho$, $\rho\mathbf{u}$) as functions of ($t$, $\mathbf{x}$), the Newtonian time and the coordinates of a Galilean frame of reference $S$ in motion through the ether, find the fields $\mathbf{E}$ and $\mathbf{B}$ generated by that charge distribution as functions of ($t$, $\mathbf{x}$).

Lorentz's theorem of corresponding states provides a general method for solving such problems. Lorentz only applied this method to static or stationary cases in optics and electrostatics. In principle, the method works for arbitrary charge distributions, but, in practice, it is of little help in non-stationary situations. The reason for this is that the relation between corresponding states will be rather complicated in such cases. In order to see how this comes about and in order to appreciate how the problem vanishes into thin air after Lorentz has taken

to heart an important lesson from Einstein, I want to briefly examine the application of Lorentz's strategy for solving problems of the general form stated above.

First of all, the equations for $\mathbf{E}$, $\mathbf{B}$, $\rho$, and $\mathbf{u}$ as functions of $(t, \mathbf{x})$ can be cast into a set of equations very similar to Maxwell's equation by introducing the auxiliary quantities $\mathbf{x}'$, $t'$, $\mathbf{E}'$, $\mathbf{B}'$ (see Eq. 3.54–3.55 for $l = 1$). The auxiliary fields $\mathbf{E}'$ and $\mathbf{B}'$ as functions of $(t', \mathbf{x}')$ satisfy four equations that are very similar to Maxwell's equations (see Eq. 3.56 in section 3.3 with $l = 1$). Two of these equations have exactly the form of the homogeneous Maxwell equations, the other two strongly resemble the inhomogeneous Maxwell equations:

$$\begin{aligned} \operatorname{div}' \mathbf{E}' &= \frac{\rho}{\gamma \varepsilon_0}\left(1 - \gamma^2 \frac{v u_x}{c^2}\right), \\ \operatorname{curl}' \mathbf{B}' &= \mu_0 \operatorname{diag}(\gamma, 1,\ 1)\rho \mathbf{u} + \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}. \end{aligned} \tag{3.134}$$

The equations for $\mathbf{E}'$ and $\mathbf{B}'$ as functions of $(t', \mathbf{x}')$ are solved by considering a corresponding charge distribution in a frame $S_0$ at rest in the ether and solving Maxwell's equations for that charge distribution. Let $(\rho_0, \rho_0\mathbf{u}_0)$ describe he charge and current densities for this corresponding charge distribution as functions of $(t_0, \mathbf{x}_0)$, the Newtonian time and the coordinates of $S_0$. These functions are found as follows. Take the charge and current densities $(\rho, \rho\mathbf{u})$ describing the charge distribution in $S$ as functions of $(t', \mathbf{x}')$, replace $(t', \mathbf{x}')$ by $(t_0, \mathbf{x}_0)$, and substitute the result into the following expressions for $(\rho_0, \rho\mathbf{u}_0)$:

$$\rho_0 \equiv \frac{\rho}{\gamma}\left(1 - \gamma^2 \frac{v u_x}{c^2}\right), \qquad \rho_0\mathbf{u}_0 \equiv \operatorname{diag}(\gamma, 1,\ 1)\rho \mathbf{u}. \tag{3.135}$$

The fields $\mathbf{E}_0$ and $\mathbf{B}_0$ generated by this charge distribution are found by solving Maxwell's equations for $(\rho_0, \rho\mathbf{u}_0)$ in Eq. 3.135. *This is the hard part of the calculation*.

Of course, it took considerable ingenuity on Lorentz's part to find convenient auxiliary quantities $\mathbf{E}'$, $\mathbf{B}'$, $t'$, and $\mathbf{x}'$, but once we have those, and the equations for $\mathbf{E}'$ and $\mathbf{B}'$ as functions of $(t', \mathbf{x}')$ (see Eq. 3.6), solving Maxwell's equations for $(\rho_0, \rho\mathbf{u}_0)$ in Eq. 3.135 is the only non-trivial step in solving any problem in the electrodynamics of moving bodies. The rest of the calculation is completely mechanical and consists of nothing but replacing subscripts '0' with primes and vice versa, and transforming back and forth between primed and unprimed quantities. The final two steps of the calculation illustrate this point.

Once we have $\mathbf{E}_0$ and $\mathbf{B}_0$ as a function of $(t_0, \mathbf{x}_0)$, we can write down $\mathbf{E}'$ and $\mathbf{B}'$ as functions of $(t', \mathbf{x}')$ simply by replacing the subscripts '0' by primes. Finally, using the inverse of the

transformation $(t, \mathbf{x}, \mathbf{E}, \mathbf{B}) \rightarrow (t', \mathbf{x}', \mathbf{E}', \mathbf{B}')$, we can write down **E** and **B** as functions of $(t, \mathbf{x})$, the functions we set out to find.

What makes it hard to solve Maxwell's equations for the charge distribution described by Eq. 3.135 is that the relation between $(\rho_0, \rho_0\mathbf{u}_0)$ as a function of $(t_0, \mathbf{x}_0)$ and $(\rho, \rho\mathbf{u})$ as a function of $(t, \mathbf{x})$ will in general be rather complicated. Consider the interpretation of the expression for $\rho_0$ in Eq. 3.135. If we start from a static distribution in the moving frame (i.e., $\mathbf{u} = 0$), $\rho_0$ just describes the stretched out static distribution Lorentz used in his calculations for electrostatics in moving frames (see Lorentz 1895, p. 37 [cf. Eq. 4.1]; 1899b, p. 260 [cf. Eq. 3.60]; 1904a, pp. 173–175, pp. 256–257). However, for a non-static distribution (i.e., $\mathbf{u} \neq 0$), the interpretation of $\rho_0$ becomes rather obscure. Even the interpretation of the *y*- and *z*-components of $\rho_0\mathbf{u}_0$, which are equal to the *y*- and *z*-components of $\rho\mathbf{u}$ (see Eq. 3.135), gets complicated for such cases. As a direct consequence of the *x*-dependence of local time, the simultaneity relations in corresponding states will be different unless those states are static or at least stationary. Suppose two charges in some non-stationary state in the frame *S* reach the points *P* and *Q* in *S* at the same instance *t* in real Newtonian time. If *P* and *Q* have different *x*-coordinates, the two charges will not reach these points at the same instance in local time. But that means that the two charges in the corresponding state in $S_0$ will not reach the corresponding points $P_0$ and $Q_0$ in $S_0$ at the same instance $t_0$ in real Newtonian time.

The upshot then is that the theorem of corresponding states does not provide a practical method for solving problems involving time-dependent charge and current densities in a charge distribution in motion through the ether. Fortunately, Einstein's work made it clear to Lorentz that the generic problem in the electrodynamics of moving bodies takes on a much more simple and managaeble form than Lorentz had tacitly been assuming in the years prior to 1905.

**3.5.4 Lorentz taking to heart a lesson from Einstein (after ignoring a similar lesson from Poincaré).** In the first edition of his book *The theory of electrons* based on lectures in New York in 1906 and published in 1909 (see section 3.4), Lorentz, for the first time, gave a physical interpretation to the primed quantities in the theorem of corresponding states (Lorentz 1916, sections 189–191, pp. 222–226). Giving full credit to Einstein,[106] Lorentz explained that these quantities are not just mathematical auxiliaries, convenient for carrying out calculations in coordinate systems moving with respect to the ether, but that they actually represent the measured—or "effective" (ibid, p. 223)—quantities for observers moving along with such coordinate systems.

[106] At this point, Lorentz cited almost all the papers of Einstein that I cited so far (Einstein 1905a, 1905b, 1906, 1907b, 1907c) and more.

In the light of recent work by Darrigol (1994b), showing that Poincaré had been using this physical interpretation of the quantities in Lorentz's theorem of corresponding states at least since 1900, it is somewhat puzzling that Lorentz only started pursuing this interpretation after he had read Einstein's work. At this point, I can only speculate what is was about Einstein's work that suddenly made Lorentz see the fruitfulness of a physical interpretation that had been available to him for several years through the work of Poincaré. My guess is that it has to do with the fact that Einstein made the physical interpretation of the Lorentz transformation the basis for a remarkably clear and simple discussion of the electrodynamics of moving bodies, whereas Poincaré's remarks on the physical interpretation of Lorentz transformed quantities may have struck Lorentz as inconsequential philosophical asides in expositions that otherwise closely followed his own. I also have a sense that Lorentz found Einstein's physically very intuitive approach more appealing than Poincaré's rather abstract but mathematically more elegant approach. The basis for this suspicion is that, as we will see below, Lorentz failed to give credit for two important results in *The theory of electrons*, that can be found in Poincaré 1906, viz. the reciprocity of the Lorentz transformation (which follows immediately from the proof that these transformations form a group) and the transformation formulae for charge and current density (which Lorentz only derived in the second edition of his book). I want to emphasize that I do not claim to have a satisfactory explanation for Lorentz's apparent neglect of Poincaré's work in this area. I only want to draw attention to it.[107]

Consider the auxiliary spatial coordinate $x'$ and the local time $t'$ (cf. Eq. 3.54 for $l = 1$):

$$x' = \gamma x, \quad t' = t/\gamma - \gamma(v/c^2)x. \tag{3.136}$$

The physical interpretation of these quantities is as follows (cf. Lorentz 1916, sections 189–190, pp. 223–226; 1922, sections 2.1–2.4, pp. 192–201). The factor $\gamma$ in the expression for $x'$ reflects that a moving observer, using a contracted measuring rod, will overestimate lengths by a factor of $\gamma$. Likewise, the factor $1/\gamma$ in the first term of the expression for $t'$ reflects the fact that a moving observer, using clocks ticking at a slower rate than those same clocks at rest in the ether, will underestimate time intervals by a factor of $\gamma$.

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[107] I am not the first to draw attention to this curious state of affairs. Valentin Bargmann brought up this point after a talk by Arthur Miller (1980) on Lorentz and Poincaré at the symposium in Princeton on the occasion of the centennial of Einstein's birth. Bargmann asked: "There is one aspect [...] that I always found strange. This is the fact that in the famous book by Lorentz on the theory of electrons [Lorentz 1916], Poincaré is hardly mentioned. There is copious mention of Einstein's work, but hardly any of Poincaré's. Now I wonder whether Dr. Miller has any comment on this?" Miller did not have an explanation to offer (and did not bother to include his reply to Bargmann in the reprint of his paper in Miller 1986). He mentioned that Lorentz gives Poincaré credit for the Poincaré stresses, which is true, and for "making the Lorentz transformations into a more symmetrical form" (Woolf 1980, p. 93), which is not true. Other than that, Miller said "there was no reason to mention Poincaré" (ibid.). I obviously share Bargmann's puzzlement.

These two effects had to some extent been recognized by Lorentz before 1905. In his discussions of the Michelson-Morley effect in 1892 and 1895 he emphasized that a moving observer can not detect the contraction effect by putting a measuring rod along the interferometer arms (see section 3.2); and in 1899 he explicitly noticed that the frequency of the oscillations of electrons responsible for the emission of light depends on the state of motion of the light source with respect to the ether (see section 3.3). Only after 1905, however, did he fully realize the pervasiveness of these phenomena and their effect on any physical system that could be used as a measuring rod or a clock.

There is not the slightest hint in Lorentz's work prior to 1905 as to how the second $x$-dependent term in the expression for $t'$ might be given a physical interpretation. This term reflects the relativity of simultaneity, or, in terms of Lorentz's theory, it reflects that a moving observer will use (something equivalent to) Einstein's light signaling method to synchronize his or her clocks. As a consequence, these clocks will keep the local rather than the real Newtonian time, as can be shown by a simple calculation that I will go through below. Before I do that, I want to draw attention to what is perhaps the best example of the general puzzle that I already mentioned: why is it that Lorentz took over this idea from Einstein and ignored what was essentially the same idea put forward several years earlier by Poincaré in the context of Lorentz's first order theory in the *Versuch*?

Poincaré had given his physical interpretation of the $x$-dependence of local time in, of all places, his contribution to the Lorentz *Festschrift* of 1900 (Poincaré 1900b, p. 483), and again in a lecture in St. Louis in 1904 that was published in 1905 in his book *La valeur de la science* (Poincaré 1904, pp. 99–100[108]). Yet, Lorentz was won over for the idea only by Einstein. Moreover, he did not even mention Poincaré in this context, whereas Lorentz is normally very careful to give credit wherever credit is due. In the second edition of *The theory of electrons* of 1916, Lorentz gives credit only to Einstein in the following footnote that is otherwise characteristic for Lorentz's intellectual generosity:

> If I had to write the last chapter now, I should certainly have given a more prominent place to Einstein's theory of relativity [...] by which the theory of electromagnetic phenomena in moving systems gains a simplicity that I had not been able to obtain. The chief cause of my failure was my clinging to the idea that the variable $t$ only can be considered as the true time and that my local time $t'$ must be regarded as no more than an auxiliary mathematical quantity. (Lorentz 1916, p. 321, note 72*)

It is totally unclear to me why Lorentz did not mention Poincaré at this juncture.

How does the physical interpretation of local time that Lorentz took from Einstein work? Once again, consider the Galilean frame $S$ moving at a velocity $\mathbf{v} = (v, 0, 0)$ with respect to the

[108] I already discussed this passage in section 3.1.

frame $S_0$ at rest in the ether. Suppose an observer in $S$ has two clocks, one ($A$) at the origin $O$ of $S$, the other one *(B)* on the $x$-axis at a distance $x'$ from $O$ (according to the moving observer's contracted measuring rods). At $t = 0$ in the real Newtonian time of $S_0$ clock $A$ is set to zero. At that moment (i.e., when $t = t_A = 0$), the moving observer sends a light signal from $A$ to $B$ in order to check whether these two clocks are properly synchronized. According to the moving observer, for whom the velocity of light to all intents and purposes is equal to the constant $c$, clock $B$ should read

$$t_B = \frac{x'}{c} \tag{3.137}$$

upon arrival of the light signal at $B$. In the real Newtonian time of $S_0$, however, the signal has to catch up with clock $B$ rushing away at velocity $\mathbf{v}$ and only arrives at $B$ at

$$t = \frac{x'/\gamma}{c - v}, \tag{3.138}$$

where the factor $\gamma$ takes into account the contraction of the moving observer's measuring rods. Since moving clocks run slow, only $1/\gamma$ times the amount in Eq. 3.138 has gone by on clock $A$ between the moment the light was sent from $A$ and the moment it was received at $B$. So, when the light arrives at clock $B$, the clock at $A$ reads

$$t_A = \frac{x'}{\gamma^2 (c - v)} = \frac{x'}{c}(1 + v/c) \tag{3.139}$$

Comparing Eq. 3.137 and Eq. 3.139, we see that, if the moving observer has synchronized his clocks using (something equivalent to) Einstein's light signaling method, an observer at rest in the ether will conclude that clock $B$ lags behind clock $A$:

$$t_B = t_A - (v/c^2)\, x'. \tag{3.140}$$

Inserting $t_A = t/\gamma$ and $x' = \gamma x$ (see Eq. 3.136), we conclude that clocks at arbitrary points $(x, 0, 0)$ of $S$, when they are synchronized by a moving observer, indeed keep the local time

$$t' = t/\gamma - \gamma (v/c^2) x. \tag{3.141}$$

Since observers in $S$ and observers in $S_0$ will agree on the synchronization of clocks in one and the same $yz$-plane, this conclusion is valid for arbitrary points $(x, y, z)$ of $S$.

As Lorentz clearly realized (Lorentz 1916, p. 224), it immediately follows from the fact that the moving observer measures the space and time variables ($t'$, $\mathbf{x}'$) of the theorem of corresponding states rather than ($t$, $\mathbf{x}$), the real Newtonian time and the coordinates of the Galilean frame $S$, that his measurements of the speed of light will always give the result $c$. The moving observer will therefore have the illusion that he is at rest in the ether (ibid., p. 226). Lorentz now imagines that the moving observer sets out to perform all sorts of experiments in electrodynamics. He argues that the moving observer will not introduce the real fields $\mathbf{E}$ and $\mathbf{B}$ in these investigations, but the fields $\mathbf{E}'$ and $\mathbf{B}'$ of the theorem of corresponding states. These fields, considered as functions of the space and time coordinates ($t'$, $\mathbf{x}'$) measured by the moving observer, satisfy Maxwell's equations. This will further strengthen the observer's illusion that he is at rest in the ether.

Summing up the discussion so far, Lorentz writes: "if both $A_0$ [an observer at rest in the ether] and $A$ [an observer in uniform motion with respect to the ether] were to keep a record of their observations and the conclusions drawn from them, these records would, on comparison, be found to be exactly identical" (Lorentz 1916, p. 226).

**3.5.5 The reciprocity of the Lorentz transformation.** In *The theory of electrons* Lorentz, for the first time, also emphasized the reciprocity of the transformation formulae of his theorem of corresponding states (Lorentz 1916, sections 192–193, pp. 226–229):

> Attention must now be drawn to a remarkable reciprocity that has been pointed out by Einstein.[109] Thus far it has been the task of the observer $A_0$ to examine the phenomena in the stationary system, whereas $A$ has had to confine himself to the system $S$. Let us now imagine that each observer is able to see the system to which the other belongs, and to study the phenomena going on in it. Then, $A_0$ will be in the position in which we have all along imagined ourselves to be (though, strictly speaking, on account of the earth's motion, we are in the position of $A$); in studying the electromagnetic field in $S$, he will be led to introduce new variables [$t'$, $\mathbf{x}'$, $\mathbf{E}'$, $\mathbf{B}'$ (see Eqs. 3.54–3.55] and so he will establish the equations [for $\mathbf{E}'$ and $\mathbf{B}'$ as functions of ($t'$, $\mathbf{x}'$) (see Eq. 3.56 for $\rho = \mathbf{u} = 0$)]. The reciprocity consists in this that if the observer $A$ describes in exactly the same manner the field in the stationary system, he will describe it accurately. (Lorentz 1916, pp. 226–227)

So, Lorentz imagines that an observer in the moving frame $S$, who is under the impression to be at rest with respect to the ether, uses the theorem of corresponding states to describe some situation in the frame $S_0$, which is actually at rest with respect to the ether, but which, for the observer in $S$, appears to be moving with respect to the ether at a velocity $-\mathbf{v}$. Labeling the auxiliary quantities of the theorem of corresponding states used by the observer in $S$ with

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[109] Again no mention is made of Poincaré who made basically the same point, albeit perhaps in a less intuitive way, by proving the group character of the Lorentz transformation in Poincaré 1906.

double primes and using the analogues of Eq. 3.57 (giving $t_0$, $\mathbf{x}_0 \rightarrow t', \mathbf{x}'$) and Eq. 3.55 (giving $\mathbf{E}, \mathbf{B} \rightarrow \mathbf{E}', \mathbf{B}'$) with $-\mathbf{v}$ instead of $\mathbf{v}$, we find:[110]

$$\mathbf{x}'' = l\,\mathrm{diag}(\gamma, 1, 1)\,(\mathbf{x}' + \mathbf{v}\,t'), \qquad t'' = l\gamma\,(t' + (v/c^2)x'), \tag{3.142}$$

$$\begin{aligned} \mathbf{E}'' &= \frac{1}{l^2}\,\mathrm{diag}(1, \gamma, \gamma)\,(\mathbf{E}' - \mathbf{v} \times \mathbf{B}'), \\ \mathbf{B}'' &= \frac{1}{l^2}\,\mathrm{diag}(1, \gamma, \gamma)\,(\mathbf{B}' + \frac{1}{c^2}\,\mathbf{v} \times \mathbf{E}'). \end{aligned} \tag{3.143}$$

What needs to be shown is that these double primes quantities are the quantities measured by the observer in $S_0$. That would establish the reciprocity Lorentz mentions. After all, from the point of view of the observer in $S_0$ examining a situation in $S$, the observer in $S$ measures the space and time coordinates and the fields of the theorem of corresponding states. Of course, we already know what the observer in $S_0$ measures. As an observer who is actually at rest in the ether, he will measure the real time $t_0$, the real coordinates $x_0$, and the real fields $\mathbf{E}$ and $\mathbf{B}$. So, what needs to be shown is that the transformation $(t', \mathbf{x}', \mathbf{E}', \mathbf{B}') \rightarrow (t'', \mathbf{x}'', \mathbf{E}'', \mathbf{B}'')$ in Eqs. 3.142–3.143 gives the same result as the inverse of the transformation $(t_0, \mathbf{x}_0, \mathbf{E}, \mathbf{B}) \rightarrow (t', \mathbf{x}', \mathbf{E}', \mathbf{B}')$ in Eq. 3.57 and Eq. 3.55. Inverting these last two equations, we find

$$\mathbf{x}_0 = \frac{1}{l}\mathrm{diag}(\gamma, 1, 1)\,(\mathbf{x}' + \mathbf{v}\,t'), \qquad t_0 = \frac{1}{l}\gamma\,(t' + (v/c^2)x'), \tag{3.144}$$

$$\begin{aligned} \mathbf{E} &= l^2\mathrm{diag}(1, \gamma, \gamma)\,(\mathbf{E}' - \mathbf{v} \times \mathbf{B}'), \\ \mathbf{B} &= l^2\mathrm{diag}(1, \gamma, \gamma)\,(\mathbf{B}' + \frac{1}{c^2}\,\mathbf{v} \times \mathbf{E}'). \end{aligned} \tag{3.145}$$

Comparing Eqs. 3.144–3.145 to Eqs. 3.142–3.143, we see that

$$(t'', \mathbf{x}'', \mathbf{E}'', \mathbf{B}'') = (t_0, \mathbf{x}_0, \mathbf{E}, \mathbf{B}) \tag{3.146}$$

as long as we set $l = 1$. This shows that even if the moving observer were to perform measurements on systems at rest in the ether, he would not be able to detect his motion with respect to the ether.

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[110] Lorentz sets $l = 1$ at the beginning of this calculation. It will be instructive, however, to leave $l$ undetermined for the time being.

Even though Lorentz does not mention this, this calculation provides a much more fundamental argument for setting $l = 1$ than the convoluted argument we examined in section 3.4 which turned on the intricacies of the electromagnetic mass of Lorentz's electron.

In this passage in *The theory of electrons*, Lorentz leaves the discussion of the reciprocity between the observers at rest and in motion with respect to the ether at this rather abstract level. He does not discuss the physical a-symmetry of the situation. Consider the measurements of Lorentz's two observers of the length of each other's rods or the rate of each other's clocks. It follows from the general argument in Eqs. 3.142–3.146 that both observers will find that the other observer's rods are shorter than his own and that the other observer's clocks run slow compared to his own. However, these identical findings receive very different physical interpretations in Lorentz's theory. The observer at rest in the ether performing measurements on the rods and clocks in motion through the ether is measuring the dynamical effect this motion has on the length of physical systems and the rate of physical processes.[111] By contrast, the observer in motion through the ether performing measurements on the rods and clocks at rest in the ether is basically deceived by his own instruments. Rods at rest in the ether are actually *longer* than his own rods, and clocks at rest in the ether actually run *fast* compared to his own clocks, but, because the moving observer has not synchronized his clocks properly (i.e., he has synchronized them according to the local time and not according to the real Newtonian time), he will (mistakenly) conclude from his measurements that rods at rest in the ether are *shorter* than his own, and that clocks at rest in the ether run *slow* compared to his own.

As one may have come to expect by now, this is another point of confusion in the historical literature (see, e.g., Schaffner 1969, p. 511; Miller 1974, pp. 42–43). Although I have not been able to pin this down on one particular sentence, Schaffner strongly suggests that the physical a-symmetry I drew attention to above is a fatal problem for Lorentz's theory. Miller, in his critique of Zahar, makes the point more bluntly. Immediately following the passage I debunked earlier, where Miller criticizes Zahar for endorsing the view that the theories of Lorentz and Einstein are empirically equivalent, citing the alleged problems concerning aberration and Doppler effect in Lorentz's theory, he writes:

> Neither are the two theories observationally equivalent concerning the *L.F.C.* [Lorentz-FitzGerald contraction] [...] [I]n Lorentz's theory the class of ether-fixed reference systems is not equivalent to the class of inertial systems, whereas in Einstein's theory of relativity of 1905 this asymmetry does not exist. This lack of symmetry in Lorentz's theory between different sets of space-time coordinates results in a lack of symmetry in the observation of the contraction. (Miller 1974, pp. 42–43)

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[111] Earlier in his book, Lorentz puts length contraction on a par with the expansion of an object or a gas upon heating it: "We may, I think, even go so far as to say that that, on this assumption [i.e., the contraction hypothesis], Michelson's experiment proves the changes of dimension in question, and that the conclusion is no less legitimate than the inferences concerning the dilatation by heat or the changes of the refractive index that have been drawn in many other cases from the observed positions of interference bands" (Lorentz 1916, p. 196).

Fortunately, there is a beautifully simple three-page paper by Dorling (1968) providing an elementary discussion of this point. The analysis I gave above was, in fact, based on Dorling's paper.[112] Dorling writes that a theory based on the notion of a stationary ether predicts that "the

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[112] Dorling's analysis does not take care of a more subtle argument to be found in the literature challenging the empirical equivalence of Lorentz's theory and special relativity in their predictions for the effects of length contraction.

The clearest statement of this more subtle argument that I have come across can be found in Feyerabend's critique of Zahar. In response to Zahar's claim that by 1905 Lorentz's theory is empirically equivalent to special relativity, Feyerabend writes: "This is not true. Even in 1905 (or in 1906, 1907, ...) equivalence had been established for equilibrium states only: if contractions and dilatations of length, time, changes of mass are the results of an interaction between the aether and the molecules embedded in it, then any change of velocity relative to the aether will lead to oscillations. Relativity, on the other hand, knows of no such oscillations. It was only in 1936 that this consequence of $L'$ [Feyerabend's notation for the hard core of Lorentz's research programme as defined by Zahar: one would think he needs an actual theory here, one incorporating the contraction hypothesis, rather than just the hard core, but that is unimportant for our purposes here] was tested, and refuted by Wood, Tomlison, and Essen [1937]" (Feyerabend 1974, p. 28; my italics). The experiment is also cited by Torretti (1983, p. 292, note 33) in this context. I am grateful to John Earman for drawing my attention to this particular challenge to the empirical equivalence of special relativity and Lorentz's theory.

Whittaker has a very different assessment of the Wood-Tomlison-Essen experiment. Describing the experiment in a few sentences taken almost verbatim out of the conclusion of the experimenters' paper (Wood, Tomlison, and Essen 1937, p. 633), he writes: "a rod in longitudinal vibration was rotated in a horizontal plane, so that its length varied periodically by reason of the FitzGerald contraction. Accurate measurements were made of the vibration frequency, which would have varied with the length, if the length only had been affected. According to relativity theory, however, there should be a complete compensation of the contraction in length, by a modification of the elasticity of the rod according to its orientation with respect to the direction of its motion, so that no change of frequency should be observed. [...] The experiment yielded a null result within narrow limits of uncertainty of about ± 4 parts in $10^{11}$, thus fully confirming the prediction of the *Poincaré-Lorentz theory of relativity*" (Whittaker 1953, II, p. 43; my emphasis).

How did the experimenters themselves understand the importance of their experiment? Did they see it the way Whittaker saw it or the way Feyerabend saw it? Given Whittaker's lack of appreciation of Einstein's work in special relativity (see the introduction to part two), one might suspect the latter. This suspicion is reinforced by the fact that on the same page he discusses the papers of Kennedy and Thorndike (1932) and Robertson (1949) that I discussed earlier, where the authors' explicit motivation was to establish the superiority of special relativity over Lorentz's theory. It turns out, somewhat surprisingly perhaps, that Whittaker's assessment of the Wood-Tomlison-Essen paper is essentially correct, and that Feyerabend and Torretti are the ones who are reading things into this paper that on closer examination simply are not there.

The Wood-Tomlison-Essen paper consists of two parts, a three-page introduction written by Wood, giving the motivation for the experiment, and a lengthy report of the experimental findings written by Tomlison and Essen. For our purposes only the introduction matters. In the first paragraph, Wood mentions Lorentz's interpretation of the Lorentz contraction as a dynamical effect (giving Lorentz's favorite analogy with the expansion of a rod upon heating it). The third paragraph starts with: "If this change of length is real, as Lorentz supposes, an interesting problem arises for a rod vibrating longitudinally" (Wood, Tomlison, and Essen 1937, p. 606). On the basis of Feyerabend's discussion of their experiment, one now expects Wood to commit the common beginners' blunder of claiming that, in special relativity, the change of length is not real. This basic misunderstanding seems to have persisted long after Einstein and Ehrenfest unambiguously set the record straight in 1910–1911 (see Klein 1970, pp. 152–154). Wood, however, does nothing of the sort. He goes on to argue, as one would expect on the basis of the almost verbatim paraphrase of his two colleagues' conclusion by Whittaker, that the relativity principle requires a compensating effect. And, contrary to the suggestion by Feyerabend, Wood does not make a clear distinction between the relativistic and the ether theoretic way of looking upon these two effects. This last point is clearly illustrated, I think, in the following footnote: "The alternative explanation, that neither length nor elasticity change, would, of course, be completely at variance with *the FitzGerald-Lorentz theory* and would require a stationary ether" (Wood, Tomlison, and Essen 1937, p. 607; my emphasis). This is a complex yet very revealing statement. It shows (a) that Wood looked upon the changes of length and elasticity required by the relativity principle as dynamical effects, just as Lorentz would

rod at rest in the ether *would appear expanded* if measured with the measuring rod of the moving system *provided* the clocks of the moving system are correctly synchronized according to a *Lorentzian prescription* [i.e., according to the real Newtonian time]" (Dorling 1968, p. 67; italics in the original) and that it "*would appear contracted* if measured with the measuring rod of the moving system *provided* the clocks of the moving system are correctly synchronized according to a *Einsteinian prescription* [i.e., according to the local time]" (ibid.; italics in the original). The proof of the first claim, Dorling writes, is "genuinely trivial," while the proof of the second claim is "a moderately easy exercise in special relativity" (ibid., p. 68). This is no exaggeration.

□Suppose we have a rod at rest in the ether pointing in the $x$-direction of the frame $S_0$ and having length $l_0$ for an observer at rest in the ether. A moving observer, at rest in a frame $S$ with velocity $\mathbf{v}$ with respect to $S_0$, who wants to measure the length $l'$ of this rod, will have to determine the positions $x'_{\mathrm{P}}$ and $x'_{\mathrm{Q}}$ of the end points of the rod *at the same instant*. The Lorentz transformation from the space-time coordinates $(x_0, t_0)$ of these two events in $S_0$ to their space-time coordinates $(x', t')$ in $S$ gives:

$$\begin{aligned} l' &= x'_{\mathrm{P}} - x'_{\mathrm{Q}} &= \gamma\left(x_0^{\mathrm{P}} - x_0^{\mathrm{Q}} - v\left(t_0^{\mathrm{P}} - t_0^{\mathrm{Q}}\right)\right), \\ t'_{\mathrm{P}} - t'_{\mathrm{Q}} &&= \gamma\left(t_0^{\mathrm{P}} - t_0^{\mathrm{Q}} - \frac{v}{c^2}\left(x_0^{\mathrm{P}} - x_0^{\mathrm{Q}}\right)\right). \end{aligned} \tag{3.147}$$

For $x_0^{\mathrm{P}} - x_0^{\mathrm{Q}}$ we can insert $l_0$ in these equations.

If the clocks in $S$ are synchronized according to the real Newtonian time, we have $t_0^{\mathrm{P}} - t_0^{\mathrm{Q}} = 0$, and the first equation in Eq. 3.147 reduces to

$$l' = \gamma\, l_0, \tag{3.148}$$

i.e., the rod at rest in the ether will appear *expanded* to the moving observer.

If the clocks in $S$ are synchronized according to the local time, we have $t'_{\mathrm{P}} - t'_{\mathrm{Q}} = 0$, and the second equation in Eq. 3.147 can be used to rewrite the first as

$$l' = \gamma\left(l_0 - (v^2/c^2)\, l_0\right) = l_0/\gamma, \tag{3.149}$$

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have done; and (b) that he did not think of the Wood-Tomlison-Essen experiment as refuting the "FitzGerald-Lorentz theory," despite the fact that (c) he did not believe in the existence of an ether!

In short, Whittaker's assessment of the Wood-Tomlison-Essen paper is pretty much correct—*including* the conflation of Einstein's and Lorentz's theories he implicitly ascribed to the authors and that one would suspect to be a reflection of Whittaker's own biases only—while Feyerabend's assessment is highly misleading at best. Even if the Wood-Tomlison-Essen experiment does distinguish between the theories of Lorentz and Einstein (and given the authority of Feyerabend, Torretti, and Earman, this is something that calls for further analysis, I guess), the experimenters themselves certainly did not think so.

i.e., the rod at rest in the ether will appear *contracted* to the moving observer.

Dorling leaves open the question whether Lorentz has the moving observer synchronizing his clocks according to real or according to local time. From Lorentz's discussion in *The theory of electrons*, it is clear that Lorentz opted for the latter alternative. In the conclusion of his paper, Dorling nonetheless writes:

> I have certainly never seen a clear statement of the theory I have been here attributing to Lorentz in his own writings. The best evidence I have come across that Lorentz did eventually hold a theory empirically precisely equivalent to Einstein's is contained in the following comment of P. Ehrenfest in the course of a lecture to an audience whose most distinguished member was Lorentz, the year after Ehrenfest had succeeded to Lorentz's chair at Leyden on Lorentz's recommendation. (Dorling 1968, p. 69)

Dorling then quotes the following passage from Ehrenfest's inaugural lecture entitled "On the crisis of the light ether hypothesis" held in Leiden on December 4, 1912 (vividly described in Klein 1970, pp. 1–5):

> So, we see that the ether-less theory of Einstein demands exactly the same here as the ether theory of Lorentz. It is, in fact, because of this circumstance, that according to the Einsteinian theory an observer must observe the exact same contractions, changes of rate, etc. in measuring rods, clocks, etc. moving with respect to him as according to the Lorentzian theory. And let it be said here right away and in all generality. As a matter of principle, there is no experimentum crucis between these two theories. (Ehrenfest 1913, pp. 17–18; quoted in Dorling 1968, p. 69)

Unlike Dorling, I do not see any reason for suspecting ulterior motives behind this statement of Ehrenfest. For one thing, this would be totally out of character for someone of Ehrenfest's almost painful intellectual integrity (amply documented in Klein 1970). However, if, for some reason, one does not want to take Ehrenfest's word for it, one can actually find a very clear discussion of the reciprocal character of the measurement of the length of rods and the rate of clocks in Lorentz's own lectures on relativity in 1910–1912 (Lorentz 1922, pp. 202–203). As we will see below, Lorentz clearly understood this discussion to be valid both in Einstein's theory and his own.

In the last section of *The theory of electrons*, Lorentz also emphasized the empirical equivalence of his own theory and special relativity. Given the importance of this section for appreciating Lorentz's attitude toward special relativity, I want to quote it in its entirety.

> It will be clear by what has been said that the impressions received by the two observers $A_0$ and $A$ would be alike in all respects. It would be impossible to decide which of them moves or stands still with respect to the ether, and there would be no reason for preferring the times and lengths measured by the one to those determined by the other, nor for saying that either of them is in possession of the "true" times or the "true" lengths. This is a point which Einstein has laid particular stress on, in a theory in which he starts from what he calls the principle of relativity, i. e. the principle that the equations by means of which physical phenomena may be described are not altered in form when we change the axes of coordinates for others having a uniform motion of translation relatively to the original system.

> I cannot speak here of the many highly interesting applications which Einstein has made of this principle. His results concerning electromagnetic and optical phenomena (leading to the same contradiction with Kaufmann's results that was pointed out in §179[113]) agree in the main with those which we have obtained in the preceding pages, the chief difference being that Einstein simply postulates what we have deduced, with some difficulty and not altogether satisfactorily, from the fundamental equations of the electromagnetic field. By doing so, he may certainly take credit for making us see in the negative result of experiments like those of Michelson, Rayleigh and Brace, not a fortuitous compensation of opposing effects but the manifestation of a general and fundamental principle.
>
> Yet, I think, something may also be claimed in favour of the form in which I have presented the theory. I cannot but regard the ether, which can be the seat of an electromagnetic field with its energy and its vibrations, as endowed with a certain degree of substantiality, however different it may be from all ordinary matter. In this line of thought it seems natural not to assume at starting that it can never make any difference whether a body moves through the ether or not, and to measure distances and lengths of time by means of rods and clocks having a fixed position relatively to the ether.
>
> It would be unjust not to add that, besides the fascinating boldness of its starting point, Einstein's theory has another marked advantage over mine. Whereas I have not been able to obtain for the equations referred to moving axes *exactly* the same form as for those which apply to a stationary system, Einstein has accomplished this by means of a system of new variables slightly different from those which I have introduced. I have not availed myself of his substitutions, only because the formulae are rather complicated and look somewhat artificial, unless one deduces them from the principle of relativity itself.[114] (Lorentz 1916, pp. 229–230).

**3.5.6 The transformation of charge and current density and the solution of the problem of non-static charge distributions.** The last sentence of the long quotation I just gave refers to Einstein's derivation of the transformation equations for charge and current density in his first relativity paper (Einstein 1905a, section 9, pp. 916–917). We need to take a close look at these transformation equations to fully appreciate Lorentz's point here. In doing so, we will also see how Lorentz eventually solved what I called the problem of non-static charge distributions in the footnote he appended to this passage in the second edition of *The theory of electrons* of 1916.

Consider a charge distribution at rest with respect to a Galilean frame $S$ with velocity $\mathbf{v}$ with respect the frame $S_0$ at rest in the ether. Before 1905, Lorentz tacitly assumed that an observer in $S$ and an observer in $S_0$ would agree on the quantity $\rho$ representing the charge density of this charge distribution, even though the observer in $S$ would write $\rho$ as a function of $(t, \mathbf{x})$ whereas the observer in $S_0$ would write $\rho$ as a function of $(t_0, \mathbf{x}_0)$. Similarly, he tacitly assumed that the velocity fields these two observer would use in the expression for the current density of the moving charge distribution would differ only by a term $\mathbf{v}$. For the observer in $S$, the current

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113 It is at this point in the second edition of the book, that Lorentz added the footnote referring to the experiments of Bucherer, Guye and Lavanchy, and others that I mentioned in section 3.4.

114 In the second edition, Lorentz added the footnote I quoted above starting with: "If I had to write the last chapter now, I should certainly have given a more prominent place to Einstein's theory of relativity" (Lorentz 1916, p. 321, note 72*).

density would be given by $\rho\mathbf{u}$ as a function of $(t, \mathbf{x})$, where $\mathbf{u}$ is a velocity with respect to $S$; for the observer in $S_0$, it would be given by $\rho(\mathbf{u} + \mathbf{v})$ as a function of $(t_0, \mathbf{x}_0)$.

Once we have come to realize, as Lorentz did after 1905, that an observer in $S$ actually measures $(t', \mathbf{x}')$ rather than $(t, \mathbf{x})$, we have to be more careful. Consider the velocity field in the current density first. Let $\mathbf{u}'$ denote velocities measured by an observer in $S$, using measuring rods that are contracted and clocks that run slow and are not properly synchronized. The relation between such velocities $\mathbf{u}'$ with respect to $S$ according to an observer in $S$ to the corresponding velocities $\mathbf{u}$ with respect to $S$ according to an observer in $S_0$ can be found with the help of the transformation equations for $(t, \mathbf{x}) \rightarrow (t', \mathbf{x}')$ in Eq. 3.54 (for $l = 1$) or Eq. 3.136 and its inverse.

First, I will express $\mathbf{u}'$ in terms of $\mathbf{u}$. For the $x$-component of $\mathbf{u}'$ we can write

$$u'_x = \frac{dx'}{dt'} = \frac{\gamma\, dx}{dt/\gamma - \gamma(v/c^2)\, dx} = \frac{\gamma^2 u_x}{1 - \gamma^2 \dfrac{u_x v}{c^2}} . \tag{3.150}$$

For the $y$- and $z$-components, we likewise find

$$u'_y = \frac{\gamma u_y}{1 - \gamma^2 \dfrac{u_x v}{c^2}}, \qquad u'_z = \frac{\gamma u_z}{1 - \gamma^2 \dfrac{u_x v}{c^2}} . \tag{3.151}$$

Eqs. 3.150–3.151 can be more compactly written as

$$\mathbf{u}' = \frac{\gamma}{(1 - \gamma^2 u_x v/c^2)} \operatorname{diag}(\gamma, 1, 1) \;\; \mathbf{u}. \tag{3.152}$$

It will be convenient to write down the relations expressing $\mathbf{u}$ in terms of $\mathbf{u}'$ as well. Using the inverse of the transformation $(t, \mathbf{x}) \rightarrow (t', \mathbf{x}')$, i.e.,

$$x = x'/\gamma, \quad y = y', \quad z = z', \quad t = \gamma\,(t' + (v/c^2)x'), \tag{3.153}$$

we can write

$$u_x = \frac{dx}{dt} = \frac{dx'/\gamma}{\gamma\left(dt' + (v/c^2)\, dx'\right)} = \frac{u'_x}{\gamma^2\left(1 + u'_x v/c^2\right)},$$

$$u_y = \frac{u'_y}{\gamma\left(1 + u'_x v/c^2\right)}, \qquad u_z = \frac{u'_z}{\gamma\left(1 + u'_x v/c^2\right)}, \tag{3.154}$$

or, in more compact notation,[115]

$$\mathbf{u} = \frac{1}{\gamma\,(1 + u'_x v/c^2)}\,\mathrm{diag}(1/\gamma, 1, 1)\;\;\mathbf{u}'. \tag{3.155}$$

If Maxwell's equations are to hold for the observer in $S$, we should have

$$\mathrm{div}'\,\mathbf{E}' = \rho'/\varepsilon_0\,, \quad \mathrm{curl}'\,\mathbf{B}' = \mu_0\rho'\mathbf{u}' + \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}, \tag{3.156}$$

where $\mathbf{E}'$, $\mathbf{B}'$, $\rho'$, and $\mathbf{u}'$ are all functions of $(t', \mathbf{x}')$. Compare this to the equations for $\mathrm{div}'\,\mathbf{E}'$ and $\mathrm{curl}'\,\mathbf{B}'$ Lorentz derived (see Eq. 3.56 (for $l = 1$) and Eq. 3.134):

$$\begin{aligned} \mathrm{div}'\,\mathbf{E}' &= \frac{\rho}{\gamma\varepsilon_0}\left(1 - \gamma^2\frac{v\,u_x}{c^2}\right), \\ \mathrm{curl}'\,\mathbf{B}' &= \mu_0\,\mathrm{diag}(\gamma, 1,\;\;1)\,\rho\,\mathbf{u} + \frac{1}{c^2}\frac{\partial \mathbf{E}'}{\partial t'}\,. \end{aligned} \tag{3.157}$$

Comparing the equations for $\mathrm{div}'\,\mathbf{E}'$ in Eqs. 3.156–3.157, we see that if Maxwell's equations are to hold for the observer in $S$, we should have

$$\rho' = \frac{\rho}{\gamma}\left(1 - \gamma^2\frac{v\,u_x}{c^2}\right). \tag{3.158}$$

It is easily seen that with Eq. 3.158 for $\rho'$ and Eq. 3.152 for $\mathbf{u}'$, the equations for $\mathrm{curl}'\mathbf{B}'$ in Eqs. 3.156–3.157 also become equivalent:

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[115] Since $\mathbf{u}$ is a velocity with respect to $S$ according to the observer in $S_0$, and since $S$ has velocity $\mathbf{v}$ with respect to $S_0$, the velocity with respect to $S_0$ according to the observer in $S_0$ will simply be $\mathbf{u} + \mathbf{v}$. Inserting Eqs. 3.154–3.155 for $\mathbf{u}$ into the expression $\mathbf{u} + \mathbf{v}$, we find the familiar relativistic addition theorem of velocities. Consider, for instance, the $x$-component:

$$\begin{aligned} u_x + v &= v + \frac{u'_x}{\gamma^2\left(1 + u'_x v/c^2\right)} \\ &= \frac{v\gamma^2\left(1 + u'_x v/c^2\right) + \gamma^2(1 - v^2/c^2)u'_x}{\gamma^2\left(1 + u'_x v/c^2\right)} \\ &= \frac{v + u'_x}{1 + u'_x v/c^2}\,. \end{aligned}$$

This clearly shows that the peculiar form of the addition theorem simply comes from the fact that we are adding velocities measured by different observers.

$$\rho'\mathbf{u}' = \frac{\rho}{\gamma}\,(1-\gamma^2 u_x v/c^2)\,\frac{\gamma\,\mathrm{diag}(\gamma,1,1)\;\;\mathbf{u}}{(1-\gamma^2 u_x v/c^2)} = \mathrm{diag}(\gamma,1,1)\;\;\rho\,\mathbf{u}. \tag{3.159}$$

The transformation equations for $(\rho, \rho\,\mathbf{u}) \rightarrow (\rho', \rho'\mathbf{u}')$ that one can read off from Eqs. 3.156–3.157, i.e.,

$$\rho' = \frac{\rho}{\gamma}\left(1-\gamma^2\,\frac{v\,u_x}{c^2}\right), \qquad \rho'\mathbf{u}' = \mathrm{diag}(\gamma,1,\;1)\rho\,\mathbf{u}, \tag{3.160}$$

are in accordance with the fact that $(\rho' c, \rho'\mathbf{u}')$ are the components of a four-vector (cf. Eq. 3.58).

This determination of the relation between the charge density $\rho'$ measured by an observer in $S$ and the charge density $\rho$ measured by an observer in $S_0$ by comparing the equations for $\mathrm{div}'\,\mathbf{E}'$ in Eqs. 3.156–3.157 is satisfactory only if we already know that Maxwell's equations are Lorentz invariant. Since Einstein in 1905 wanted to prove the Lorentz invariance of Maxwell's equations, he should have given an independent argument for the relation between $\rho'$ and $\rho$. Instead, Einstein derived this relation, as Lorentz correctly points out in the passage I quoted above, "from the principle of relativity itself," i.e., he used that Maxwell's equations are compatible with the two basic postulates of his theory, rendering his proof of the Lorentz invariance of Maxwell's equations circular. The problem is easily fixed. In his paper "On the dynamics of the electron," Poincaré, working independently of Einstein, gave an impeccable derivation of the relativistic transformation law for charge density (Poincaré 1906, pp. 151–152; quoted and discussed in Miller 1973, pp. 251–252). Lorentz does not refer to this important contribution of Poincaré in this passage in *The theory of electrons*. In the footnote he appended to this passage in the second edition of 1916, Lorentz gives a derivation of the relation between $\rho'$ and $\rho$ which is mathematically equivalent to Poincaré's 1906 derivation, again without citing Poincaré.

Before I go through Lorentz's derivation, I need to address another notorious muddle in the literature on Lorentz's theorem of corresponding states, a muddle for which, once again, Poincaré seems largely responsible. Notice that Lorentz refers to Einstein's "new variables slightly different from those which I have introduced" (Lorentz 1916, p. 230). Both in his famous 1904 paper (Lorentz 1904b, p. 176, Eqs. (7)–(8)) and in *The theory of electrons* (Lorentz 1916, p. 197, Eqs. (289)–(290)), Lorentz had introduced the auxiliary variables

$$\rho' \equiv \rho/\gamma l^3, \qquad \mathbf{u}' \equiv \mathrm{diag}(\gamma^2,\,\gamma,\,\gamma)\;\mathbf{u}. \tag{3.161}$$

The quantity $\rho'$ comes into play in Lorentz's treatment of electrostatics (see Eq. 3.47 (with $l$ = 1) and Eq. 3.60). The rationale for the definition of $\mathbf{u}'$ is that it gives

$$\rho'\mathbf{u}' = \mathrm{diag}(\gamma/l^3, 1/l^3, 1/l^3)\ \rho\mathbf{u}, \tag{3.162}$$

so that the equation for $\mathrm{curl}'\mathbf{B}'$ gets the same form as the corresponding Maxwell equation (cf. Eq. 3.56). After deriving transformation equations equivalent to the ones in Eq. 3.160, Poincaré writes: "Here I must for the first time indicate a disagreement with Lorentz's analysis" (Poincaré 1906, p. 152). Poincaré proceeds to show that with the definitions of $\rho'$ and $\mathbf{u}'$ in Eq. 3.161, the continuity equation $\partial\rho'/\partial t' + \mathrm{div}'\rho'\mathbf{u}' = 0$ is not satisfied. If we want to interpret $\rho'$ and $\rho'\mathbf{u}'$ as the charge and current density measured by the moving observer, as Poincaré did and as Lorentz would do *after 1905*, this obviously shows that Lorentz's Eq. 3.161 is wrong and that the correct equation is Poincaré's Eq. 3.160. But, in 1904, Lorentz attached as little physical meaning to $\rho'$ and $\mathbf{u}'$ as he did to $t'$, $\mathbf{x}'$, $\mathbf{E}'$, and $\mathbf{B}'$. In the words of Robert Rynasiewicz: "The claim that Lorentz had [...] the wrong formula for the transformation of charge density and that Poincaré corrected it, rests on a misunderstanding of the strategy involved [i.e., Lorentz's strategy of corresponding states in its pre-1905 version]. One is free to stipulate definitions as one pleases" (Rynasiewicz 1988, p. 73).

Rynasiewicz does not give any references to authors making this claim, but the confusion is wide spread. Miller (1973, p. 252), in discussing Poincaré 1906, for instance, accepts Poincaré's misguided criticism of Lorentz at face value. Zahar (1973, p. 233) talks about "*Lorentz's mistake in the transformation equation of ρ*" (Zahar's italics).[116] Holton has described Lorentz's definitions of $\rho'$ and $\mathbf{u}'$ in Eq. 3.161 more accurately as a "striking flaw" (Holton 1969, p. 321). In support of this assessment, Holton quotes a footnote Lorentz added to the German translation that was made of Lorentz 1904b for inclusion in Blumenthal 1913.[117] Lorentz wrote:

> One will notice that in this work [i.e., Lorentz 1904b] the transformation equations of Einstein's Relativity Theory have not quite been attained. Neither equation (7) [i.e, the equation for $\rho'$ in Eq. 3.161] nor formula (8) [i.e, the equation for $\mathbf{u}'$ in Eq. 3.161] has the form given by Einstein, and as a result I was unable to make the term $-wu'_x/c^2$ [i.e., in my notation, $-\gamma^2 vu_x/c^2$] in equation (9) [i.e., Eq. 3.56] disappear and to put equation (9) exactly in the form which holds for a system at rest [i.e., Maxwell's equations]. On this circumstance depends the clumsiness of many of the further considerations in this work. It is owing to

[116] Zahar does make an interesting point in this context. He emphasizes the physical interpretation of the difference between Lorentz's Eq. 3.161 for $\rho'$ and $\rho'\mathbf{u}'$ and Poincaré's Eq. 3.160, when both equations are understood as purporting to represent the charge and current density measured by the moving observer. When evaluated from this point of view, Lorentz only takes into account length contraction and time dilation and fails to take into account the relativity of simultaneity, whereas Poincaré deals with all three effects. The effect of the relativity of simultaneity is, of course, precisely what is responsible for Lorentz's impression that these equations (which Lorentz took from Einstein 1905a rather than from Poincaré 1906) "are rather complicated and look somewhat artificial, unless one deduces them from the principle of relativity itself" (Lorentz 1915, p. 230).

[117] For the English translation of this volume (Lorentz et al. 1952), the English version in the Proceedings of the Amsterdam Academy was used and this very important footnote was overlooked.

> Einstein that the relativity principle was first announced as a general strictly and exactly valid law. (quoted (in translation) in Holton 1969, p. 321)

I will now go through a somewhat more intuitive version of the derivation of Eq. 3.158 for $\rho'$ that Lorentz included in the second edition of *The theory of electrons* (Lorentz 1916, note 72*, pp. 324–325).

Consider Fig. 3.9. An observer in $S$, moving through the ether at a velocity $\mathbf{v} = (v, 0, 0)$, wants to determine the charge density $\rho'$ at the origin $O$ of $S$ at (local) time $t' = 0$. To this end, he considers a small box with sides of length $\Delta x'$, $\Delta y'$, and $\Delta z'$ around $O$, and he determines the total amount of charge $\Delta q$ enclosed by that box at time $t' = 0$. Assuming that $\rho'$ and $\mathbf{u}'$ are continuous functions of $(t', \mathbf{x}')$, we can take $\rho'$ and $\mathbf{u}'$ to be constant inside the box, provided $\Delta x'$, $\Delta y'$, and $\Delta z'$ are chosen small enough. The charge density $\rho'$ at O at time $t' = 0$ for the observer in $S$ is given by:

$$\rho'(\mathbf{x}'=0, t'=0) = \frac{\Delta q}{\Delta x' \Delta y' \Delta z'} \tag{3.163}$$

An observer in $S_0$ will not agree with the observer in $S$ that this is the charge density at this particular point in space and time. The observer in $S_0$ will complain that the observer in $S$ uses measuring rods that are contracted and clocks that run slow and that are not properly synchronized in his determination of the volume of the box occupied by the charge distribution with total charge $\Delta q$ under consideration at the moment the moving clock at $O$ reads $t' = 0$. For the observer in $S_0$, the charge distribution occupies a box of a somewhat different shape than the box the observer in $S$ claims it occupies, and the volume of that slightly different box should be determined with uncontracted measuring rods.

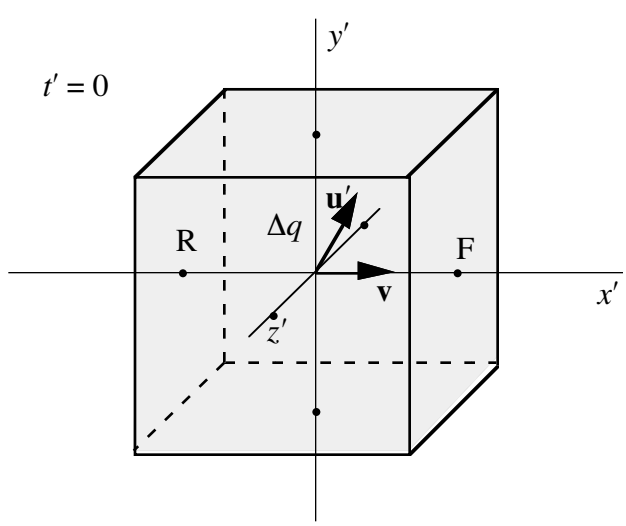


Figure 3.9: The transformation of charge density.

Fig. 3.9 shows the front and rear ends of the charge distribution (indicated by the shading in the figure) at $t' = 0$. So, in real time the front end is shown at

$$t_{\mathrm{F}} = \gamma\left(t'_{\mathrm{F}} + (v/c^2)x'_{\mathrm{F}}\right) = \gamma(v/c^2)\frac{\Delta x'}{2}, \tag{3.164}$$

where I used Eq. 3.153 for the transformation $(t', \mathbf{x}') \rightarrow (t, \mathbf{x})$; the rear end is shown at

$$t_{\mathrm{R}} = \gamma\left(t'_{\mathrm{R}} + (v/c^2)x'_{\mathrm{R}}\right) = -\gamma(v/c^2)\frac{\Delta x'}{2}. \tag{3.165}$$

So, Fig. 3.9 shows the front end at $t > 0$ and the rear end at $t < 0$. At t = 0, the front end would have been to the left of the point labeled $F$ in the figure, and the rear end would be to the right of $R$. It is easy to calculate by how much. Recall that we are assuming that $\mathbf{u}'$ and therefore $\mathbf{u}$ (see Eq. 3.155) are constant inside the box. So, between $t = 0$ and $t = \gamma(v/c^2)\Delta x'/2$, the front end of the charge distribution moves a distance

$$\gamma\,\frac{v\,u_x\Delta x'}{2c^2}, \tag{3.166}$$

and the rear end moves that same distance between $t = -\gamma(v/c^2)\Delta x'/2$ and $t = 0$. So, for the observer in $S_0$, the length $\Delta x$ of the box enclosing the charges considered by the observer in $S$ is

$$\Delta x = \frac{\Delta x'}{\gamma} - \gamma\frac{v\,u_x}{c^2}\Delta x' = \frac{\Delta x'}{\gamma}\left(1 - \gamma^2\,\frac{v\,u_x}{c^2}\right). \tag{3.167}$$

Eq. 3.167 can be rewritten as

$$\Delta x' = \frac{\gamma\Delta x}{1 - \gamma^2\,\dfrac{v\,u_x}{c^2}}. \tag{3.168}$$

Inserting Eq. 3.168 into Eq. 3.163 for $\rho'$, along with $\Delta y' = \Delta y$ and $\Delta z' = \Delta z$, we find

$$\rho'(\mathbf{x}'=0, t'=0) = \frac{\Delta q}{\Delta x\,\Delta y\,\Delta z}\,\frac{1}{\gamma}\left(1 - \gamma^2\,\frac{v\,u_x}{c^2}\right). \tag{3.169}$$

For the observer in $S_0$, the charge density at this particular point in space and time is:

$$\rho(\mathbf{x}=\mathbf{x}'=0, t=t'=0) = \frac{\Delta q}{\Delta x\,\Delta y\,\Delta z}. \tag{3.170}$$

Inserting Eq. 3.170 into Eq. 3.169 and suppressing the dependence on the space-time coordinates, we find

$$\rho' = \frac{\rho}{\gamma}\left(1 - \gamma^2 \frac{v\,u_x}{c^2}\right). \tag{3.171}$$

This is just Eq. 3.158, which Einstein inferred directly from the Lorentz invariance of Maxwell's equations.

With this result, Lorentz's theory for the electrodynamics of moving bodies has at last reached its final form. The technique of corresponding states now works to solve any problem of the following form:

> Given $\rho'$ and $\rho'\mathbf{u}'$ (the charge and current densities *measured* by an observer in $S$, a frame moving through the ether at some constant velocity $\mathbf{v}$) as functions of $\mathbf{x}'$ and $t'$ (the space and time coordinates *measured* by the observer in $S$) find $\mathbf{E}'$ and $\mathbf{B}'$ (the electric and magnetic fields *measured* by the observer in $S$) as functions of $\mathbf{x}'$ and $t'$.

Since the primed quantities satisfy Maxwell's equations, this problem is fully equivalent to solving the identical problem in a frame at rest in the ether. In principle, one can use the definitions of the primed quantities to compute the corresponding real quantities. However, since the observer in $S$ has no way of ascertaining his velocity $\mathbf{v}$ with respect to the ether and might as well pretend to be at rest in the ether, these real quantities are no longer of any real interest.

**3.5.7 Lorentz's arguments for preferring his own theory over Einstein's**. As we already saw, the final section of *The theory of electrons* contains one short paragraph where Lorentz states his reasons for preferring his own ether theory over Einstein's ether-less theory.[118] In his

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[118] Despite unequivocal textual evidence that Lorentz continued to believe in absolute simultaneity and a classical ether to the end of his life, there is no unanimous consensus in the secondary literature that he did. Zahar, for instance, has claimed that around 1914, Lorentz accepted special relativity. The following passage shows some of the (for the most part actually quite subtle) historical misunderstandings that add up to this conclusion: "... why was [Lorentz] not quickly converted to Relativity (he was of course finally converted around 1914)? In my opinion, it is because he had good empirical reasons for not accepting Maxwell's equations as fully covariant. In [the first edition of *The theory of electrons* (Lorentz 1916)] he mentioned Kaufmann's results as constituting serious evidence against the Relativity Principle. In the same book he reproduced his 1904 results [the transformation equations for charge and current density, I take it] without taking account of the corrections which Poincaré published in his [Poincaré 1905]. These corrections would have made Lorentz's electrodynamics Lorentz-covariant and hence indistinguishable from Einstein's. As soon as Lorentz found good reasons—such as the result of Bucherer's experiment [Bucherer 1908]—for accepting the covariance of Maxwell's equations, he realized that the ether had lost all heuristic value and consequently joined the relativistic camp. This conversion is clearly expressed in [Lorentz 1914] and in the footnotes to the 1915 edition of the *Theory of Electrons*" (Zahar 1978, pp. 50–51). This passage clearly illustrates, I think, how hard it is to

lectures on special relativity in Leiden in 1910–1912, published in 1922 under the title "The principle of relativity for uniform translations" (edited by Lorentz's former student Adriaan D. Fokker), Lorentz gave a more elaborate version of the arguments in this paragraph.

After showing, in somewhat more intuitive terms than in *The theory of electrons*, that two observers *A* and *B* at rest with respect to different inertial frames will find identical results in all experiments they perform, Lorentz explains that one can have two attitudes to the dispute between these observers as to whom is really at rest in the ether:

> With regard to such a dispute between *A* and *B*, tending to decide whose space-and-time determination deserves to be considered as based upon the absolute aether, two different attitudes are possible.
>
> There is a plurality of co-ordinate systems $x$, $y$, $z$, $t$; $x'$, $y'$, $z'$, $t'$, etc., such that a phenomenon can be described in terms of each of them by the same equations. All these systems can be derived from each other by certain transformations. All these transformations form what is called a group.
>
> Now, one possible point of view is to say that among all these systems there is one which in some respects is above all the others. Although it may be impossible to make out which it is, yet it is theoretically conceivable that a privileged co-ordinate-and-time system can be assigned.
>
> According to the other point of view all these systems are perfectly equivalent, and it is meaningless to say that there is one among them which in certain respects differs from the others.
>
> The dilemma is closely connected with what one thinks of the aether. (Lorentz 1922, p. 208)

In the next two subsections of Lorentz's 1910–1912 lectures, numbered 2.81 and 2.82, Lorentz pursues two rather different lines of thought that lead him to the "privileged co-ordinate-and-time system" mentioned in the passage quoted above, a frame of reference which is at rest in the ether and which can be used to decide on the true simultaneity of events. From a modern point of view, neither of these two arguments are cogent, of course. However, the first is much better than the second. I will examine both arguments.

The first argument starts immediately after the passage just quoted. Lorentz elaborates on what he thinks about the ether:

> The *aether* has been introduced in order that it may serve as a substratum, originally, for light, and later also for the electromagnetic phenomena. The need was felt for something substantial as the bearer of electromagnetic energy [...]
>
> [...] The aether was gradually deprived of various attributes of matter and, finally, during the last ten years the prevailing opinion was that the less we assume about it the better. The aether has to serve only as the bearer of electromagnetic phenomena [...] (Lorentz 1922, pp. 208–209)

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straighten out complicated historical misunderstandings. Comparing Zahar's remarks to my discussion of (a) Lorentz's attitude toward Kaufmann's experiment, (b) the problem concerning the transformation of charge density, and (c) the importance of distinguishing between the Lorentz invariance of Maxwell's equations (with and without sources) and the Lorentz invariance of the laws governing non-electromagnetic systems, the reader will see that none of Zahar's remarks are really accurate, although some of them do not miss by much. To borrow an analogy from John Norton: if you use stones that are all slightly askew, you end up with a house that is badly askew.

This is a very strong and time-honored argument. As we will see below, even Einstein would eventually come to accept it. A particularly clear version of the argument, albeit it in the context of optics rather than in the context of electrodynamics in general, is given by Maxwell in his article "Ether" for the ninth edition of the *Encyclopaedia Britannica* of 1875:[119]

> *Function of the æther in the propagation of radiation.*—The evidence for the undulatory theory of light will be given in full, under the Article on LIGHT, but we may here give a brief summary of it so far as it bears on the existence of the æther.
>
> That light is not itself a substance may be proved from the phenomenon of interference. A beam of light from a single source is divided by certain optical methods into two parts, and these, after travelling by different paths, are made to reunite and fall upon a screen. If either half of the beam is stopped, the other falls on the screen and illuminates it, but if both are allowed to pass, the screen in certain places becomes dark, and thus sh[o]ws that the two portions of light have destroyed each other.
>
> Now, we cannot suppose that two bodies when put together can annihilate each other; therefore light cannot be a substance. What we have proved is that one portion of light can be the exact opposite of another portion, just as $+a$ is the exact opposite of $-a$, whatever $a$ may be. Among physical quantities we find some which are capable of having their signs reversed, and others which are not. Thus a displacement in one direction is the exact opposite of an equal displacement in the opposite direction. Such quantities are the measures, not of substances, but always of processes taking place in a substance. We therefore conclude that light is not a substance but a process going on in a substance, the process going on in the first portion of light being always the exact opposite of the process going on in the other at the same instant, so that when the two portions are combined no process goes on at all. (Niven 1952, pp. 764–765)

Variants of Lorentz's and Maxwell's argument are routinely rehearsed by modern philosophers of space and time. It is perhaps the single most important argument in favor of manifold substantivalism in the context of field theories. John Earman, for instance, writes, rephrasing a point made by Hartry Field:

> When relativity theory banished the ether, the space-time manifold $M$ began to function as a kind of dematerialized ether needed to support the fields. In the nineteenth century the electromagnetic field was construed as the state of a material medium, the luminiferous ether; in postrelativity theory it seems that the electromagnetic field, and indeed all physical fields, must be construed as states of $M$. In a modern, pure field-theoretic physics, $M$ functions as the basic substance, that is, the basic object of predication. (Earman 1989, p. 155)

However, the next step in Lorentz's argument is easily seen to be fallacious from a modern point of view. Lorentz tacitly assumes that the space-time structure that has to be given some substantiality in order to serve as a substratum for the electromagnetic field has to be the Newtonian space-time structure. This is obviously false. What has to be given some substantiality, in modern terms, are points of the bare manifold. Lorentz's argument for substantivalism tells us nothing about the geometric object fields defined on the manifold and encoding the space-time structure. In other words, even if we insist on having some substratum

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[119] I am grateful to John Norton for drawing my attention to this passage.

for the electromagnetic field (which is perfectly reasonable), a Minkowski space-time would serve the purpose just as well as a Newtonian space-time. Not surprisingly, given that he did not think of space-time in terms of differentiable manifolds dressed up with metric fields and affine connections, Lorentz felt that his substantivalist argument could be used to make it respectable to cling on to the Newtonian space-time structure. In the paragraph immediately following the passage I quoted above, he writes:

> How much substantiality should we, then, attribute to the aether? Let us leave it so much substantiality as to be able to say with some significance that things can move or be at rest with respect to it. It will then be possible to lay in the aether a co-ordinate system which will differ from all others just by being fixed in the aether, and the time corresponding to this privileged system could be called the absolute time or, simply, *the* time. But then it would no longer be *a priori* certain that all other co-ordinate systems are as good, and that there are corresponding phenomena which can always be described in any of these systems by means of the same equations. In fine [sic], the validity of the principle of relativity would then assume a special character and could not be predicted *a priori* or expected.
>
> But if one insists that it is meaningless to speak of something being fixed in or moving through the aether, in fine [sic], if one abolishes the aether to the extent that not even a co-ordinate system can be laid down in it, then there is no possibility left to assign a system which would in some respect differ from the other systems. Then indeed all systems $x$, $y$, $z$, $t$ and $x'$, $y'$, $z'$, $t'$, etc., become equivalent. Conversely, if this equivalence is assumed at the outset, the aether must be entirely abandoned. (Lorentz 1922, p. 209; italics in original)

Einstein, of course, did assume the equivalence Lorentz is talking about from the outset. Hence, what Lorentz claims here is that, on Einstein's view, we cannot have a substratum for the field. Again, from a modern point of view, it is easily seen that this conclusion does not follow at all. However, it is my impression that, in the period under consideration (from, say, 1909 till 1912), it was widely accepted that it does. The disagreement only appears to have been about whether the alleged incompatibility of special relativity and what, in modern philosophy of space and time, would be called manifold substantivalism constituted a serious problem for special relativity or not. For Lorentz, it obviously did; for Einstein it did not. Lorentz was not alone in his assessment that this is a problem for special relativity. In his inaugural lecture in Leiden, from which I already quoted above, Ehrenfest, one of Einstein's early allies in special relativity, indicates that he finds this aspect of the theory hard to swallow. Ehrenfest contrasts Lorentz's ether theory with the emission theory of the late Walther Ritz, a friend of Ehrenfest from his days in Göttingen (Klein 1970, p. 5, p. 41). Ehrenfest emphasizes that in Ritz's theory, one has no trouble imagining light propagating through empty space without an ether. He then says

> But please notice that something entirely different is asked of us when we are admonished to deny the ether in the Einsteinian sense. In that case, we are asked to subscribe to the following three articles:
>
> 1. Light sources throw light signals at us as independent structures in empty space.
> 2. For light rays from a source which is approaching us and another which is at rest with respect to us we would find, upon actual measurement, the same velocity.
> 3. We declare that the combination of these two assertions satisfies us. (Ehrenfest 1913. p. 19; discussed and partly quoted and translated in Klein 1970, p. 5)

As Martin Klein points out in his discussion of Ehrenfest's lecture: "Ehrenfest did not discuss the basic revision of the concepts of space and time that lay at the heart of Einstein's theory; he limited himself to pointing out that the results of the relativity theory were indistinguishable from those obtained by Lorentz, despite the fundamentally different logical structures of the two theories" (Klein 1970, p. 5).

This underscores that we have to be careful using our modern understanding of Minkowski space-time in assessing the understanding of special relativistic space-time in the period 1909–1912 under consideration. We have to keep in mind that the transition from Newtonian space-time to relativistic space-time which Einstein proposed in his 1905 paper was not conceptualized in terms of a transition from one metric on the manifold to another, not even by Minkowski himself.

As a mathematician, Minkowski worked in the tradition of the so-called Erlangen program of his Göttingen colleague Felix Klein (see, e.g., Norton 1993b, p. 797, pp. 832–833). This means that he did not think in terms of a metric tensor field giving an amorphous bare manifold its spatio-temporal properties but in terms of symmetries and invariants of a fully dressed up space-time. As a mathematical physicist, Minkowski worked in the tradition of the electromagnetic view of nature of Wien and Abraham (Galison 1979). Recall that in his famous lecture "Space and time,"[120] he described his "world postulate"—which says that "only the four-dimensional world in space and time is given by the phenomena" (Minkowski 1909, p. 83)—as "the true nucleus of an electromagnetic image of the world, which, discovered by Lorentz, and further revealed by Einstein,[121] now lies open in the full light of day" (ibid., p. 91). As Jon Dorling has pointed out to me (private communication), Minkowski clearly thought that the Lorentz invariance of Maxwell's equations explains why the structure of space-time is Minkowskian rather than the other way around as in the modern understanding of special relativity.

On this modern reading, Minkowski's work can be used—and is, in fact, routinely used nowadays—to visualize the new space-time structure revealed by the rod and clock measurements discussed in Einstein's 1905 paper. Yet, Einstein himself had great difficulties recognizing the relevance of Minkowski's work for the conceptual clarification of the foundations of special relativity. As he later told his collaborator Valentin Bargmann, who, in turn, related it to Pais, Einstein initially considered Minkowski's work as "superfluous

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[120] This lecture was delivered before the *Versammlung Deutscher Naturforscher und Ärzte* in Cologne on September 21, 1908, two days before Planck's lecture on the principle of action and reaction from which I quoted extensively in chapters one and two.

[121] Never mind that most physicists associated Einstein and Lorentz with the opposition to the electromagnetic view of nature (see section 3.4).

learnedness" (*überflüssige Gelehrsamkeit*, Pais 1982, p. 152). After Einstein discovered the importance of the metric tensor for a theory of gravitation, he would come to appreciate the importance of Minkowski's conceptual contribution, but this was not until 1912. Around 1910, he had only come to appreciate the formal advantages of Minkowski's four-dimensional formalism, a formalism he had carefully avoided earlier in his work with Laub.[122]

By the time Einstein had finished general relativity, he was in a much better position to tell the good from the bad in Lorentz's arguments concerning the ether. He now accepted Lorentz's substantivalist argument, but argued that this does not entail a return to a pre-relativistic ether. Probably at Lorentz's suggestion, he made the issue the topic of his inaugural lecture on October 27, 1920 in Leiden, where Lorentz had arranged a visiting professorship for him. Einstein concluded his lecture, entitled "Ether and the theory of relativity," by saying:

> Recapitulating, we may say that according to the general theory of relativity, space is endowed with physical qualities; in this sense, therefore, there exists an ether. According to the general theory of relativity, space without ether is unthinkable; for in such space, not only would there be no propagation of light, but also no possibility of existence for standards of space and time (measuring rods and clocks), nor therefore any space-time intervals in the physical sense. But, this ether may not be thought of as endowed with the quality characteristic of ponderable media, as consisting of parts that may be tracked through time. The idea of motion may not be applied to it. (Einstein 1920, pp. 23–24; quoted and discussed in Kostro 1992, p. 269)

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[122] This can be inferred from a letter from Einstein to Sommerfeld of July 1910 (Klein et al. 1994, Doc. 211, pp. 244–247). Einstein praises Sommerfeld 1910a (Klein et al. 1994, note 9), an important contribution to the development of the four-dimensional formalism in special relativity. Apparently, Sommerfeld had expressed some concern that Einstein would not care much for this work. Einstein writes: "How can you think I would not appreciate the beauty of such an investigation? The study of the formal relations in four dimensions seems to me to be an advance, *like, for instance, the introductions of complex functions* in hydrostatics and electrostatics in two dimensions. I probably expressed myself incorrectly when I talked to you about this in Salzburg" (ibid., p. 246; my emphasis). The conversation Einstein is referring to took place at the *Versammlung Deutscher Naturforscher und Ärzte* in Salzburg in September 1909 (ibid., note 11).

Another interesting document in this context is a letter from Laub to Einstein of May 18, 1908 (Klein et. al, Doc. 101, pp. 119–121). Although this is not mentioned in the annotation in Klein et al. 1994, the portion of the letter about Minkowski is quoted, partly translated, and discussed in Pyenson 1976, p. 99–100. Laub writes: "It is quite curious what pleases Cantor [Matthias Cantor, theoretical physicist at the University of Würzburg (Klein et. al 1994, p. 120, note 10)] about the Minkowski paper [Minkowski 1908 (Klein et. al 1994, p. 120, note 7)]. He only values the treatment of the time and the coordinates as quantities of the same nature ($x_1$, $x_2$, $x_3$, $x_4$) [and] that one can treat it as rotation. He values this for epistemological reasons. To my question what it actually means, physically, to treat time as a fourth spatial coordinate (or *it*), he still owes me an answer. I believe he has let himself be impressed by non-Euclidean geometry [...] I am now even more skeptical about the Minkowski paper; were not your work available, we would at best have reached the same position with the Minkowski transformation equation for time (as far as the physical interpretation is concerned) as with the Lorentzian "local time."" (Klein et al. 1994, pp. 119–120; translation taken in part from Pyenson 1976, p. 99). Unfortunately, we do not know Einstein's reaction to these comments. Given the letter to Sommerfeld I quoted above; given that Einstein and Laub had just collaborated on a paper in the same field as Minkowski 1908, altogether avoiding his mathematics, which, they write, "poses great demands" on the reader (Klein et al. 1994, p. 121, note 12); and given Laub's reference to his earlier, albeit somewhat milder, skepticism, we can safely assume that Einstein shared Laub's assessment of Minkowski's work at this point. Let me emphasize, however, that the letter was written several months before Minkowski's famous "Space and time" address.

This is the clearest statement I could find on a quick scan of Einstein's pronouncements on the topic of ether and relativity. It is fair to say, I think, that what I take to be Einstein's point can be expressed much more forcefully simply by pointing out that the manifold does not have to be endowed with any particular metric field to serve as a substratum for the field.

Let me summarize my assessment of Lorentz's first argument for the existence of a privileged frame of reference among all the empirically equivalent ones. It is quite reasonable to insist on the existence of a substratum for the field. Einstein would come to accept this himself, and so do manifold substantivalists in modern philosophy of space and time. However, it does not follow from this substantivalist claim that there has to be a preferred frame of reference that could be used as the standard for absolute simultaneity.

I now turn to Lorentz's second line of reasoning leading to a privileged frame of reference. Lorentz writes:

> There is yet another way of picking out a particular system. This is associated with our notion of time.
>
> According to the old-fashioned view, to which we have all adhered some time, there is a "true" time, viz. an ever-increasing variable. Associated with this is the belief in a clear, unambiguous meaning of the concept of simultaneity. Time and space being separated from each other, simultaneity has an unambiguous meaning, independent of the place. [...] [W]e could pick out from a number of different space-time systems that in which truly simultaneous events are also called simultaneous. In this system, then, we would work with the true time and the true simultaneity, while in the other systems we would have to do with auxiliary mathematical magnitudes, with relative or "local" times. Notwithstanding this, two different observers could observe exactly the same phenomena, neither of them, nor a third one, being able to find out who is in possession of the unique true time. (Lorentz 1922, p. 209–210)

This is a rather feeble argument, as Lorentz himself seems to realize. After all, he introduces it without trying to hide that it is based only on the traditional notions of time and simultaneity that we happen to be accustomed to. The contrast with the first argument becomes especially strong if we look back at Lorentz's 1906 New York lectures of *The theory of electrons*. There Lorentz only alludes to the first argument. He writes:

> *I cannot but regard* the ether, which can be the seat of an electromagnetic field with its energy and its vibrations, as endowed with a certain degree of substantiality, however different it may be from all ordinary matter. In this line of thought it seems natural not to assume at starting that it can never make any difference whether a body moves through the ether or not, and to measure distances and lengths of time by means of rods and clocks having a fixed position relatively to the ether. (Lorentz 1916, p. 230; my italics)

As we will see below, Lorentz presents this argument in equally strong terms ("One cannot deny ...") in the concluding paragraph of chapter two of his 1910–1912 Leiden lectures. However, it would be a mistake to think that Lorentz did not take the second argument (starting

from absolute simultaneity rather than from the substantiality of the ether) seriously.[123] The argument returns in the draft of a letter to Einstein in 1915 from which I quoted in section 3.2 and in the closing section of his lectures at Caltech in 1922 (cf. Nersessian 1984, pp. 116–119; 1986, p. 232). As has been emphasized by A.J. Kox (private communication), the most telling passage occurs in the letter to Einstein (cf. Kox 1988, p. 210, note 33). At the end of the letter, after the remarks on simultaneity quoted by Nersessian, Lorentz adds the following remarks, explaining in a footnote that he will "enclose the following in parentheses, for with it I cross the boundaries of physics:"[124]

> A "world spirit," who would permeate the whole system under consideration without being tied to a particular place or "in whom" the system would consist, and for whom it would be possible to "feel" all events directly, would obviously immediately single out one of the systems [read: Lorentz frames] $U$, $U'$, etc. over all others. Although we are not such world spirits, yet we may not, if we adhere to the usual view of "mind" and "body," be so tremendously different from one. According to this view, namely, we must feel material processes taking place in the brain, and since one can hardly say that the mind has its seat at a specific point of the brain, it looks as if the mind could actually experience what goes on at different places in the brain and (given sufficient power of differentiation) could test this directly for "simultaneity." (Draft of a letter from Lorentz to Einstein, January 1915)

These remarks about a "world spirit," highly uncharacteristic for Lorentz, remind one of Newton's "Sensorium of God." The remarks show, I think, how deeply rooted Lorentz's sense of absolute simultaneity was.

To close this chapter, I want to quote the final subsection (numbered 2.9 and immediately following the subsections 2.81–2.82 examined above) of chapter two of Lorentz's lectures in Leiden in 1910–1912, a time when it still seemed plausible that Lorentz's substantivalist argument supported the pre-relativistic notions of space and time:

> We thus have the choice between two different plans: we can adhere to the concept of an aether or else we can assume a true simultaneity.[125] If one keeps strictly to the relativistic view that all systems are equivalent, one must give up the substantiality of the aether as well as the concept of a true time. The choice of the standpoint depends thus on very fundamental considerations, especially about the time.

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[123] I am indebted to A.J. Kox for preventing this unwarranted application of the "charity principle" on my part.

[124] The original German reads: "(Ein "Weltgeist", der ohne an einem bestimmten Ort gebunden zu sein, das ganze betrachtete System durchdränge, oder "in dem" dieses System bestände, und der unmittelbar alle Ereignisse "fühlen" könnte, würde natürlich sofort eins der Systeme $U$, $U'$, u.s.w. vor den anderen auszeichnen. Obgleich wir nun solche Weltgeister nicht sind, so dürfen wir uns doch, wenn wir uns an der üblichen Auffassung von "Geist" und "Körper" halten, doch nicht so himmelsweit davon verschieden. Wir müssen nämlich nach dieser Auffassung materielle in dem Gehirn stattfindende Vorgänge fühlen, und da man schwerlich sagen kann, der Geist habe in einem bestimmten Punkte des Gehirns seinen Sitz, so sieht es aus, alsob er wirklich was an verschiedenen Stellen des Gehirns vor sich geht, empfinden und (bei genügendem Unterscheidungsvermögen) direkt auf die "Gleichzeitigkeit" prüfen könnte.)" Notice that the second sentence is ungrammatical. Lorentz apparently switched mid-sentence from the construction "so dürfen wir uns davon nicht himmelsweit unterscheiden" to the construction "so sind wir davon nicht himmelsweit verschieden."

[125] The choice Lorentz presents here refers back to the two ways he has outlined in the preceding subsections for picking a preferred frame of reference, starting from the substantiality of the ether (2.81) or from absolute simultaneity (2.82), respectively.

Of course, the description of natural phenomena and the testing of what the theory of relativity has to say about them can be carried out independently of what one thinks of the aether and the time. From a physical point of view these questions can be left on one side, and especially the question of the true time can be handed over to the theory of knowledge.

The modern physicists, as Einstein and Minkowski, speak no longer about the aether at all.[126] This, however, is a question of taste and of words. For, whether there is an aether or not, electromagnetic fields certainly exist, and so also does the energy of the electrical oscillations. If we do not like the name of "aether," we must use another word as a peg to hang all these things upon. It is not certain whether "space" can be so extended as to take care not only of the geometrical properties but also of the electric ones.

One cannot deny to the bearer of these properties a certain substantiality, and if so, then one may, in all modesty, call true time the time measured by clocks which are fixed in this medium, and consider simultaneity as a primary concept. (Lorentz 1922, pp. 210–211)

[126] At this point, Fokker added a footnote saying "see, however, Einstein's address [Einstein 1920]."