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A Glimpse into the Poincaré Archives

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Abstract. This paper discusses the only presently known correspondence between Poincaré and the great Dutch physicist H.A. Lorentz. These six letters concern matters on the very cutting edge of theoretical physics during the first decade of the 20th century, and are meant as a sampling of the riches to be found in the Poincaré archives.

Résumé. Cet article traite de la seule correspondance actuellement connue entre Poincaré et le grand physicien hollandais H.A. Lorentz. Ces six lettres concernent les questions les plus aigües de la physique théorique pendant la première décennie du XXe siècle, et sont proposées ici comme un échantillon des richesses restant à découvrir dans les archives de Poincaré.

In July of 1976 I had the good fortune to discover a large amount of Henri Poincaré's archival material, which was in the possession of his great-grandson, Monsieur François Poincaré. I returned to Paris the following year to catalog this material and as well to enlarge the collection through exploring archives elsewhere in Paris and Europe generally. Presently the Poincaré archives are at Université Nancy 2, under the able direction of Professor Gerhard Heinzmann.

This paper discusses the only presently known correspondence between Poincaré and the great Dutch physicist H.A. Lorentz. These six letters concern matters on the very cutting edge of theoretical physics during the first decade of the 20th century, and are meant as a sampling of the riches to be found in the Poincaré archives.

1. H.A. Lorentz's Letter of 20 January 1901 to Henri Poincaré

By 1900 the electron on which H.A. Lorentz's electromagnetic theory had been predicated was discovered. So successful had Lorentz's become that Henri Poincaré's assessments of it had gone from the "least defective" (in 1895) to the "most satisfactory" (1900) [Poincaré 1895, 409; 1900a, 175]. Poincaré, however, was disturbed by two blemishes on Lorentz's theory that Lorentz had been willing to overlook: (1) Lorentz's ad hoc 1892 contraction hypothesis for explaining the null result of the 1887 Michelson and Morley experiment involving an effect of second order in v/c (where v is the earth's velocity relative to the ether); Poincaré had scathingly criticised the contraction hypothesis — "hypotheses are what we lack least."[1] 2) But even worse, in Poincaré's opinion, Lorentz's theory violated Newton's principle of action and reaction. Lorentz had built

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[1] Lorentz invented the contraction hypothesis to explain a single experiment, and he derived its mathematical form Newton's law for the addition of velocities, which Lorentz's theorem of corresponding states was to supposed to have avoided. For further discussion see [Miller 1997].

this violation into his theory since the ether acted on bodies, but not vice versa. In his publication of 1895 Lorentz had dodged this issue by asserting that Newton's principle of action and reaction need not be universally valid. Newton's principle of action and reaction, however, was on the highest level of Poincaré's hierarchical view of a scientific theory because its generality precluded its experimental disconfirmation [Poincaré 1902, 172][2].

The 1900 *Lorentz-Festschrift*, celebrating the twenty-fifth anniversary of Lorentz's doctorate from Leiden, included Poincaré's paper, "The Theory of Lorentz and the Principle of Reaction," in which he demonstrated the lengths to which he was prepared to go in order to save a principle [Poincaré 1900b] In his work, using the Maxwell-Lorentz equations, Poincaré reexpressed the Lorentz force on a charged body in a volume V, owing to charges external to V as

$$\mathbf{F} = \nabla \cdot \mathbf{T} - \frac{d}{dt}(\mathbf{E} \times \mathbf{B})/4\pi c \qquad (1)$$

where **T** is Maxwell's stress tensor. He calculated the net force $\mathbf{F}_N$ on the charged body by integrating over the volume V, which he extended out to infinite in order to include the sources of the external electromagnetic fields. Poincaré's result is

$$\mathbf{F}_N = -\frac{d}{dt}\,[\int(\mathbf{E} \times \mathbf{B})/4\pi c]dV, \qquad (2)$$

since the contribution from the Maxwell stress tensor vanishes. Consequently, in Lorentz's theory a charged body cannot attain equilibrium. Poincaré traced this result to the absence of isolated systems in Lorentz's theory in the sense that this term was used in mechanics. Setting

$$\mathbf{F}_N = \frac{d}{dt}(m_o v) \qquad (3)$$

where m_o is the charged body's inertial mass, Poincaré rewrote Eq.(2) as

$$\frac{d}{dt}\{\, m_o v + [\int(\mathbf{E} \times \mathbf{B})/4\pi c]dV\} = \mathbf{0}. \qquad (4)$$

The second term in Eq.(4) violates the principle of action and reaction as this principle is understood in Newton's mechanics. Poincaré next took the short but bold step that his philosophical view

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[2] Thus, Poincaré referred to Newton's principle of action and reaction as a "convention". See [Miller 1986a; 1996].

demanded: in order to rescue Newton's principle of action and reaction he compared the electromagnetic field to a "fictitious fluid" with a mass and "momentum"

$$\mathbf{G} = [\int(\mathbf{E} \times \mathbf{B})/4\pi c]dV. \quad (5)$$

Thus, Poincaré's electromagnetic momentum permitted him to simulate isolated closed systems in Lorentz's theory. In these systems, according to Poincaré, the net force $\mathbf{F}_N$ is cancelled by compensatory mechanisms in the ether arising from the electromagnetic momentum's temporal variation. But further hypotheses were necessary. Supposed continued Poincaré, that an emitter of unidirectional radiation and an absorber were in relative inertial motion. Using Lorentz's local-time coordinate Poincaré demonstrated the insufficiency of the electromagnetic momentum for saving the principle of action and reaction separately for emitter and absorber. For this purpose he postulated an "apparent complementary force." In order to emphasise the necessity for this desperate step to save a principle, Poincaré reiterated that in conservative mechanical systems the principle of action and reaction could be considered as a consequence of the principles of energy conservation and of relative motion[3]. According to Poincaré's view of mechanics, what he called the "principle of relative motion" could never be overthrown because it embodied the covariance of Newton's laws in inertial reference systems *and* Newton's second law; furthermore, the principle of relative motion asserted the meaningfulness of only the relative motion between ponderable bodies, which is information drawn from the world of our perceptions; and so, wrote Poincaré, "the contrary hypothesis is singularly repugnant to the mind." [Poincaré 1902, 111]

Leiden, le 20 janvier 1901

Monsieur et très honoré collègue,

Permettez moi de vous remercier bien sincèrement de la part que vous avez bien voulu prendre au recueil de travaux que m'a été offert à l'occasion du 25eme anniversaire de mon doctorat. J'ai été profondément touché de ce que tant d'illustres savants aient choisi ce jour pour me témoigner leur sympathie et l'intérêt qu'ils portent à

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[3] For further discussion of this point, including whether at this time Poincaré understood the basic concepts of what would become known as the special theory of relativity see [Miller 1997, esp. pp. 94-95].

mes études, malgré l'imperfection des résultats auxquelles elles m'ont conduit. Cette imperfection est telle que je n'ose presque pas regarder comme un signe d'approbation le livre qu'on m'a dédié; j'y verrai plutôt un encouragement qui m'est précieux.

Comme votre jugement a, à mes yeux une très grande importance, vous m'avez particulièrement obligé par le choix de votre sujet et par les paroles qui précèdent votre article. J'ai suivi vos raisonnements avec toute l'attention qu'ils demandent et je sens toute la force de vos remarques. Je dois vous avouer qu'il m'est impossible de modifier la théorie de telle façon que la difficulté que vous signalez disparaisse. Il me semble même guère probable qu'on puisse y réussir; je crois plutôt —et c'est aussi le résultat auquel tendent vos remarques— que la violation du principe de réaction est nécessaire dans toutes les théories que peuvent expliquer l'expérience de Fizeau. Mais faut-il en vérité que nous nous en inquiétions. Il y a un certain rapport entre vos considérations et une question qui a été soulevée, comme vous le savez, par Helmholtz dans un de ses derniers mémoires. En effet, vos formules démontrent que l'éther contenu dans une surface fermée ne sera pas en équilibre sous l'influence des pressions de Maxwell exercées à cette surface, dès que le vecteur de Poynting est une fonction du temps. De ceci, Helmholtz tire la conclusion que l'éther sera mis en mouvement dans un tel cas, et il cherche à établir les équations qui déterminent ce mouvement.

J'ai préféré une autre manière de voir. Ayant toujours en vue les phénomènes d'aberration, j'ai admis que l'éther est absolument immobile — je veux dire que ses éléments de volume ne se déplacent pas, bien qu'ils puissent être le siège de certains mouvements internes. Or, si un corps ne se déplace jamais, il n'y a aucune raison pour laquelle on parlerait de forces exercées sur ce corps. C'est ainsi que j'ai été amené à ne plus parler de forces qui agissent sur l'éther.

Je dis que l'éther agit sur les électrons, mais je ne dis pas qu'il éprouve de leur côté une réaction; je nie donc le principe de la réaction dans ces actions élémentaires. Dans cet ordre d'idée je ne puis pas non plus parler d'une force exercée par une partie de l'éther sur l'autre; les pressions de Maxwell n'ont plus d'existence réelle et ne sont que des fictions mathématiques qui servent à calculer d'une manière simple la force qui agit sur un corps pondérable. Évidemment, je n'ai plus à me soucier de ce que les pressions qui agissent à la surface d'une portion le partie de l'éther ne seraient pas en équilibre.

Quant au principe de la réaction, il ne me semble pas qu'il doive être regardé comme un principe fondamental de la physique. Il est vrai que dans tous les cas où un corps acquiert une certaine quantité de mouvement *a*, notre esprit ne sera pas satisfait tant que nous ne puissions indiquer un changement simultané dans quelque autre

> corps, et que dans tous les phénomènes où l'éther intervient, ce changement consiste dans l'acquisition d'une quantité de mouvement $-a$. Mais je crois qu'on pourrait être également satisfait si ce changement simultané ne fût pas lui-même la production d'un mouvement. Vous avez déduit la belle formule
>
> $$\Sigma \mathrm{Mv}_x + \int d\tau(\gamma g - \beta h) = \mathrm{Const.}$$
>
> Il me semble qu'on pourrait se borner à considérer
>
> $$\int d\tau(\gamma j - \beta h), \int d\tau(\alpha h - \gamma j), \int d\tau(\beta j - \alpha g)$$
>
> comme des quantités dépendantes de l'état de l'éther qui sont pour ainsi dire "équivalentes" à une quantité de mouvement. Votre théorème nous donne pour toute modification de la quantité de mouvement de la matière pondérable une modification simultanée de cette quantité équivalente; je crois qu'on pourrait bien se contenter de cela.
>
> Je ne veux pas prétendre que cette manière de voir soit aussi simple qu'on pourrait le désirer; aussi n'aurais-je pas été conduit à cette théorie si les phénomènes de l'aberration ne m'y eussent pas forcé. Du reste, il va sans dire que la théorie ne doit être considérée que comme provisoire. Ce que je viens d'appeler "équivalence" pourra bien un jour nous apparaître comme une "identité"; cela pourrait arriver si nous parvenons à considérer la matière pondérable comme une modification de l'éther lui-même.
>
> Il est presque inutile de dire qu'on pourrait aussi se tirer d'embarras en attribuant à l'éther une masse infiniment (ou très) grande. Alors les électrons pourraient réagir sur l'éther sans que ce milieu se mit en mouvement. Mais cette issue me semble assez artificielle.
>
> Je désirerais bien vous faire encore quelques remarques au sujet de la compensation des termes en v^2, mais cette lettre serait trop longue. J'espère donc que vous me permettrez de revenir sur cette question une autre fois. Il y a là encore bien des difficultés; vous pourriez peut être parvenir à les surmonter.
>
> Veuillez agréer, Monsieur et très honoré collègue, l'assurance de ma sincère considération. Votre bien dévoué
>
> H. A. Lorentz

Lorentz's response to Poincaré's *Festschrift* paper is in his letter of 20 January 1901. In this letter Lorentz expressed his admiration for Poincaré's paper and then launched into an eight page rebuttal. Lorentz's opinion of Poincaré's valiant attempt at saving the principle of action and reaction was, "But must we, in truth, worry ourselves about it?" Lorentz added in his typically forthright way: "I must claim to you that it is impossible for me to modify my theory in such a way that the difficulty that you cited disappears." Lorentz goes on to emphasise several times that his ether acts on bodies but there is no reaction back on the ether. He explains that the "phenomenon of aberration," that is, first-order effects, had "forced him" to assume a motionless ether; in fact, in Lorentz's seminal paper on electromagnetic theory, [Lorentz 1892a][4] second-order effects are nowhere mentioned.[5] Lorentz continues in this letter: "I deny therefore the principle of reaction in these elementary actions." In mechanics, Lorentz continues, action and reaction are instantaneous because disturbances are not mediated by an ether; however, in electromagnetic theory the reaction of an emitter of radiation is not compensated simultaneously by the action on the absorber. Poincaré had avoided this problem by attempting to satisfy the principle of reaction separately by emitter and absorber.

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[4] For analysis of this paper see [Miller 1997, chap. 1].

[5] Whereas the Michelson-Morley experiment always had been of great concern to Lorentz, in 1892 it did not determine the formulation of his electromagnetic theory. This might seem surprising in the light of Lorentz's strongly empiricist message in 1886; namely, that in a question "as important" as the choice between a pure Fresnel theory, or a hybrid one containing elements of the theories of Fresnel and Stokes, one whould not be guided by "considerations of the degree of probability or of simplicity of one or the other hypotheses, but to address oneself to experiment" [Lorentz 1886].

(According George G. Stokes the ether at the earth's surface is dragged totally, and the dragging decreases with increasing distance from the earth's surface. In the 1886 paper Lorentz proved the inconsistency of Stokes's assuming both a velocity potential and that the relative velocity between the earth and the ether should vanish over the earth's surface. According to Augustin Fresnel's theory of moving body drags the excess of ether in its interior, which he assumed to be $1-1/n^2$, where n is the body's refractive index.)

To Lorentz at this time, Michelson's interferometer experiment of 1881 was of importance since it was the only reliable means to obtain data accurate to second order in *v/c*. Having shown in 1886 that Michelson's 1881 experiment was inconclusive, Lorentz urged its repetition. This was carried out in 1887, and the results apparently excluded a pure Fresnel theory. Yet, as Lorentz explained in 1892, hybrid theories, "being more complicated are less worthy of consideration," and again in 1897 he wrote that the "Fresnel theory is without doubt simpler." Consequently, the formulation of Lorentz's electromagnetic theory ran counter to his empiricist guideline of 1886. As is so often the case with highly creative scientists, because in the end simplicity is a guide. See [Lorentz 1892b; 1897]. This episode is explored in [Miller 1997, esp. chap. 1].

Consistent with his desire to maintain an absolutely immobile ether, Lorentz protested Poincaré's naming the quantity in Eq.(5), which Lorentz compared to Poynting's vector, to be an electromagnetic momentum. To Lorentz the term momentum, of course, connoted motion. Lorentz was willing to concede only that Poincaré's electromagnetic momentum was formally "'equivalent' to a momentum". Thus, in this letter, Lorentz informed the critical Poincaré of his own sensitivity toward adding further hypotheses to an already overburdened theory, especially hypotheses invented solely to save a principle whose violation permitted the theory's formulation in the first place. Lorentz concluded this letter by emphasising that at that this time, his chief concern was folding the contraction hypothesis into his electromagnetic theory.

Possibly as a result of this letter, one of Poincaré's reasons in papers of 1904 and 1908 for rejecting the principle of action and reaction was Lorentz's argument that the compensation between action and reaction could not be simultaneous.

To summarise: Poincaré's attempt in 1900 to attempt to save the principle of action and reaction is a fine example of the application of his immense powers of mathematics, and his philosophic view, to criticising, clarifying, and extending previously proposed physical theories. Lorentz's 1901 letter to Poincaré is typical of Lorentz's style at this point in his research on electromagnetic theory — that is, pushing ahead and leaving problems of foundations and philosophy to others. It turned out that the road to a foundationally sound electrodynamics could be achieved only through an analysis of fundamental problems within a philosophical framework that was, to some extent, liberated from empirical data. Only in this way, Albert Einstein could formulate a basis for exploring what it meant for action and reaction not to be simultaneous.[6]

2. Three Letters of Poincaré to Lorentz Written Sometime During Late 1904 and Early 1905

Poincaré wrote the first of this set of three letters sometime after he returned from the September 1904 Congress of Arts and Science at St. Louis, Missouri [Poincaré 1904]. At St. Louis, Poincaré had discussed excitedly and optimistically the main results of Lorentz's 1904 theory of the electron [Lorentz 1904][7]. Using a

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[6] For comparison bewteen Einstein's and Poincaré's views on simultaneity see [Miller 1997; 1996].

[7] For an in-depth analysis of Lorentz's paper see [Miller 1997, esp. chap. 1].

term well known from fundamental studies in geometry, he had renamed Lorentz's new theorem of corresponding states the "principle of relativity," according to which the "laws of physical phenomena must be the same for a stationary observer as for an observer carried along in a uniform motion of translation; so that we have not and cannot have any means of discerning whether or not we are carried along in such a motion."

Mon cher collègue,

J'ai énormément regretté les circonstances qui m'ont empêché d'abord d'entendre votre conférence et ensuite de causer avec vous pendant votre séjour à Paris.

Depuis quelque temps j'ai étudié plus en détail votre mémoire electromagnetic phenomena in a system moving with any velocity smaller than that of the light, mémoire dont / l'importance est extrême et dont j'avais déjà cité les principaux résultats dans ma conférence de St. Louis.

Je suis d'accord avec vous sur tous les points essentiels; cependant il y a quelques divergences de détail.

Ainsi page 813, au lieu de poser :

$$\frac{1}{k\ell^3}\rho = \rho' ; k^2 u_x = u'_x , k^2 u_y = u'_y$$

il me semble qu'on doit poser :

$$\frac{1}{k\ell^3}\rho(1+\varepsilon v_x) = \rho' \qquad \frac{1}{k\ell^3}\rho(v_x+\varepsilon) = \rho' u'_x$$

où $\varepsilon = -\frac{w}{c}$ ou $\varepsilon = -w$ si nous choisissons les unités de telle façon que c=1

Cette modification me semble s'imposer si l'on veut que la charge apparente de l'électron se conserve.

Les formules (10) page 813 se trouvent alors modifiées et je trouve pour le dernier terme au lieu de

$$\ell^2 \frac{w}{c^2}(u'_y d'_y + u'_z d'_z), -\frac{\ell^2}{k}\frac{w}{c^2}u'_x d'_y, -\frac{\ell^2}{k}\frac{w}{c^2}u'_x d'_z$$

je trouve

$$\ell^2 \frac{w}{c^2}(u'_x d'_x + u'_y d'_y + u'_z d'_z), 0, 0$$

C'est la force de Lienard, que vous trouvez aussi, mais avec des différences. / Et alors la question se pose de savoir si cette force est ou non compensée.

Ceci montre qu'entre les forces réelles X,Y,Z et les forces apparentes X',Y',Z' il y a les relations

$$X' + A(X + \varepsilon\Sigma X \mathrm{v}_x), Y' = BY, Z' = BZ$$

A et B étant des coeff. et $A\varepsilon\Sigma X \mathrm{v}_X$ représentant la force de Lienard. Si toutes les forces sont d'origine électrique les conditions d'équilibre (ou du principe de d'Alembert modifié) donnent

$$X = Y = Z = 0$$

d'où

$$X' = Y' = Z' = 0$$

Si toutes les forces ne sont pas d'origine électrique, il y aura encore compensation pourvu qu'elles se comportent toutes comme si elles étaient d'origine électrique.

Mais il y a autre chose.

Vous supposez $\ell = 1$.

Langevin suppose $k \ell^3 = 1$.

J'ai essayé $k \ell = 1$ pour conserver l'unité de temps, mais cela m'a conduit à des / conséquences inadmissibles.

D'un autre côté j'arrive à des contradictions (entre les formules de l'action et de l'énergie) avec toutes les hypothèses autres que celles de Langevin.

Le raisonnement par lequel vous établissez que $\ell = 1$ ne me paraît pas concluant, ou plutôt il ne l'est plus et laisse ℓ indéterminé quand je fais le calcul en modifiant comme je vous l'ai dit les formules de la page 813.

Que pensez-vous de cela, voulez-vous que je vous communique plus de détails ou ceux que je vous ai donnés vous suffisent-ils.

Excusez moi en tout cas d'abuser de votre temps.

Votre bien dévoué collègue,

Poincaré

Poincaré began the first of this set of three letters in a familiar way to his past critiques of Lorentz's works, that is, by noting certain errors in Lorentz's 1904 paper. Lorentz's use of three different reference systems had led him to deduce incorrect equations for the electron's velocity, charge density, and Lorentz force as these quantities are related between the reference systems S_r *and* Σ'. The coordinate transformation linking the reference systems S_r *and* Σ'

that Poincaré dubbed the "Lorentz transformations" [Poincaré 1905b; 1906][8] is

$$x' = klx_r \tag{6}$$

$$y' = ly_r \tag{7}$$

$$z' = lz_r \tag{8}$$

$$t' = lt_r / k - klvx_r / c^2 \tag{9}$$

$$(k = 1/\sqrt{1-\beta^2}),$$

where $\beta = v/c$, and l is a function of the electron's momentum. The coordinates (x', y', z', t') refer to an auxiliary or mathematical reference system Σ' the Maxwell-Lorentz equations (Lorentz's electromagnetic field equations were referred to) have the same form as they do relative to an ether-fixed reference system; the coordinates (x_r, y_r, z_r, t_r) refer to an inertial reference system S_r; and $x_r = x - vt$, $y_r = y$, $z_r = z$, $t_r = t$, where (x, y, z, t) refer to the ether-fixed system Σ; l admits the possibility of any sort of deformation, where $l = 1$ yields Lorentz's contraction and also is the only value of l that is consistent with Lorentz's theorem of corresponding states that applies to any order in v/c. Lorentz's goal was to transform the fundamental equations of his electromagnetic theory from the inertial reference system S_r to the fictitious reference system Σ' in which he could perform any necessary calculations, and then transform back to S_r. For example, it was particularly useful to take Σ' to be the electron's instantaneous rest system in order to reduce problems in electrodynamics and optics of moving bodies to problems concerning bodies at rest.[9]

In fact, Lorentz had been aware of the errors pointed out by Poincaré, and had assured their not affecting his final results by having taken Σ' as the electron's instantaneous rest system, thereby causing offending terms to vanish. Using only S and Σ', Poincaré obtained the correct transformations for the general case where Σ' was not the electron's instantaneous rest system.

Poincaré continued by pointing out where the fundamental difficulty with Lorentz's theory lay, although he did not know yet how to resolve it: two of the quantities characterising the moving electron (its momentum and longitudinal mass) differed when

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[8] For a detailed analysis of these papers see [Miller 1973].

[9] For further discussion of the importance of these three coordinate systems in physics research in the first decade of the twentieth century see [Miller 1997].

calculated from the Lagrangian and from the energy.[10] Since Lorentz had calculated the electron's mass directly from its momentum, he had missed this problem. For Poincaré, investigating the foundations of Lorentz's theory meant reformulating it within the framework of that most elegant and powerful formulation of physical theory — the mechanics of Hamilton and Lagrange. Consistent with his style of treating every problem in the most general possible manner, Poincaré's reformulation was based on a generalised version of the dynamics of the electron in which he could study every possible model of the electron, that is, all connections between k and l. It turned out as Poincaré wrote in this letter, that only the 1904 theory of the electron by Poincaré's former student, Paul Langevin, offered consistency regarding the electron's momentum.[11]

In the conclusion of this letter, Poincaré expressed his dissatisfaction with Lorentz's method of proof for $l = 1$ because it employed formulae for forces and accelerations valid only in the electron's instantaneous rest system, and so the proof was not general enough.

> Mon cher collègue,
>
> Merci de votre aimable lettre. Depuis que je vous ai écrit mes idées se sont modifiées sur quelques points. Je trouve comme vous $\ell = 1$ par une autre voie.
>
> Soit $-\varepsilon$ la vitesse de translation celle de la lumière étant prise pour unité.
>
> $$k = (1 - \varepsilon^2)^{-\frac{1}{2}}$$
>
> On a la transformation
>
> $$x' = k\ell(x + \varepsilon t), \qquad t' = k\ell(t + \varepsilon x)$$

---

[10] The inconsistency of Lorentz's theory concerning the electron's longitudinal mas was also brought to Lorentz's attention by Max Abraham in an unpublished letter of Abraham to Lorentz written from Wiesbaden on 26 January 1905 (on deposit at the Algemeen Rijkarchief, The Hague). Abraham developed the calculations in this letter in his [1905] book, *Theorie der Elektrizität: Elektromagnetische Theorie der Strahlung*. For details see [Miller 1997].

[11] According to Langevin the connection between l and k is $l^3k = 1$. See [Langevin 1905]. Simultaneously and independently A.H. Bucherer in Bonn offered the same theory [Bucherer 1904].

$$y' = \ell y, \quad z' = \ell z$$

Ces transformations forment un groupe. Soient deux transformations correspondant à

k, ℓ, ε

et

k', ℓ', ε'

leur résultante correspondra à

$k'', \ell'', \varepsilon''$

où

$$k'' = (1-\varepsilon'')^{-\frac{1}{2}}, \ell'' = \ell\ell', \varepsilon'' = \frac{\varepsilon + \varepsilon\varepsilon'}{1+\varepsilon\varepsilon'}$$

Si nous voulons maintenant prendre :

$$\ell = (1-\varepsilon^2)^m, \ell' = (1-\varepsilon'^2)^m$$

nous n'aurons

$$\ell'' = (1-\varepsilon''^2)^m$$

que pour m=0.

D'un autre côté je ne trouve d'accord entre le calcul des masses par le moyen des quantités de mouvement électromagnétique et par le moyen de la moindre action, et par le moyen de l'énergie que dans l'hypothèse de Langevin.

J'espère tirer bientôt au clair cette contradiction, je vous tiendrai au courant de mes efforts.

Votre bien dévoué collègue,

Poincaré

But in the next letter, Poincaré elegantly resolved this insufficiency. By eliminating the inertial reference system S_r, Poincaré was able to rewrite Eqs.(6)-(9) in a form possessing a high degree of mathematical symmetry:

$$x' = kl(x - vt) \tag{10}$$

$$y' = ly \tag{11}$$

$$z' = lz \tag{12}$$

$$t' = kl(t - vx/c^2) \tag{13}$$

where in Poincaré's letter $\varepsilon = -v$. Considering two successive Lorentz transformations along the same direction, it was easy for him to prove that the Lorentz transformations form a group, and l must be equal to one. As a bonus, Poincaré also obtained the new addition law for velocities that is independent of l.[12]

> Mon cher collègue,
>
> J'ai continué les recherches dont je vous avais parlé. Mes résultats confirment pleinement les vôtres en ce sens que la compensation parfaite (qui empêche la détermination expérimentale du mouvement absolu) ne peut se faire complètement que dans l'hypothèse $\ell = 1$. Seulement pour que cette hypothèse soit admissible, il faut admettre que chaque électron est soumis à des forces complémentaires / dont le travail est proportionnel aux variations de son volume.
>
> Ou si vous aimez mieux, que chaque électron se comporte comme s'il était une capacité creuse soumise à une pression interne constante (d'ailleurs négative) et indépendante du volume.
>
> Dans ces conditions, la compensation est complète.
>
> Je suis heureux de me trouver en parfait accord avec vous et d'être arrivé ainsi à l'intelligence parfaite de vos beaux travaux.
>
> Votre bien dévoué collègue,
>
> Poincaré

In the third letter of this set, Poincaré's key result is that only Lorentz's 1904 theory of the electron offered "perfect compensation (which prevents the experimental determination of absolute motion)." Poincaré linked this result to solving the problem concerning the longitudinal mass of Lorentz's electron. Poincaré's solution was to add a term to the electron's Lagrangian that preserved the Lorentz invariance of the principle of least action. Thus, he was led to interpret the additional term as the energy due to the internal pressure that served to prevent the deformable electron from exploding in its rest system.[13] Now Lorentz's theory was freed from all removable blemishes. We can well believe that Poincaré was, as he put it, in "parfait accord" with the "beaux travaux" of Lorentz. Lorentz's 1904 theory of the electron was really the

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[12] Since the ether system S never moves, Poincaré could give only a mathematical interpretation to the Lorentz transformation's inversion symmetry; interchanging primes and changing ε with $-\varepsilon$ is the mathematical operation of rotation by 180° about the common y-axes of S and Σ' [Poincaré 1905b/1906].

[13] For details concerning the stability problem see [Miller 1997].

culmination of over a decade of painstaking work and interaction by Lorentz and Poincaré.

Of the existing electron theories, only Lorentz's was really adequate. It could explain optical phenomena accurate to second order in v/c. It explained effects such as the dependence of mass on velocity, the contraction of moving objects, and the isotropy of the velocity of light in inertial reference systems, all to have been *caused* by the interaction between electrons constituting matter and the ether. Lorentz's theory of the electron appeared to many physicists to be the most likely candidate for the cornerstone of unified field-theoretical description of nature. As he showed in his 1905-1906 publication, Poincaré had cast the theory into the Hamilton-Lagrange formalism, replete with group theory and four-dimensional spaces, and then extended it to a Lorentz-covariant theory of gravity.

3. H.A. Lorentz to Henri Poincaré of 8 March 1906

Ever since 1901, the eminent experimentalist Walter Kaufmann, had been performing experiments to explore the dependence of the electron's mass on its velocity relative to the laboratory system.[14] As had been predicted by calculations performed in 1887 by the electron's discoverer J.J. Thomson, and subsequently others, Kaufmann found that the electron's mass increased seemingly without limit as its velocity approached that of light. The importance of Kaufmann's discovery cannot be overstated because it lay at the core of an exciting new research programme which was referred to as the "electromagnetic world-picture": to reduce all of physics to H.A. Lorentz's electromagnetic theory.[15] For example, Newton's second law of motion, F = ma, would be deduced from Lorentz's force equation

$$\mathbf{F} = \rho\mathbf{E} + \rho\mathbf{v} \times \mathbf{B}/c \qquad (14)$$

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[14] For a detailed discussion of Kaufmann's experiments on high-velocity electrons during 1901-1906, see [Miller 1997, chap. 1, 7 & 12]. In 1897 Kaufmann, then at the University of Berlin, performed experiments on the nature of cathode rays that were more precise than those of J.J. Thomson. Being a follower, however, of Ernst Mach's positivistic philosophy, Kaufmann could not bring himself to interpret his data as the effects of submicroscopic particles. To his horror, Kaufmann missed out on the electron's discovery. Starting in 1901 he was determined to make up for this loss by exploring the nature of high-velocity electrons.

[15] See [Miller 1997, chap. 1] for details.

where, in this case, **F** is the force density exerted on an electron of charge density ρ and self generated electric and magnetic fields **E** and **B**, respectively, and consistent with programmatic intent of the electromagnetic world-picture, the electron's inertial mass is set equal to zero.

In order to fit Kaufmann's astounding new data, assumptions have to added onto Lorentz's electromagnetic theory. These assumptions concern the electron's structure, that is, whether it is a rigid sphere or an elastic one, and what sorts of accelerated motion it can undergo and yet be able to manipulate Eq.(14) to yield an expression identifiable as Newton's second law. In this case the mass multiplying into the electron's acceleration would have a velocity dependence characteristic of the assumptions made.

The first theorist to take up the challenge offered by Kaufmann's data was his young colleague at Göttingen, Max Abraham [1902a; b; 1903][16]. In order to avoid the necessity of assuming nonelectromagnetic forces to hold the electron together, Abraham assumed that the electron is a rigid sphere. Indigenous to all theories of the electron in which its mass was assumed to be deducible from Lorentz's force equation, the electron exhibited two masses depending on whether it was reacting to a force along or transverse to its direction of motion. Abraham designated these two quantities as the electron's longitudinal mass m_L and transverse mass m_T. Together with certain assumptions on the electron's acceleration, in 1902 Abraham produced a prediction for the electron's transverse mass that seemed to agree with Kaufmann's most recent data which was supposed to measure the electrons transverse mass. I emphasise the phrase Kaufmann's most recent data, because Kaufmann was ever fine tuning and correcting his data. Nevertheless, the apparent precision of his apparatus, coupled with his reputation for accuracy, led to acceptance of his data as the last word in high energy experimental particle physics, of which he was the first practitioner.

In March 1904 Lorentz's electron theory appeared [Lorentz 1904][17]. Lorentz assumed the electron to be a balloon smeared with charge. While at rest it is a sphere, but when moving it undergoes a Lorentz contraction. With similar assumptions on the electron's acceleration, Lorentz produced a prediction for the electron's mass. Lorentz claimed that his prediction for the electron's transverse mass also agreed with Kaufmann's published data. What follows is a

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[16] See [Miller 1997, chap. 1] for details.

[17] For an in-depth analysis see [Miller 1997, chap. 1].

fascinating example of the interplay between data and theory which centres about a letter written by Lorentz to Poincaré on 8 March 1906.

The drama begins with a letter from Kaufmann to Lorentz of 10 July 1904.[18] In it Kaufmann informs Lorentz that Lorentz's transverse mass fits Kaufmann's most data better than even Lorentz imagined. In order to decide the issue between the electron theories of Abraham and Lorentz, Kaufmann promised to increase his accuracy in the next series of experiments. He was as good as his word, but the results were not to please Lorentz.

In a short paper of 30 November 1905, and then in a longer one of early 1906, Kaufmann reported that his new measurements agreed best with Abraham's theory — a close second was Bucherer's[19] — but as put it, the "measurement results are not compatible with Lorentz-Einstein fundamental assumption" [Kaufmann 1905; 1906]. As Kaufmann wrote in 1905, "a recent publication by Mr. A. Einstein on the theory of electrodynamics... leads to results which are formally identical with those of Lorentz's theory" — that is, the "formal equivalence" of Einstein's 1905 relativity transformations for space and time and his prediction for the electron's transverse mass with these quantities in Lorentz's theory led to the names "Lorentz-Einstein theory" and "Lorentz-Einstein principle of relativity."[20]

Leiden, le 8 mars 1906.

Monsieur et très honoré collègue,

C'est déjà trop longtemps que j'ai négligé de vous remercier de l'important mémoire sur la dynamique de l'électron que vous avez bien voulu m'envoyer. Inutile de vous dire que je l'ai étudié avec le plus grand intérêt et que j'ai été très heureux de voir / mes

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[18] Letter on deposit at the Algemeen Rijksarchief, The Hague.

[19] According to the electron models proposed simultaneously and independently by Paul Langevin and Adolf Bucherer, the moving electron undergoes a deformation of a sort in which its volume remains unchanged. See [Langevin 1905] and independently [Bucherer 1904].

[20] In his papers of [1905] and [1906], Kaufmann was the first to point out an interesting error in Einstein's equation for the electron's transverse mass in Einstein's 1905 special relativity paper. Correction of this error brought Einstein's prediction in line with Lorentz's. See [Miller 1997, chap. 7].

> conclusions confirmées par vos considérations. Malheureusement mon hypothèse de l'aplatissement des électrons est en contradiction avec les résultats des nouvelles expériences de M. Kaufmann et je crois être obligé de l'abandonner; je suis donc de mon latin et il me semble impossible d'établir une théorie qui exige l'absence complète d'une influence de la translation sur les phénomènes électromagnétiques et optiques.
>
> Je serais très heureux si vous arriviez à éclaircir les difficultés qui surgissent de nouveau.
>
> Veuillez agréer, cher collègue, l'expression de mes sentiments sincèrement dévoués.
>
> H.A. Lorentz

Just how devastated Lorentz was by Kaufmann's results is clear from his letter, dated 8 March 1906. It is a revealing moment in the history of relativity theory. After congratulating Poincaré for the paper "On the Dynamics of the Electron," [Poincaré 1905b; 1906][21] Lorentz wrote that, nevertheless all their work may have been for nothing:

> Unfortunately my hypothesis of the flattening of electrons is in contradiction with Kaufmann's new results, and I must abandon it. I am, therefore, at the end of my wits. It seems to be impossible to establish a theory that demands the complete absence of an influence of translation on the phenomena electricity and optics. I would be very happy it you would succeed in clarifying the difficulties which arise again.

What a remarkable confession, and what a clear-cut case of Popperian falsification. After all those years of work Lorentz was willing to abandon his theory because of the report of a single experiment. Lorentz repeated this message in his lectures at Columbia University, New York City, in March and April 1906 that appeared subsequently as *Theory of Electrons*: "[I]t seems very likely that we shall have to relinquish this idea [Lorentz's electron theory] altogether" [Lorentz 1909, 213]. For his part, Poincaré, too, was strongly affected by Kaufmann's new results; but his reaction was not as radical as Lorentz's. In the introductory paragraphs to the 1905 version of "On the Dynamics of the Electron," Poincaré had written that the theories of Abraham, Langevin and Lorentz "agreed with Kaufmann's experiments." Of course he meant those based on data prior to 1905. But in the last paragraph of the introduction to the 1906 version, Poincaré wrote, concerning

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[21] For a detailed analysis of these papers see [Miller 1973].

Lorentz's theory, that at "this moment the entire theory may well be threatened" by Kaufmann's new 1905 data. Poincaré was still willing for the sake of argument to consider the principle of relativity for the moment as valid in order "to see what consequences follow from it." He called for someone to repeat Kaufmann's experiment [See Poincaré 1908]. Whereas for Lorentz, Kaufmann's results threatened a theory; for Poincaré, a philosophic view was also at stake, one that emphasised a principle of relative motion.

It turned out that Kaufmann's data were incorrect. In a letter of 7 September 1908, A.H. Bucherer in Bonn wrote Albert Einstein in Bern of his recent experimental results concerning the mass of high-velocity electrons: "by means of careful experiments, I have elevated the validity of the principle of relativity beyond any doubt." On 23 September 1908, Bucherer presented his results to the Meeting of German Scientists and Physicians at Cologne; his communication was entitled "Measurements on Becquerel-rays. The Experimental Confirmation of the Lorentz-Einstein Theory." Poincaré and Lorentz welcomed Bucherer's results, and did not call for someone to repeat his experiments. Although, incidentally, thirty years later it turned out that Bucherer's data were inconclusive.[22]

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[22] Lorentz's electron seemed the proper one because experiment supported it. That is, it seemed that way until 1938. In that year two American experimentalists were carrying out electron deflection experiments using electrons from radioactive sources. Their goal was to ascertain whether the electrons emerging from nuclei were really Lorentz electrons and not ones whose mass had some weird velocity dependence so that neutrinos were unnecessary. Knowing their history of physics they realised that they were repeating, with improved apparatus, the old electron deflection experiments of Bucherer and successors. Upon looking into those papers from 20 odd years before they ascertained that all of these physicists used velocity filters whose geometry caused severe loss of accuracy when the electrons' velocities exceeded .7c, just the region where mass variation becomes increasingly evident. Consequently, the only result that any of the old electron deflection experiments demonstrated was that the electron's mass increased with its velocity. By 1938 the problem of the proper expression for the electron's mass had become moot. Nevertheless the interesting problem arises of when should one stop experimenting? As Einstein put it in 1946, comparison between experiment and theory can be "quite delicate." See [Zahn/Spees 1938], [Einstein 1946, 21] and for details of Zahn and Spees' experiments, [Miller 1997, chap. 12].

Concluding Comments

Looking through the key hole we have glimpsed science in the making, that is, the trials and tribulations of two of the major players at the cutting edge of theoretical physics. What is all the more intriguing is that waiting in wings was the person who would slice through all of what Lorentz and Poincaré were worrying about in a manner reminiscent of Alexander cutting the Gordian knot. Yet we should bear in mind that Poincaré's mathematical methods would survive to find their deepest meaning within the new relativity theory of Albert Einstein [See Miller 1997, chap. 12].

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