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Mechanisms, principles, and Lorentz's cautious realism

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Abstract

I show that Albert Einstein's distinction between principle and constructive theories was predated by Hendrik A. Lorentz's equivalent distinction between mechanism- and principle-theories. I further argue that Lorentz's views toward realism similarly prefigure what Arthur Fine identified as Einstein's "motivational realism."

Keywords: Lorentz; Einstein; Realism; Mechanisms; Principle theories

> With all his devotion to scientific study,
> he nevertheless was perfectly aware that
> the human intellect cannot penetrate very deeply
> into the essential core of things. It was not until
> my later years that I was able fully to appreciate
> this half skeptical, half humble disposition.
> (Einstein, 1957, pp. 8–9)

1. Introduction

In a letter to *The London Times* in 1919 Albert Einstein famously distinguished what he called "principle-theories" from "constructive theories," explaining that the new theory of relativity is an example of the former kind (Einstein, 1954, p. 228). Einstein's distinction has received, especially in recent years, a fair amount of attention in the philosophical

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literature and it has become common, following Einstein, to characterize the difference between Einstein's theory and Hendrik A. Lorentz's pre-relativistic theory of the electron by appealing to this distinction. One of my main aims in this paper is to show that a distinction equivalent to Einstein's was drawn at least as early as 1900 by Lorentz himself, who distinguished theories positing principles generalized from experience from theories positing mechanisms (Lorentz, 1900b, c). As we will see, despite Lorentz's and Einstein's disagreement about the correct approach to the electrodynamics of moving bodies, both agreed in fact rather closely on the merits of the two kinds of approaches to physical theorizing in general.

A second aim of this paper is to locate Lorentz's claims concerning the respective roles of principle- and mechanism-theories within Lorentz's broader methodological and philosophical views on science—in particular with respect to what I take to be Lorentz's view on the issue scientific realism.[1] In the lecture in which Lorentz drew his distinction between the two types of theory he maintained that the ultimate goal of physics, pursued by all "great researchers," is to realize the Faustian dream of discovering "deep under the surface"

> how everything is woven together,
> one thing acts and lives through another.[2]
> (quoted in Lorentz 1900c, p. 348)

Lorentz consequently preferred mechanism-theories, for these theories embody the hope of uncovering hidden, underlying realities in a way that principle-theories do not. And this hope, of course, is the hope of scientific realism. Both mechanism- and principle-theories could, on a realist construal, be true, but the former promise to reveal the hidden springs of nature in ways the latter do not. Lorentz's characterization of the ultimate aim of science and his preference for mechanism theories seem to suggest a strong commitment to scientific realism.[3] Despite its initial plausibility, however, I will argue that this interpretation of Lorentz is mistaken or at least over-simplified. To the extent that it is correct to see Lorentz as a scientific realist at all, his realism is another instance of Lorentz's views prefiguring those of Einstein. According to Lorentz, our confidence that our best scientific theories in some sense correctly represent features of the natural world can ultimately be based on nothing but an inner urge of ours to trust these theories—an idea we find echoed in Einstein's notion of a "religious trust" at the foundation of all scientific theorizing.[4]

I will proceed as follows. In the next section I will present some of the physical background for Lorentz's methodological views and will sketch certain aspects of Lorentz's theory of the electron and its problems. In Section 3 I will discuss Lorentz's

[1]Lorentz nowhere presents a fully developed methodology of science and, in comparison even to Einstein, discussed philosophical questions only rarely in his writings. Yet there are enough *meta*-physical remarks interspersed in Lorentz's published works to suggest a substantive and interesting—even if not fully developed and argued for—'philosophy of science.'

[2]All translations from the German in this paper are my own. For the translations of this passage from *Faust* and the passages that I cite below I have consulted the translation by Martin Greenberg (Goethe, 1992).

[3]One might believe that mechanism theories ought to be interpreted instrumentally, but unless they are intended to uncover real underlying structures it becomes harder to understand what the general advantage of mechanism theories over principle theories may be.

[4]For a discussion of Einstein's realism see (Fine, 1996).

distinction between principle- and mechanism-theories and compare it to Einstein's later distinction. Einstein's distinction, I submit, is at least with equal right Lorentz's distinction. One point I will stress in particular is that while Lorentz personally clearly preferred mechanism-theories, he not only acknowledged that others may have differing preferences but also allowed that principle theories can play an important role in scientific theorizing and advocated what we might call a "theoretical pluralism."

In his discussion of mechanism-theories Lorentz explicitly adopted the core idea of Heinrich Hertz's picture theory of theories. In Section 4 I will discuss Lorentz's remarks on the question of how our theories represent the world, by comparing and contrasting Lorentz's views with Hertz's better known views. Finally, in Section 5, I will sketch out a realist position that, on the one hand, can do justice to Lorentz's invocation of the Faustian dream of uncovering what is hidden behind the appearances, and, on the other hand, is compatible with Lorentz's commitment to theoretical pluralism and to the Hertzian idea that theories provide us with mere apparent images of things. I will argue that Lorentz's realism—"half skeptical, half humble"—is remarkably similar to the motivational realism Arthur Fine attributes to Einstein (Fine, 1996).

What story should we tell about this close resemblance between Einstein's and Lorentz's philosophical views? Two hypotheses suggest themselves. The first sees the resemblance as sufficiently unusual to demand some kind of special explanation, perhaps by trying to argue that Einstein was far more deeply influenced by Lorentz than has perhaps previously been realized. The second hypothesis is that the views expressed by Lorentz and Einstein were rather more common than philosophical discussions of Einstein's views might suggest. The distinction between principle- and mechanism-theories, as well as Einstein's realism, may simply be expressions of views not uncommon among physicists (at least in the early twentieth century). Determining which of these two hypotheses is correct is beyond the scope of this paper. My aim here is simply to draw attention to a cluster of Lorentz's methodological or philosophical views that to my mind deserve more attention than they seem to have received to date.

2. The theory of the electron and the electromagnetic world picture

In this Section I want to summarize some of the main features of Lorentz's theory of the electron that are important as background for my discussion of his philosophical views. Since the historical development has already been examined in detail (see Hirosige, 1969; McCormmach, 1970; Miller, 1981; Nersessian, 1986), I do not wish to repeat such a discussion here and will only provide a brief sketch of some of the central points. Lorentz's theory of the electron was an attempt to provide a microscopic foundation for Maxwellian electrodynamics by combining an atomic conception of matter with a field theoretic treatment of electromagnetic phenomena. In contrast to Maxwellian electrodynamics, which did not clearly distinguish charged matter from electromagnetic fields, Lorentz's theory posited two independently existing and equally fundamental types of entities—an all pervasive ether as the seat of the electromagnetic field and matter which contains microscopic electric charges that interact with electromagnetic fields.

The basic question for the theory of the electron is how the two distinct systems of discrete, moving charges and the stationary ether interact. According to the theory there are two kinds of interaction. First, microscopic equivalents of the Maxwell equations specify the electromagnetic fields associated with a given charge and current configuration.

Maxwell's macroscopic theory involved four fields—the electric field strength **E**, the displacement **D**, the magnetic field strength **H**, and the magnetic induction **B**—which the Maxwell equations relate to macroscopic charge and current configurations. The relation between **E** and **D** and that between **B** and **H** are experimentally determined properties of material substances. Four different fields are required in Maxwell's theory, since the theory does not clearly distinguish a medium carrying the fields from the matter carrying the charges. By introducing such a distinction, Lorentz was able to reduce the number of fundamental fields. Since Lorentz's all pervasive ether has the same properties within matter as it does in free space, Lorentz's microscopic equations are the equivalent of Maxwell's macroscopic equations in vacuum and involve only two microscopic fields, $\mathbf{E}_{\text{micro}}$ and $\mathbf{B}_{\text{micro}}$, instead of the four macroscopic fields of Maxwell's theory needed to characterize the electromagnetic state of dialectric matter. According to Lorentz (1916, p. 8), the molecular conception offers a deeper insight into the nature of the phenomena, since postulating microscopic electric charges makes it unnecessary to introduce a dielectric constant, conductivity, or magnetic permeability for each substance empirically, and in addition allows us to ask how non-electromagnetic properties such as temperature and density are related to the electromagnetic properties of matter. One of the hallmarks of Maxwell's theory had been its synthesis of electromagnetic and optical phenomena. By introducing a molecular hypothesis into electrodynamics Lorentz hoped to achieve a far greater synthesis, comprising many different branches of physics, than that achieved by Maxwell.

The second kind of interaction between charges and fields is given by Lorentz's force law, which specifies the force on a charge distribution in an electromagnetic field. While Lorentz was not the first to write down an equation similar to the force law, only in his theory does the equation acquire the status of specifying a force between two independent dynamic systems. In Lorentz's theory, then, charges and fields mutually determine each another: the ether affects the motion of electric charges; and charge and current configurations act as sources of electromagnetic fields propagating in the ether. By clearly distinguishing between electromagnetic fields and microscopic fundamental constituents of matter that interact with these fields, Lorentz's theory was an important precursor to elementary particle physics and to modern field theories. Since the theory allows for two substances to exist simultaneously in the same place (namely the ether and matter), Lorentz's theory was in a sense the first step beyond classical physics, as Nersessian (1986, p. 234) argues.

Lorentz's microscopic reduction of macroscopic electrodynamics provided a rival picture to nineteenth century attempts to arrive at a mechanical foundation for electromagnetic phenomena. In mature presentations of his theory Lorentz introduced his basic microscopic assumptions axiomatically without providing a mechanical rationale for them: electromagnetic interactions are treated as being fundamental. But not only did Lorentz take electromagnetic phenomena to be irreducible to mechanics, he also hoped that the theory of the electron could provide explanations for other, prima facie non-electromagnetic phenomena and that eventually a wide range of seemingly disparate physical phenomena could be accounted for in terms of a single underlying electromagnetic foundation. Thus, he speculated that the theory of the electron might give rise to electromagnetic theories of chemical interactions, of the properties of metals (including their thermal properties), of intermolecular forces, and perhaps even of gravitational forces (see Lorentz, 1900c, p. 348). As the list of possible applications of the theory of the electron grew, the possibility emerged that microscopic electrodynamics might lead to a unified and universal physics—an electromagnetic theory of everything. According to Lorentz, the ''final aim of research'' was not merely to collect as many facts as possible but rather ''the deduction of the innumerable natural phenomena as necessary consequences of a few simple fundamental principles'' (Lorentz, 1878, p. 28)—an aim which the theory of the electron promised to fulfill.

Arguably the most promising aspect of the theory of the electron as far as the unifying and reductive project of finding an electromagnetic world picture was concerned was that the theory seemed to be able to account for the mass of the electron in purely electromagnetic terms. More than any other claim, the hypothesis of a purely electromagnetic mass appeared to have fueled the hope for finding such a world picture, since this hypothesis quite directly raised the prospects of an electromagnetic foundation for mechanics, thereby challenging centuries-old attempts to provide a mechanical foundation for all of physics. Lorentz and Abraham showed that the field carried by a moving charged particle—the particle's self-field—contributes to the particle's inertia, which quite naturally can be understood as an electromagnetic contribution to the particle's mass.[5] Contrary to the Newtonian assumption that mass is a constant parameter, the electromagnetic mass is velocity dependent. Walter Kaufmann's experiments from 1901 to 1905 on the velocity dependence of the mass of the electron, which in 1906 Lorentz called ''one of the most important results of modern physics'' (Lorentz, 1916, p. 43), seemed to provide crucial experimental support for the hypothesis of an electromagnetic mass.[6] In fact, both Lorentz and Abraham took Kaufmann's data to be compatible, at least initially, with the assumption of a purely electromagnetic mass.

Even though Kaufmann's data did not exclude the possibility of a small mechanical contribution to the electron's mass in addition to its electromagnetic mass, Lorentz thought that one ought to take the mechanical mass to be zero ''with a view to simplicity'' (Ibid.), since it would amount to an ''unnecessary dualism if we considered [the electric] charges and what else there may be in the particles as wholly distinct from each other'' (Ibid., p. 45). Considerations of simplicity also suggested to Lorentz that all forces between particles may be regarded as connected more or less intimately with those we study in electromagnetism,'', since all forces are transmitted by the ether and one can ''hardly admit that one and the same medium is capable of transmitting two or more actions by wholly different mechanisms'' (Ibid., p. 46.)

The importance Lorentz attached to Kaufmann's experiment is also evident in a speech to the ''Elektrotechnischer Verein'' in Berlin where Lorentz said that

> the result obtained by Kaufmann encourages us to further deliberate the question raised by several people whether there is any true mass at all. If the assumption [of a purely electromagnetic mass and all of matter composed of electrons] was confirmed, then instead of interpreting electromagnetic phenomena mechanically, one would have to interpret mechanical phenomena electromagnetically. (Lorentz, 1904, p. 101)

He continued: ''For you, gentlemen this would have the happy consequence that all of engineering would fundamentally be electrical engineering.'' But Lorentz also added a word of caution to his speculations and said: ''But we are still far from reaching that goal.''

[5]See, for example, Lorentz (1916, pp. 39, 212) and Abraham (1908, p. 127, 169ff).

[6]For a detailed discussion of the various experiments performed by Kaufmann see Miller (1981).

(Ibid.) Thus, despite his obvious attraction to an electromagnetic world picture, Lorentz was cautious in his embrace of the view. Not only did he emphasize that it is far from conclusively established, but he also distances himself from the view by attributing the core assumption of the world picture to "several people," instead of presenting it as his own hypothesis.

Cautious remarks such as these are generally presented in the literature as a reflection of Lorentz's worries about specific scientific problems of the theory of the electron. The theory faced two kinds of problems—problems of empirical adequacy and foundational problems. McCormmach ties Lorentz's hesitancy to fully adopt the electromagnetic world picture to questions concerning its empirical adequacy (McCormmach, 1970). He suggests that Lorentz's support for the electromagnetic world picture was greatest around 1900 and traces what he takes to be Lorentz's increasingly ambivalent attitude toward that world picture first and foremost to the discovery of non-classical radiation phenomena that were eventually to lead to the development of quantum theory. As support for this reading one can cite the fact that Lorentz was acutely aware of this problem. For example, Lorentz discussed black body radiation in (Lorentz, 1904, pp. 120–121), conceding that his theory only agrees with Planck's formula for long wavelengths.

The foundational problems of the theory concerned the question of the stability of the electron and the so-called '4/3-puzzle.' Arthur Miller, who in contradiction to McCormmach's view claims that from 1900–1903 Lorentz "was as yet somewhat cautious about supporting an electromagnetic world picture" (Miller, 1981, p. 67) relates Lorentz's caution in embracing the reductive project of an electromagnetic world picture primarily to such foundational problems. For the project to be successful, one has to be able to provide a satisfactory electromagnetic model of the object at the center of the project—that is, the electron. And here the project encountered serious difficulties. Lorentz's theory of the electron, as well as its most fully articulated rival, Max Abraham's theory, treat the electron as extended particle. The models they considered were that of a uniformly charged sphere or that of a sphere with a uniformly distributed surface charge. The obvious problem with any such model is that since the field associated with one part of an extended charged particle results in repulsive forces on all other parts of the charge, an extended charged particle should be unstable and blow apart. Thus, the worry is that the electromagnetic world picture is inconsistent with the existence of the fundamental particles at its very core.

Abraham tried to solve the stability problem by simply postulating that electrons are perfectly rigid objects that are spherical even if they move with respect to the ether rest frame and argued that in analogy to constraints on rigid bodies in classical mechanics the constraints that keep the electron rigid do no work and need not be accounted for in terms of additional non-electromagnetic forces (see Abraham, 1908, p. 130). Yet Abraham's proposal appears to be avoiding the stability problem rather than solving it. For while it may be appropriate to invoke brute constraints in a theory like rigid body mechanics that is not intended to be fundamental, Abraham's appeal to such constraints in the context of a fundamentally purely electromagnetic world picture appears dubious, since it is unclear how the existence of such constraints could be compatible with there being nothing but electromagnetic interactions.

Lorentz's main objection against Abraham's electron model was that modeling electrons as perfectly rigid spheres is "at variance with [Lorentz's] theorem of corresponding states" (Lorentz, 1916, p. 210). In his book Lorentz proved two different results to which he referred as "theorem of corresponding states." The first, more restricted version establishes that if the Maxwell equations allow a certain configuration of charges and fields in a system at rest (in the privileged ether frame) then they allow the same configuration in an inertial frame moving through the ether, where the fields now are 'fictitious' fields in terms of 'fictitious' coordinates, which are related to the real fields and coordinates through what amount to the Lorentz transformations (Lorentz, 1916, Section 162). Lorentz showed that it follows from this theorem that the electromagnetic "force **f** with which the ether acts on unit [sic] of electric charge" (Lorentz, 1916, p. 198) transforms differently from the acceleration (Lorentz, 1916, p. 203), which implies that there cannot be "a theorem of corresponding states [...] unless we give up the equality of masses in [the system of rest and the moving system]." (Lorentz, 1916, p. 204) Lorentz's second theorem of corresponding states "of wider scope" assumed in addition that *all* forces transform in the same manner as electromagnetic forces, including the elastic forces that govern vibratory motions of electrons (Lorentz, 1916, Section 175, see also Section 172). This allowed Lorentz to derive formulae for the transformation of the ('longitudinal' and 'transverse') mass of the electron, which differ from the relations derivable from Abraham's model.[7]

Thus, by contrast with Abraham's rigid electrons, Lorentz assumed that electrons are deformable and are flattened into ellipsoids, when they have a non-zero velocity in the ether rest frame. But in the case of deformable electrons the stability problem arises with added force. Poincaré proposed in 1905 that the electron's stability might be ensured by internal non-electromagnetic cohesive force, and this proposal was adopted, at least tentatively, by Lorentz, even though he argued that Poincaré's internal stresses can only ensure the stability of the electron for the case of electrons moving with constant velocity.

Poincaré's internal stresses also appeared to be needed to help with another problem—the 4/3-puzzle. Abraham showed that the velocity dependent mass of Lorentz's electron (more precisely, the 'longitudinal mass') that one can derive from the electron's electromagnetic momentum differs from the expression for the mass that one can derive from the electromagnetic energy by a factor of 4/3.[8] Adding Poincaré stresses appeared to solve this problem since the stresses contribute to the momentum of the electron reducing the mass by exactly a factor of 1/3, but the stresses do not affect the internal energy of the electron.[9] Of course postulating non-electromagnetic internal cohesive forces is in direct conflict with the idea of a *purely* electromagnetic world picture, which is one of the reasons why Abraham objected to Lorentz's model of a deformable electron (Abraham, 1908, p. 195).[10]

[7]See, Janssen (2002) for a discussion of Lorentz's theorem of corresponding states. Janssen distinguishes the theorem of corresponding states (what I have referred to as the more restricted theorem) from a physical assumption: "the generalized contraction hypothesis." It is worth pointing out that Lorentz did not see the contraction hypothesis as basic, but thought of it as a consequence of the assumption that all forces transform in the manner of electromagnetic forces and that, as we have seen, Lorentz thought of both the restricted and the more general results as a "theorem of corresponding states."

[8]This also meant that the expression for the relativistic four-momentum of Lorentz's electron does not transform like a four-vector. Put this way, however, this would not have been an objection Abraham would have raised, since he rejected the 'Einstein–Lorentz theory' of relativity.

[9]Thus, it appeared initially that the 4/3 puzzle and the stability problem are inseparably connected. As Rohrlich (1990, p. 17) discusses, however, the 4/3 puzzle can be traced to the definition of the four-momentum and can be avoided if one chooses an alternative definition. The stability problem, by contrast, cannot so be avoided.

[10]As McCormmach argues (1970, p. 489) many younger physicists who embraced the electromagnetic world picture were critical of both Lorentz's and Einstein's theories, which they initially took to be equivalent, because

While perceptions of the empirical and foundational problems of an electromagnetic world picture were evolving during the first decade of the nineteenth century, reservations about fully embracing the electromagnetic world picture appear to have been a constant accompaniment to Lorentz's discussions of that view throughout the first decade of the twentieth century. For example, in 1901 he said:

> Perhaps matter must be imagined entirely to be made out of electrons, but there is nevertheless an 'ordinary' mass, as well as the electromagnetic mass. Whether the whole of mechanics will have to be constructed on an electromagnetic basis, as W. Wien thinks, or whether we should treat electromagnetic phenomena by means of ordinary mechanics, only the future will tell. For the moment much remains to be done in addressing simple problems and many difficulties still have to be surmounted. (Lorentz, 1901, pp. 109–110)[11]

And this assessment is closely echoed in the remarks from 1904 that I cited above. Lorentz's caution in supporting the electromagnetic world, it appears, is a reflection of a cautious attitude towards fully embracing the results physical theorizing in general and also affects Lorentz's methodological or philosophical views, as we will see below.

3. Mechanism-theories and principle-theories

Despite the interest that Einstein's distinction between principled and constructive theories has received, it has apparently not been noticed that an equivalent distinction had been drawn earlier by Lorentz. In 1900c, Lorentz distinguished theories that begin by postulating "general principles" (p. 335, see also 1904, p. 83) or "general laws" (p. 336) from theories that attempt to account for the phenomena by postulating a "mechanism of the appearances" (p. 336, see also 1904, p. 82). As examples of general principles he cited the principle of energy conservation and the second law of thermodynamics. Such general principles, Lorentz said, express "generalized experiences" (1900c, p. 337). Examples of theories postulating mechanisms, according to Lorentz, are the molecular kinetic theory of gases, Kelvin's vortex theory, and Hertz's mechanics of concealed motions.[12] Clearly, when Lorentz referred to a "mechanism of the appearances" the mechanism need not be *mechanical* in the sense of a Newtonian mechanical world picture. By "mechanism" Lorentz appears to have meant any underlying (micro-)process independent of its nature. An electromagnetic and a mechanical world picture can both be mechanistic in this sense, even though they postulate different types of underlying mechanisms. Important to Lorentz's idea of a mechanism appears to be that a mechanism-theory provides us with

(*footnote continued*)
they saw the "mechanical-relativistic postulate" as a throwback to the old mechanical world picture. Yet the real threat of Einstein's relativity postulate (as opposed to Lorentz's theory) to the electromagnetic world picture was that it removed the grounds for believing in a purely electromagnetic mass, since all mass becomes velocity dependent.

[11]I would like to thank David Atkinson with help in this translation from the Dutch original.

[12]One might be tempted think that in 1900c, Lorentz was drawing a three-fold distinction between (1) general laws, (2) theories of underlying mechanisms, such as the kinetic theory of gases, and (3) truly foundational theories, such as Kelvin's vortex theory. But since Lorentz only introduced two different terms for the different levels of theories and since Lorentz's terminology here closely resembles the terminology in (1904) and (1905), I think it is more plausible that Lorentz took both the kinetic theory and Kelvin's vortex theory to be examples of theories postulating mechanisms, differing only in the degree of "speculation" involved (see 1900c, p. 337).

some physical picture or 'model' of an underlying process. (Lorentz, 1904) seems to have distinguished a third kind of theory: theories that "without concern for the hidden mechanism of the goings-on" "summarize a large domain of appearances in a system of a few equations." (p. 83) These theories are distinct both from principle-theories, since the systems of equations are not simply generalizations of experiences, and from mechanism theories.

Those familiar with Einstein's distinction between different kinds of theories will recognize how closely Lorentz's (1900b, c) distinction resembles Einstein's later distinction. "Constructive theories," according to Einstein, "attempt to build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out." (Einstein, 1954, p. 228) And, echoing Lorentz's discussion of mechanism-theories, Einstein cited the kinetic theory of gases as an example of such a theory. Einstein contrasts constructive theories with "principle-theories:" "These employ the analytic, not the synthetic method. The elements which for their basis and starting point are not hypothetically constructed [such as the hypothesis of molecular motion] but empirically discovered ones, general characteristics of natural processes." (Ibid.) Thus, like Lorentz's theories appealing to general principles, Einstein's principle-theories are generalizations of experiences, and like Lorentz, Einstein took the laws of thermodynamics to be a paradigm example of such theories. Even though there are of course slight differences in formulation, the resemblance between Lorentz's and Einstein's distinctions is striking. Both appealed to the same paradigm example—the contrast between thermodynamics and the kinetic theory of gases—and both characterized the former theory to be based on generalizations from experience. Where Lorentz spoke of postulating "mechanisms," Einstein speaks of "constructing" a picture of complex phenomena out of simpler building blocks. These building blocks, according to Einstein are "hypothetically constructed," while for Lorentz they involve "speculation." I do not know what the correct explanation for these similarities is, but it is quite possible that Einstein owed the distinction to Lorentz and may well have been familiar with Lorentz's (1900c) account of the distinction, which was published in the *Physikalische Zeitschrift*.

It appears to be quite common to suggest that Einstein's discovery of the theory of special relativity and Lorentz's apparent hesitancy in adopting the theory are rather closely related to differences in their attitudes toward principle-theories, on the one hand, and constructive or mechanism theories, on the other. The special theory of relativity is a principle-theory, while Lorentz's theory of the electron is a mechanism-theory and this difference between the theories is supposed to be mirrored by and explanatorily related to a difference in attitudes—Einstein's alleged preference for principle-theories and Lorentz's preference for mechanism-theories. Yet how the two physicists characterized the roles of the two types of theory is in fact rather more similar than this received view suggests.

What, according to Lorentz, ought the role of different kinds of theory to be in scientific theorizing? Lorentz recognized that general principles play an important role in scientific theorizing and that they can give us "insight" (Lorentz, 1900c, p. 335) into the phenomena. The advantage of general principles is that they are versatile and apply to a wide variety of phenomena, since they abstract from and are independent of "the inner constitution of bodies." (Ibid.) Yet Lorentz thought that this advantage was at the same time a disadvantage: "To operate with such general basic principle also has its drawbacks." Using general laws teaches us "nothing or only very little about the mechanisms of the appearances" and thus may "lead us to desirable results, but will not show us much during the trip.'' (Ibid.) Therefore an explanation in terms of general laws ''is only partly satisfactory'' (pp. 335–336).

By contrast, theories concerned with the mechanisms of the appearances, according to Lorentz, at least partly satisfy our explanatory demands in ways in which appeals to general principles do not. Such theories, he said, provide us with ''flawed yet lively and clear representations'' of ''the connections between and the nature of things'' (p. 336). As the example of the kinetic theory of gases, according to Lorentz, shows, the explanatory advantage of assumptions about underlying mechanisms consists in the fact that they can be used to extend and broaden our understanding of particular physical systems. Yet Lorentz also acknowledged that there are potential drawbacks to postulating mechanisms as well, since it is the more risky of the two approaches: the latter kind of theories are ''daring'' while the former, ''phenomenological'' theories are ''careful''. (Lorentz, 1905, p. 56, see also p. 67.) Mechanism-theories are always in danger of becoming too speculative: ''Of course the scientist has to be cautious with such speculations so as not to be carried away by his imagination.'' (Lorentz, 1900c, p. 337.) And, indeed, Lorentz continued, ''only when there is absolutely no other way out to be found'' scientists will ''dare to diverge from the generalized experiences'' embodied in principle-theories (Ibid.)

Even though theorizing based on general principles is less risky, since these principles are generalizations from experiences, such principles are not unrevisable, according to Lorentz. In postulating a mechanism, Lorentz believed, general laws or principles such as energy conservation serve as constraints, which can, however, be abandoned as a last resort (Lorentz, 1905, p. 56), and Lorentz himself was willing to give up Newton's third law and the principle of momentum conservation in the theory of the electron to retain his constructive hypothesis of a strictly stationary ether (Lorentz, 1916, p. 31).

How does Lorentz's assessment of the two types of theory compare to Einstein's assessment? Einstein said that ''the advantages of the constructive theory is completeness, adaptability, and clearness, those of the principle theory are logical perfection and security of foundation'' (Einstein, 1954, p. 228). The very last characterization echoes Lorentz's claim that principle-theories are less risky and speculative than mechanism-theories and Einstein's idea that constructive theories are complete and adaptable has a counterpart in Lorentz's claim that mechanism theories ''help us to unearth what is still hidden and lead us to new investigations and perhaps new discoveries'' (Lorentz, 1900c, p. 336). Yet perhaps the most surprising similarity in their views concerns the issue of understanding and explanation. As we have seen, Lorentz thought that explanations based on general principles are only partly satisfactory: principle-theories can lead to ''insight,'' in that they point to the ''general direction'' in which natural phenomena occur, but they don't ''show us much during the trip,'' en route to the results[13] (Lorentz, 1900c, p. 335). Einstein agreed with Lorentz that mechanism theories are more explanatory and if anything appears to have been more unequivocal in his dismissal of the explanatory potential of principle theories: ''When we say that we have succeeded in understanding a group of natural processes,'' Einstein said, ''we *invariably* mean that a constructive theory has been found which covers the processes in question'' (Einstein, 1954, p. 228, my emphasis). That is, in an essay that is devoted to explaining (and promoting!) the theory of relativity as a

[13]I am here assuming that the notions of explanation, understanding, and of gaining insight are closely related: to gain insight into a phenomenon is to increase one's understanding of the phenomenon and explanations provide understanding or insight.

principle-theory, Einstein said explicitly that we do not take ourselves to understand a set of phenomena unless we have found a constructive theory of these phenomena![14]

Thus, we can point to aspects of Lorentz's and Einstein's views that appear to turn the standard story of the difference between their respective attitudes toward scientific theorizing on its head: While Lorentz thought that mechanism-theories should only be pursued as a last resort when all else fails, Einstein believed that such theories are necessary for successful understanding. Yet this characterization would, on its own, be equally as misleading as the claim that Lorentz strongly preferred constructive theories while Einstein in his work on the electrodynamics of moving bodies came to prefer a principled approach to physical theorizing. Both characterizations ignore another important similarity between Einstein's and Lorentz's view—the fact that both Einstein and Lorentz saw the pursuit of *both* types of approaches as useful and as potentially scientifically fruitful. This is evident from Einstein's discussion of the respective advantages of the two approaches and also from the quotes from Lorentz above.

And, indeed, Lorentz was quite explicit in advocating theoretical pluralism. He stressed repeatedly that opting for one approach to scientific theorizing rather than another is a matter of personal preference and that different approaches can be successful in different ways. Thus, he prefaced his discussion of the distinction between principle theories and mechanisms in 1900c by saying: "There are multiple ways by which we try to understand natural phenomena [...] Individual characteristics and inclinations determine the choice for each scientist" (Lorentz, 1900c, p. 333). The suggestion here is clearly not only that scientists do make different (and incompatible) choices based on their inclinations but also that there can be multiple approaches none of which can be excluded based on purely scientific arguments. And this idea is echoed in later writings. In his 1904 overview over the successes and problems of the theory of the electron, after laying out the basic assumptions of his atomistic approach to electrodynamics, Lorentz said: "It has to be acknowledged that in many cases other [non-mechanism] approaches, where one as much as possible follows general, universally accepted basic principles, can be pursued with equal or even larger success." (Lorentz, 1904, p. 83). As an example he mentioned an approach to magneto-optical phenomena that abstracts from any underlying mechanisms and that, as he said, has been successful in ways that his own theory has not been. Lorentz's view appears to have been that even in cases where there may be ways in which to rank the successes of different theories as far as one particular phenomenon is concerned, no overall ranking taking into account different phenomena needs to exist. Different theories may be successful at accounting for different phenomena, even within what is intuitively the same domain. Continuing his discussion of rival theories Lorentz said:

> Even though such considerations [of the different successes of different theories] should warn us against taking a certain opinion to be the best or most satisfactory, they should not prevent us from advancing as far as possible on the path that strikes us as the most promising. Science can only benefit, if everyone does that in his own manner. (Lorentz, 1904, p. 83)

[14]It is perhaps equally curious that Einstein says that an advantage of constructive theories over principle-theories is the 'clearness' of the former. Nersessian (1986) argues that Einstein only reluctantly gave up a constructive approach to the electrodynamics of moving bodies.

Thus, not only did Lorentz believe that there may be no general argument for the overall superiority of one particular approach to scientific theorizing, but he maintained that science would be at its most successful, when it allowed for a plurality of different competing approaches.

Lorentz's commitment to theoretical pluralism applied to differing constructive approaches as well. Thus in his 1907 lecture discussing the advantages of atomism he also mentioned a rival view:

> Experience shows that many theories in which one thinks of matter as continuously extended satisfy us, that some physicists decidedly prefer this conception and would rather avoid molecular views, and that many [..] have no reservations against conceiving of the ether as continuum. (Lorentz, 1907, p. 14)

Since assessments of a theory's successes may change with time we should be cautious in our acceptance of even the most successful theories at any given time and it may be prudent not to abandon certain approaches completely during periods when they appear less useful. We can see Lorentz adopting both these attitudes. Thus, on the one hand, even in 1900 (when according to McCormmach Lorentz's confidence in the electromagnetic world picture was at its largest) he qualified his support: "Among the theories of contemporary physics there is one group—we can call them the *electromagnetic theories of physical phenomena*—which I see as so promising in *the near future* that …" [the second set of italics are my emphasis] On the other hand, even as Lorentz himself was actively contributing to the general theory of relativity during the second decade of the twentieth century, he argued that it was premature to completely abandon the concept of the ether since it might still at some time in the future turn out to be scientifically fruitful (Lorentz, 1919, p. 274). One might simply try to discount this remark as a sign of a conservatism which may have made it impossible for Lorentz fully to accept the new physics. But since the remark is consistent with Lorentz's earlier avowal of theoretical pluralism (at a time when his own theory was the most promising candidate under consideration), it would be more charitable to see them similarly as expressions of such a pluralism.

To sum up, in this section I showed that Einstein's 1919 distinction between principled and constructive theories closely resembles a distinction between principle- and mechanism-theories drawn almost two decades earlier by Lorentz. I argued that Einstein's and Lorentz's view on the respective merits of the two types of theories are rather similar. Both took both principle- and a mechanism-approaches to theorizing to be of value; and both saw principle-theories as more secure or less risky but agreed that mechanism- or constructive theories are more explanatory. And both seemed, on balance, to prefer mechanism or constructive theories.

4. Theories and *Scheinbilder*

Lorentz's theoretical pluralism already casts doubt on the characterization of Lorentz as a 'hard-nosed' realist who takes the foremost aim of science to be to uncover the mechanisms behind the appearances. This characterization is further called into doubt by the manner in which Lorentz adopted (at least certain parts of) Hertz's 'picture theory' of theories. After having drawn the distinction between principle- and mechanism-theories, Lorentz characterized the process of postulating underlying mechanisms as one of producing "'innere Scheinbilder' [inner apparent images], or symbols of external objects", attributing this expression to Hertz.[15] In making use of these images, Lorentz said, we assume that "whatever we can deduce from them according to the laws of our capacity of thought corresponds to what occurs outside of us according to the laws of nature." (Lorentz, 1900c, p. 337). This is a close paraphrase of Hertz's famous passage in the introduction to the *Principles of Mechanics*, where Hertz said:

> We form for ourselves inner apparent images or symbols of external objects, and we do this in such a manner that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the objects pictured. (Hertz, 1910, p. 1)

Hertz went on to elaborate his remark as follows:

> [These images] are our representations of objects; in satisfying the above-mentioned demand they agree with objects in one important respect. But it is not important for their purposes that there is any further agreement between them and the objects. In fact, we do not know and have no means of finding out whether our representations of objects agree with the objects in any other than this *one* fundamental respect. (Hertz, 1910, p. 2)

The view appears to be this. Our theories contain representations of external objects. That is, our theories provide us with images or pictures of the things in nature. But these representations are mere *Scheinbilder* or apparent images of external objects whose purpose is not by themselves to represent the objects faithfully.[16] That is, the images with which theories provide us are representations of objects yet are not intended to *resemble* the objects represented. They function as symbols of objects, and like linguistic symbols need not resemble the objects they stand for. The only agreement between our images and the objects of nature for which we can reasonably aim in our theories is an agreement between *relations* that different images bear to one another and *relations* among the objects they represent. More specifically, the logical ("denknotwendigen") relations between images ought to mirror lawful relations existing in nature.[17]

As the quote above shows, Lorentz seemed to believe that Hertz's picture theory applied to mechanism-theories at least to the following extent: the relations between the representations of the phenomena posited by a mechanism-theory are meant to correspond to relations among the objects of our representations. But, importantly, our representations of the phenomena have the status of being mere *Scheinbilder*. To call representations "Scheinbilder" is not merely to assert that we form images of things, but that these images have a character akin to illusions or mirages: They purport to represent the objects in a certain way, but do in fact misrepresent them. Our representations are not simply *Bilder*, or images, but are *Scheinbilder*—that is, they are illusory or mere apparent images.

Hertz maintained that we can have no confidence that our images agree with their objects in any other way beyond an agreement in relations. But why should we have any confidence that there is even an agreement between the inferences we draw from internal

[15]Lorentz used the German expression "innere Scheinbilder" even in the Dutch original (Lorentz, 1900b, p. 10).

[16]Thus the translation of "Scheinbilder" as "pictures" or "images" (see Hertz, 1956, p. 1) is inaccurate in that it does not capture the idea that our images are only *apparent* images.

[17]Hertz's view, thus, bears some resemblance to the cluster of views that today are often referred to as *structural realism*.

images and the lawful relations between objects? What, if anything, justifies our use of our mental images? Hertz maintained that for this kind of agreement to be possible there would have to exist "certain correspondences between nature and our minds," and he believed that "experience teaches us that […] such correspondences do in fact exist" (Hertz, 1910, p. 1). Thus for Hertz, the empirical success of a theory justifies our belief that it represents lawful relations between objects correctly.

Lorentz agreed with Hertz insofar as he, too, believed that it is a presupposition of all scientific theorizing that there is a correspondence between nature and the mind. After contrasting atomic theories with continuum theories and conceding that continuum theories "may satisfy us" he said:

> This does not exclude the fact that if in other cases atomistic theory is more prone to provide us with clear insight, then this cannot merely be due to the nature of things outside of us but must also be due to the constitution of our minds. In general, comprehending a natural phenomenon presupposes a certain kinship between it and the mind." (Lorentz, 1907, p. 14)

But, contrary to Hertz, Lorentz did not seem to have believed that the presupposition of a basic kinship can be justified by appealing to experience or by appeal to a rational argument. Instead Lorentz said

> that we have every right to proceed in this manner [by constructing internal images that we take to represent relations among objects in nature] we base on [literally: "we borrow from"] the inner urge of our minds to trust in the success of those theories we already had luck in finding. We could not have formed the kind of images demanded by Hertz, if there was no kinship and agreement between our mind and nature that saves us from being completely mistaken. (Lorentz, 1900c, p. 337)

How to interpret this passage correctly, is not completely obvious, but I take Lorentz to say the following. In the second sentence Lorentz, in agreement with Hertz, seems to have expressed the thought that, in order to form images which meet Hertz's criterion of bearing relations among one another that correctly represent relations among the objects, there needs to be a kinship between our minds and nature. But this expresses merely a necessary condition of having successful representations but provides no grounds for believing that our representations do in fact succeed in the way demanded by Hertz. For we have no reason for believing in the kinship between our minds and nature independently of the success of our representations. The first sentence speaks to our justification for taking our representations to represent external relations successfully. And here, by contrast with Hertz's appeal to experience, Lorentz suggested that all scientific theorizing is ultimately grounded in or based on something that is itself not rationally justifiable—an "inner urge" to trust that our theories get something right about the world. Since Lorentz saw the need to appeal to a basic "trust" from which we "borrow" our right to have confidence in our theories, he appeared to have believed that no rational arguments can justify that confidence. While we might have good reasons to believe that the empirical consequences of a theory are true, our belief that the underlying mechanisms posited by a theory resemble relations between underlying entities cannot ultimately be justified. To the extent, then, that Lorentz was a scientific realist at all, his realism was distinct from contemporary philosophical variants of the position in that it was explicitly not grounded in rational arguments—instead of appealing to experience and what might provide the best explanation for our experiences, Lorentz invoked an inner urge to trust successful theories.

What decides which theories or images of the objects are best? Hertz maintained that the requirement that "the consequents of the images should be images of the consequents" is not sufficient to determine a theory uniquely. Thus, in addition to comparing theories with respect to this requirement, which Hertz called "correctness," we also need to distinguish theories according to their "purposefulness" ("Zweckmässigkeit") (Hertz, 1910, p. 2). The purposefulness of a theory, according to Hertz, depends on two criteria: First a theory is more purposeful than a competitor, all other things being equal, if it represents more of the essential relations between its objects. Hertz said that such a theory provides us with a more "distinct" image. Second, a theory is more purposeful, all other things being equal, if it is simpler—that is, if it contains fewer "superfluous or empty relations" (Ibid., pp. 2–3). The two criteria pull in opposite directions and the most purposeful theories are those that provide the best balance of the two criteria. A theory would appear to be most purposeful, according to Hertz's criteria, if the necessary relations in thought between its images would represent *all and only* the lawful relations between the images' objects in nature. In a more modern idiom, a theory would be maximally purposeful if the (set-theoretic) structure defined by our images as domain and their *denknotwendigen* relations was isomorphic to the structure defined by the external objects and their lawful relations. However Hertz insisted that empty relations cannot be entirely avoided in our theories. The reason for this, Hertz said, is that since the images are images in our mind they are partly determined by its representational properties. Thus, while Hertz postulated "correspondences between nature and our minds," he did not seem to believe that there could be a perfect match.

As far as I know, Lorentz nowhere discussed criteria of theory evaluation in much detail. But he said on several occasions that the best scientific defense of any theory "lies in its fruitfulness and utility," (1907, p. 14; see also 1904, p. 82) an expression that appears to echo Hertz's criteria, and he frequently invoked simplicity as a feature a good theory should exhibit. He also mentions a further constraint not discussed by Hertz on our construction of images:

> All our theories help us to form pictures, or images, of the world around us, and we try to do this in such a way that the phenomena may be coordinated as well as possible, and that we may see clearly the way in which they are connected. (Lorentz, 1922, p. 221)

Thus, Lorentz thought that we construct images of the world with the aim to unify different phenomena.

Like Hertz, Lorentz appears to have believed that empty relations are a common feature of scientific theories. In the letter to Einstein quoted above he said that the images

> that we make of the appearances [...] depending on what is characteristic of us may contain more or less superfluous ingredients." (Lorentz, 1915)

There is also evidence of a more indirect sort that Lorentz would have agreed with Hertz that it was common for theories to posit richer structures as underlying mechanisms than could be interpreted realistically. In his discussion of the role of the constructive hypotheses of his own theory of the electron in his Columbia lectures (Lorentz, 1916), Lorentz showed that the hypothesis of a deformable electron can account for the null results of ether drift experiments. Yet he also thought that a deformable electron undergoing arbitrary motion would not be stable, *even if* one were to assume the existence of internal non-electromagnetic "Poincaré forces." Lorentz argued that the forces postulated by Poincaré can only ensure that an ellipsoidal electron moving with constant velocity is stable, but that for an accelerating electron the internal stresses would work to further and further elongate the major axis of the ellipse pulling the electron apart (1916, pp. 215, 335–336). But Lorentz did not think that these foundational problems of his electron model were fatal to the theory as a whole. Rather he maintained:

> 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 the phenomena. (Lorentz, 1916, p. 215)

Either, Lorentz speculated, there might be additional forces that ensure the electron's stability, or perhaps "we are wholly on the wrong track when we apply to the parts of an electron our ordinary notion of force" (Ibid.). As far as the theory of the electron is concerned we can either assume that there are forces which ensure the stability of the electron or we can postulate that the deforming electron is stable "as a matter of fact which we have not to analyze any further." (Ibid.)

Finally, after having derived what we would today characterize as the full relativistic invariance of his theory of the electron as his main result Lorentz said:

> Having got thus far, we may proceed as is often done in theoretical physics. We may remove the scaffolding by means of which the system of equation has been built up, and, without troubling ourselves any more about the theory of electrons and the difficulties amidst which it has landed us, we may postulate the above equations as a concise and, as far as we know, accurate description of the phenomena. (Lorentz, 1916, pp. 222–223)

Thus, the hypothesis of a deformable electron is useful, even if it turns out that it cannot be construed realistically. Put in the terms of Hertz's picture theory, the practice of erecting an auxiliary "scaffolding" amounts to positing underlying relational structures of which only a proper substructure is interpreted realistically, while the excess structure is "thrown away." Thus Lorentz would have agreed that at least very commonly in theoretical physics theories are not maximally simple.

There are two aspects of this discussion to which I want to draw special attention. First, Lorentz's response to the foundational problems of the theory of the electron suggests that he would have disagreed with Hertz's claim that it is a necessary condition our images need to satisfy that they be consistent and "that we should from the outset take as inadmissible all images which contain a contradiction against the laws of our thought." (Hertz, 1910, p. 2) Lorentz showed that, if consistently applied, his theory was inconsistent with the existence of stable charged particles. Nevertheless he used his theory to predict the behavior of such particles. This suggests the view that while consistent hypotheses may very well be preferable, consistency is not a necessary condition for a theory to be explanatorily successful.

The second aspect is this. Even though Lorentz explicitly restricted his constructive hypotheses to playing only an "instrumental" role in the theory of the electron, he believed that these hypotheses could play an explanatory role and could be explanatorily superior to a more principled approach. Even without a detailed realistic model of the electron itself, Lorentz claimed that his theory could allow us to "make some progress in the understanding of the phenomena." In fact, despite acknowledging that by postulating "a general and fundamental principle" Einstein was able to arrive at the same results which he himself had "deduced with some difficulty and not altogether satisfactorily," Lorentz preferred his own approach, since it does better justice to the idea that the ether as carrier of the electromagnetic field has a certain degree of substantiality (Lorentz, 1916, pp. 229–230). Thus, Lorentz's believes that constructive theories are explanatory in ways principled theories are not, even when a constructive theory cannot be construed in a completely realist fashion.

There might appear to be a tension between Lorentz's view in his Columbia lectures and worries he expressed earlier about Abraham's criticism of his model. As we have seen, Abraham had shown that for a deformable electron there are two different and incompatible ways to calculate the energy of the electron. And he had suggested that this problem, as well as the stability problem for the deformable electron could only be solved by positing an additional non-electromagnetic internal energy. In 1904, Lorentz discussed Abraham's energy calculations in a footnote and said that

> should it turn out, as is well possible, that the assumption of a change in the internal energy is illegitimate, then this attempt to derive from the theory of the electron that the appearances are completely independent of any velocity of translation has to be seen as a failure. (1904, p. 100)

Unlike in his later discussion Lorentz does not here allow that the assumption of a deformable electron could be at least instrumentally useful in deriving the independence of the velocity of translation. One might hold that Lorentz's later instrumental construal of his hypothesis was a last desperate attempt to save the theory despite its foundational problems. But there is also a reading that renders the two passages consistent. The problem to which Abraham drew attention is that a deformable electron without internal non-electromagnetic stabilizing forces allows for two incompatible ways of calculating the electron's energy and, hence, its mass. But the mass is empirically measurable. Thus, one cannot instrumentally 'solve' Abraham's worry by postulating that the electron is stable in spite of the electromagnetic repulsive forces, for even with that postulate we are left with a theory that allows us to make different and incompatible empirical predictions without providing us with a principled way to single out one as correct (and with no principled way to delineate the 'scaffolding'). However, once internal stabilizing forces are introduced, in the manner proposed by Poincaré, the two mass calculations become consistent and the remaining stability problem can now be solved by fiat, since it is, in a sense, isolated from empirical test. To be sure, we can test empirically that accelerating electrons are stable, contrary to what follows from the existence of the repulsive forces, which are not perfectly balanced by the Poincaré forces for accelerating electrons. But if one imposes by fiat that electrons are stable, then the theory makes unambiguous predictions about how these particles behave.

There is one final point of agreement between Lorentz and Hertz to which I want to draw attention. We have already seen that Lorentz advocated what I have called a *theoretical pluralism* and that different theories of the phenomena in a single domain may each have their own advantages. This, too, is a view that Lorentz shared with Hertz, who believed that even the full set of criteria of theory evaluation does not in general determine a single theory as the best theory. For not only does the criterion of correctness allow for ties among theories, but that of purposefulness does so as well:

> Whether or not an image is useful cannot be decided without ambiguity, and there can be disagreement about this. One image may have certain advantages, another image may have others, and only by gradually testing many pictures can we finally succeed in obtaining those that are most useful.[18] (Hertz, 1910, p. 3)

Thus, different people, according to Hertz, can reasonably disagree in their assessment as to which theories are the most useful. Gradually over time, after testing many theories, our verdicts might converge. But even at the end there need not be a single theory that comes out ahead; rather there can be multiple theories that prove to be the most useful.

5. Realism as regulative ideal

It is time now to pull the different strands of our discussion together. How does the pragmatic view that has emerged fit with Lorentz's apparent commitment to realism expressed in the quote from Goethe's Faust? How are allowing for a plurality of different and incompatible theories and the instrumental use of hypotheses as "scaffolding" compatible with the aim of uncovering how deep under the surface things are woven together? I want to propose that for Lorentz realism functioned like something of a regulative ideal and a motivation for scientific research. The ultimate aim of science is the Faustian goal to understand "the power that holds the universe together." This aim motivates physicists in their research and functions as a constraint on the kinds of theories they construct. For Lorentz, the realist aim is reflected in his preference for mechanism-theories, which would have to be such that their hypotheses could (for the most part) be true of the world. Wildly speculative theories, for example, would be incompatible with that constraint. Molecular theories might satisfy this constraint, but Kelvin's vortex theory or the mechanics of concealed motions proposed by the "careful Hertz" (Lorentz, 1900c, p. 336) are in danger of being too speculative.

But the Faustian dream is far from being realized in our actual theories and may in fact never be fulfilled. Even though scientific theorizing may be motivated by a search to lay bare the underlying mechanisms behind the appearances, much less than the full truth about an underlying reality is actually obtainable. Thus, in evaluating and accepting theories in practice we are guided by more pragmatic (and even 'instrumental') considerations.

It might be useful to compare the view I am attributing to Lorentz with Bas van Fraassen's distinction between realism and empiricism. Van Fraassen characterizes scientific realism as follows: "Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true" (van Fraassen, 1980, p. 8). Van Fraassen's empiricism denies both conjuncts of this view and hold instead that "[s]cience aims to give us theories which are empirically

[18] The second sentence of this passage reads as follows in the original: "Das eine Bild kann nach der einen, das andere nach der anderen Richtung Vorteile bieten, und nur durch allmähliches Prüfen vieler Bilder werden im Laufe der Zeit schliesslich die zweckmässigsten gewonnen." The English translation of "die zweckmässigsten" as "the most appropriate" in Hertz (1956) is inadequate, since it does not distinguish between the plural used by Hertz and the singular "das zweckmässigste."

adequate; and acceptance of a theory involves as belief that it is empirically adequate.'' (Ibid, p. 12) Lorentz's view, it seems to me, fits neither of the two characterizations and in a sense appears to cut across the distinction drawn by van Fraassen. Lorentz thought that the *motivation* for scientific work is that of van Fraassen's realist: Science is motivated by the aim to discover a literally true story of the mechanisms behind the appearances.

Yet this realist motivation was paired with a highly pragmatic attitude toward scientific theorizing in practice. We should accept a theory, Lorentz thought, if it proves to be useful and fruitful, and in accepting a theory we are *not* accepting it as a literally true account of what the world is like. First, Lorentz thought that theories provide us with mere *Scheinbilder* and could at best represent relations among external objects faithfully. Second, accepting a theory as successful explanation of a range of phenomena is compatible, for Lorentz, with the belief that some other, rival theory might be equally or more successful in accounting for some phenomena within what ought to be the theory's domain. That is, accepting a theory does not entail that one should reject all of its rivals. And this implies that accepting a theory does not amount to believing in the literal truth of the theory. Instead, accepting a theory is compatible with construing some of its hypotheses instrumentally. Thus, Lorentz's view concerning theory *acceptance* is closer to that of van Fraassen's empiricist. But his view is not identical to that of the empiricist, since, as we have seen, Lorentz also believed that while our theories do not represent the world completely accurately, they get something right. This belief for Lorentz was based on a basic trust in a certain kind of kinship between our minds and nature. Given such a kinship, we can trust that the theories we accept based on the pragmatic considerations of fruitfulness or usefulness will make some contact with the world. Lorentz would not, then, agree entirely with van Fraassen's empiricist about what belief is involved in accepting a scientific theory. Rationally, in accepting a theory we are licensed to believe only in the theory's empirical adequacy. But Lorentz thought that it was an additional extra-rational presupposition of all scientific theorizing to believe that empirically successful constructive theories are not merely empirically adequate—a presupposition that falls far short, however, of a belief in the literal truth of our theories.

To the extent that Lorentz believed that realist commitments play a motivational role that cannot be underwritten by rational arguments, he once again appears to have anticipated a view of Einstein's. As Arthur Fine has persuasively argued, for Einstein realism is ''the main motive that lies behind creative scientific work and makes it worth doing'' (Fine, 1996, p. 110), even though Einstein, like Lorentz, did not embrace the cognitive aspects of scientific realism. Dismissing the idea of a merely positivist science, Einstein said:

> Behind the tireless efforts of the investigator their lurks a stronger, more mysterious drive: it is existence and reality that one wishes to comprehend.[19]

And on another occasion he likened the basic realist presupposition of scientific theorizing to a religious attitude:

> I have no better expression than the term 'religious' for this trust in the rational character of reality and its being accessible, at least to some extent, to human reason.[20]

[19]In an address at Columbia University in 1934, quoted in Fine (1996, p. 106).
[20]In a letter to his friend Solovine, quoted in Fine (1996, p. 110).

Thus, Einstein's view seems to resemble closely that of Lorentz: The characterization of the scientist's realist attitude as due to a "drive" or "religious trust" echoes Lorentz's notion of "an inner urge to trust." Einstein agreed with Lorentz that a realist attitude was an in some sense necessary component of scientific theorizing, despite the fact that it lacked a rational basis. Moreover, like Lorentz, Einstein appeared to have been less than sanguine about the prospects of science ever gaining a full understanding of reality. Yet Lorentz's realism appears to have run more deeply than that of Fine's Einstein. Whereas for Einstein, according to Fine, realism had the status of "childhood fantasies," from which Einstein would instantly "retreat" "just as soon as they are let out into the open," (Fine, 1996, p. 111) Lorentz seemed to have embraced the belief in a basic kinship between our minds and the world without subjecting it to philosophical doubt. Einstein, according to Fine's account, seems to have taken realism to be unjustified in the sense that despite its perhaps psychological unavoidability, he thought that there are good philosophical arguments against the position. Lorentz, by contrast, either did not believe that there were such arguments or perhaps simply never saw any reason to subject his basic trust to any doubt.

I want to conclude by pointing out that the idea that our actual scientific theories fall short of realizing our realist aims fits in fact rather nicely with the passage from Goethe's Faust that Lorentz quoted in 1900c. This passage is taken from the first scene of the play set in Faust's study, as Faust, after lamenting his inability to uncover the powers of nature, is contemplating the sign of the macrocosm. The sign appears to reveal these powers to him:

> By these pure brush strokes can I see,
> At my souls feet, great Nature unconcealed

But then Faust realizes that what he grasps is not in fact the universal order of nature and despairs:

> Oh what a spectacle! But a spectacle, and no more!
> How seize you and hold you, infinite nature?

Just like the sign of the macrocosm, our theories seem to carry the promise to reveal to us the inner workings of nature, but like the sign they ultimately fail to fulfill that promise. For scientific theories, on Lorentz's view, are partly instrumental constructions which provide us with merely apparent images of objects. Unlike for Faust, however, realizing the limits to what can be obtained in practice were no ground for despair for Lorentz. Lorentz rather advocated a pragmatically inspired modest optimism:

> The physicist, and that applies to all of us, has to restrict himself to read in the book of nature in his own manner. Without allowing the realization that the deep sense will remain hidden to depress him, he feels encouraged by the conviction that within the bounds of what is achievable wide and unexpected vistas will open up as he progresses. (Lorentz, 1907, p. 16)

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