Poincaré's Light

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Abstract. Light is a recurrent theme in Henri Poincaré's mathematical physics. He scrutinized and compared various mechanical and electromagnetic theories of the optical ether. He gave an important boost to the mathematical theory of diffraction. He analyzed the difficulties encountered in applying Lorentz's electromagnetic theory to optical phenomena. He emphasized the metrological function of light in the nascent theory of relativity. Amidst these optical concerns, he derived his philosophy of *rapports vrais*, hypotheses, conventions, and principles, which in turn oriented the critical enterprise through which he pioneered relativity theory.

Reflections on light, its nature, and its propagation pervade Henri Poincaré's works in physics, from his Sorbonne lectures of 1887-88 to his last considerations on geometry, mechanics, and relativity. These reflections enabled him to solve long-standing problems of mathematical optics, most notoriously in diffraction theory; they nourished his criticism of *fin de siècle* electrodynamics; and they inspired a good deal of his philosophy of science. Although Poincaré began to write on celestial mechanics before he did on optics, and although his first physics course at the Sorbonne was on mechanics, he chose the *Théorie mathématique de la lumière* as the topic of the first course he gave from the prestigious chair of Physique mathématique et calcul des probabilités.[1]

Why did Poincaré favor optics over any other domain of physics? (astronomy and mechanics were not regarded as physics proper, at least according to the definition of classes in the French Academy of Sciences). A first hint is found in the obituary that Poincaré wrote for Alfred Cornu, one of his physics professors at the Ecole Polytechnique:

> He has written much on light. Even though he left his mark on every part of physics, optics was his favorite topic. I surmise that what attracted him in the study of light was the relative perfection of this branch of science, which, since Fresnel, seems to share both the impeccable correction and the austere elegance of geometry. In optics better than in any other domain, he could fully satisfy the natural aspiration of his mind for order and clarity.

[1] The following abbreviations are used: *ACP, Annales de chimie et de physique; AP, Annalen der Physik; CR*, Académie des sciences, *Comptes rendus hebdomadaires des séances*; *PO*i, Henri Poincaré, *Œuvres*, 11 vols. (Paris, 1954), vol. i; *PRS*, Royal Society of London, *Proceedings*. On Poincaré's early biography, cf. Gaston Darboux, "Eloge historique de Henri Poincaré, membre de l'Académie, lu dans la séance publique annuelle du 15 décembre 1913," in *PO*9, VII-LXXI; Scott Walter, "Henri Poincaré's student notebooks, 1870-1878," *Philosophia scientiae*, 1 (1996), 1-17.

It seems reasonable to assume that Poincaré was here projecting the reasons for his own predilection. The foreword to his optical lectures begins with: "Optics is the most advanced part of physics; the theory of undulations forms a whole that is most satisfactory to the mind." Being a mathematician with a special fondness for geometric reasoning, Poincaré was naturally drawn to the elegant and powerful geometry of Augustin Fresnel's theory. There also are biographical and cultural reasons for Poincaré's interest in optics. He got his first notions of optics at the lycée of Nancy, and much more at the Ecole Polytechnique, where the two physics professors Jules Jamin and Alfred Cornu and the physics répétiteur Alfred Potier were leading experts in optics. This is no coincidence. In France, elite physicists had long favored optics as a field of research, because it was closely related to the most prestigious sciences of astronomy and mathematics, and because France had excelled in this domain since Fresnel's decisive contributions to the wave theory of light.[2]

From Cornu's course at the Ecole Polytechnique, Poincaré learned the basics of modern optics: the representation of light as a transverse vibration of an elastic medium (the ether); the empirical laws of dispersion; Fresnel's theory of diffraction; Fresnel's construction of rays in anisotropic media; and the finite velocity of light and related phenomena (stellar aberration and the Fresnel drag). Cornu, like most teachers of optics, avoided the deeper theories that involved the nature of the ether and its partial differential equations of motion. There were too many such theories and no evident criterion to select among them; their exposition would have required more advanced mathematics than used in typical physics courses; and the average French physicist of Cornu's time, being first and foremost a sober experimentalist, had little interest in theoretical speculation.[3]

In contrast, Poincaré judged that his mathematical skills would be best employed if he lectured on the various theories of the ether. He was joining a French tradition of "mathematical physics" in which deeper theory tended to be left to the mathematicians. The challenge was especially high in optics, because of the historical intricacies of its theoretical development. In order to understand the stakes and contents of Poincaré's course, it is necessary to know something about the voluminous literature that Poincaré had to digest and criticize.

1 Optical ether theories

The ether before Poincaré

Although Fresnel's main results, established in the 1820s, can be understood and justified without reference to his precise concept of the ether, there is no doubt that this concept helped him accept the transverse character of luminous vibrations and

[2]Poincaré, "La vie et les œuvres d'Alfred Cornu," *Journal de l'Ecole Polytechnique*, 10 (1905), 143-155, on 146 [Il a beaucoup écrit sur la lumière; si, en effet, il a laissé sa trace dans toutes les parties de la Physique, c'est surtout pour l'Optique qu'il avait de la prédilection. Je crois que ce qui l'attirait dans l'étude de la lumière, c'est la perfection relative de cette branche de la Science, qui, depuis Fresnel, semble participer à la fois de l'impeccable correction et de la sévère élégance de la Géométrie elle-même. Là, il pouvait, mieux que partout ailleurs, satisfaire pleinement les aspirations naturelles de son esprit d'ordre et de clarté.]; *Leçons sur la théorie mathématique de la lumière, professées pendant le premier semestre 1887-1888, rédigées par J. Blondin* (Paris, 1889), I [L'Optique est la partie la plus avancée de la physique; la théorie dite des ondulations forme un ensemble vraiment satisfaisant pour l'esprit.]. On optics and astronomy in 19th-century France, cf. John Davis, "The influence of astronomy on the character of physics in mid-nineteenth century France," *Historical studies in the physical sciences*, 16 (1986), 59-82.

[3]Alfred Cornu, *Cours de Physique, première division, 1874-1875*, cours autographié (Paris: Ecole Polytechnique, 1875).

the kind of elastic response needed to explain propagation in crystals. Fresnel, and most French theorists after him, believed that the ether was a regular lattice of point-like molecules interacting through central forces. They believed the finite spacing of the molecules to be necessary for transverse vibrations. Indeed in a continuous medium ruled by point-to-point central forces, there cannot be any elastic response to a shearing deformation (because the net force between two adjacent layers remains unchanged during a mutual slide of these layers). Moreover, Fresnel believed that the molecular interaction could be adjusted so that his ether would be rigid with regard to optical vibrations and yet liquid with regard to the penetration of ordinary matter. Fresnel's description of his ether was mostly qualitative: he did not derive equations of motion at the molecular or at the medium scale.[4]

The first author to do so was the mathematician Augustin Cauchy, on the basis of the molecular theory of elasticity that Claude Louis Navier, Siméon Denis Poisson, and Cauchy himself had recently developed. For average displacements over volume elements including many molecules, this theory leads to second-order parabolic differential equations of motion involving fifteen arbitrary constants in the general, anisotropic case, and allowing for longitudinal vibrations never seen in optics. In order to account for Fresnel's laws of propagation in anisotropic media, which only involve six constants, Cauchy imposed ad hoc conditions on the elasticity constants. In order to account for Fresnel's laws for the intensity of reflected and refracted light at the interface between two homogeneous isotropic transparent media, which only involve transverse vibrations, he adopted boundary conditions incompatible with mechanical common sense (in particular, the displacement of the medium failed to be continuous at the interface). Later attempts by Franz Neumann, George Green, George Gabriel Stokes, and Gustav Kirchhoff dropped the molecular picture of the ether and replaced it with a continuum approach based on Cauchy's strain and stress tensors (to put it in modern terms). Although these theories encountered similar difficulties, they were regarded as mostly successful. Most physicists in France and abroad were convinced that the ether was some kind of elastic medium obeying the usual laws of mechanics.[5]

Among the many theories of the elastic ether, the "rotational ether" theory and the "labile ether" theory deserve special attentions because they did not share the defects of the former theories: they naturally led to the correct number of elastic constants; they naturally excluded longitudinal vibrations; and their boundary conditions were dynamically correct. The Irish mathematician James MacCullagh proposed the rotational ether theory in 1839 on the basis of the Lagrangian density

$$L = \frac{1}{2}\rho\dot{\mathbf{u}}^2 - \frac{1}{2}(\nabla\times\mathbf{u})\cdot[K](\nabla\times\mathbf{u}), \tag{1}$$

where $\mathbf{u}(\mathbf{r},t)$ denotes the displacement of the medium at point $\mathbf{r}$ and at time t, ρ the density of the ether, and [K] a symmetric operator determining the elastic response of the ether. MacCullagh proceeded inductively from Fresnel's and others' semi-empirical laws. Although he understood that no ordinary medium had the purely rotational elasticity assumed in his theory, he accepted this feature as an indication

[4]On Fresnel's theory, cf. Emile Verdet, Introduction, notes, and comments to Augustin Fresnel, *Œuvres complètes d'Augustin Fresnel, publiées par Henri de Sénarmont, Émile Verdet et Léonor Fresnel*, 3 vols. (Paris, 1866-1870); Edmund Whittaker, *A history of the theories of aether and electricity*, vol. 1: The classical theories (London, 1951); Jed Buchwald, *The rise of the wave theory of light: Optical theory and experiment in the early nineteenth century* (Chicago, 1989); Olivier Darrigol, *A history of optics from Greek antiquity to the nineteenth century* (Oxford, 2012).

[5]Cf. Whittaker, ref. 4; Darrigol, ref. 4.

that "the constitution of the ether, if it ever would be discovered, will be found to be quite different from any thing that we are in the habit of conceiving, though at the same time simple and very beautiful." MacCullagh's approach was not to please his contemporaries, who did not believe that a Lagrangian offered sufficient mechanical understanding. It is only much later that two other Irishmen, George Francis FitzGerald and Joseph Larmor, realized that MacCullagh's theory contained the basic structure of James Clerk Maxwell's electromagnetic theory of light. Indeed MacCullagh's equation of motion,

$$\rho\ddot{\mathbf{u}} = -\nabla \times [K](\nabla \times \mathbf{u}), \tag{2}$$

has the electromagnetic counterpart

$$\mu\ddot{\mathbf{H}} = -\nabla \times [\epsilon]^{-1}\mathbf{D}, \tag{3}$$

if we take $\mathbf{H} = \dot{\mathbf{u}}$ for the magnetic force field, and $\mathbf{D} = \nabla \times \mathbf{u}$ for Maxwell's displacement, $\mu = \rho$ the magnetic permeability, and $[\epsilon] = [K]^{-1}$ for the dielectric permittivity (which is an operator in anisotropic media).[6]

In the same year 1839, Cauchy sketched his so-called "third theory" of the ether, revived in 1888 in a slightly different form and renamed "labile ether" by William Thomson (Lord Kelvin). In this theory the elastic constants of the generic Cauchy-Green theory are adjusted so that the velocity of longitudinal waves (nearly) vanishes. The associated medium is a bit strange: it has negative cubic compressibility; its equilibrium is indifferent to plane compressions (hence the qualification "labile"); and it is best compared to shaving foam. The optical consequences are exactly the same as in MacCullagh's theory, except that the vibrations of the medium occur perpendicularly to the plane of polarization (empirically defined, for instance, by the reflection plane for light polarized by vitreous reflection) whereas in MacCullagh's theory they occur in the plane of polarization. Again, the theory admits an electromagnetic interpretation. Its anisotropic generalization, provided by Richard Glazebrook, rests on the equation of motion

$$[\rho]\ddot{\mathbf{u}} = -K\nabla \times (\nabla \times \mathbf{u}), \tag{4}$$

whose electromagnetic counterpart reads[7]

$$[\epsilon]\ddot{\mathbf{E}} = -\mu^{-1}\nabla \times (\nabla \times \mathbf{E}). \tag{5}$$

if we take $\mathbf{E} = \dot{\mathbf{u}}$, $\mathbf{B} = -\nabla \times \mathbf{u}$, $[\epsilon] = [\rho]$, and $\mu = K^{-1}$.

In general, there were two classes of mechanical ether theories: those for which the vibration belonged to the plane of polarization (Cauchy 1, MacCullagh, Neumann, Green 1, Kirchhoff), and those for which the vibration was perpendicular to this plane (Fresnel, Cauchy 2, Cauchy 3, Green 2, Stokes, Kelvin, Boussinesq). In the first case, the density of the ether is the same in every homogeneous medium;

[6] James MacCullagh, "An essay towards the dynamical theory of crystalline reflexion and refraction," Royal Irish Academy of Sciences, *Transactions*, 21(1848, read 9 Dec. 1839), 17-50. Cf. Darrigol, "James MacCullagh's ether: An optical route to Maxwell's equations?" *European physical journal H*, 2 (2010), 133-172.

[7] Augustin Cauchy, "Mémoire sur la polarisation des rayons réfléchis ou réfractés par la surface de séparation de deux corps isophanes et transparents," *CR*, 9 (1939), 676-691; William Thomson (Lord Kelvin), "On the reflexion and refraction of light," *Philosophical magazine*, 26 (1888), 414-425, 500-501. Cf. Whittaker, ref. 4, 145-147; Darrigol, ref. 4, 235-236. Glazebrook justified the anisotropic density $[\rho]$ by analogy with the anisotropic effective mass of a solid ellipsoid immersed in a perfect liquid.

in the second the elastic constant is the same. The second option was by far the most popular for at least three reasons: it implied a more familiar kind of elasticity; it bore the stamp of Fresnel's authority; it permitted a simple interpretation of the Fresnel drag, as we will see in a moment. Yet the first class of theories is better adapted to anisotropic media because an anisotropic elasticity is easier to imagine than an anisotropic density. In the course of the century, there were several attempts to empirically decide between these two options. For instance, Stokes in 1851 and Ludvig Lorenz in 1860 argued that the observed polarization of diffracted light could only be understood if the vibration was in Fresnel's direction. Much later, in 1890, Otto Wiener used his photographic recording of polarized stationary waves near a metallic reflector to decide in favor of Fresnel's choice.[8]

By Wiener' time, physicists were losing interest in such ether-mechanical questions because Heinrich Hertz's production of electromagnetic waves, in 1888, greatly increased the plausibility of Maxwell's electromagnetic theory of light. Maxwell had arrived at this theory in 1865 on the basis of his field-theoretical interpretation of the received laws of electricity and magnetism. In 1855, guided by hydrodynamic analogies and by Stokes's circulation theorem, he had obtained Cartesian variants of the equations

$$\nabla \times \mathbf{H} = \mathbf{j}, \quad \nabla \times \mathbf{E} = -\partial \mu \mathbf{H}/\partial t, \quad \nabla \cdot \mu \mathbf{H} = 0, \quad \nabla \cdot \epsilon \mathbf{E} = 0 \tag{6}$$

for the fields $\mathbf{E}$ and $\mathbf{H}$, the magnetic permeability μ, the dielectric permittivity ϵ, the electric density ρ, and the (quasi-stationary) current density $\mathbf{j}$. In 1861, on the basis of a mechanical model in which the magnetic field corresponded to the rotation of cells and the electric current to the flow of idle wheels between these cells, Maxwell replaced the equation $\nabla \times \mathbf{H} = \mathbf{j}$ with the more general equation

$$\nabla \times \mathbf{H} - \partial \epsilon \mathbf{E}/\partial t = \mathbf{j}\ , \tag{7}$$

which includes the "displacement current" $-\partial \epsilon \mathbf{E}/\partial t$ caused by the elastic deformation of the cellular mechanism. In 1865, Maxwell realized that his new system of field equations implied the existence of waves traveling at the velocity $1/\sqrt{\epsilon\mu}$, which happened to be very close to the velocity of light. He described monochromatic plane electromagnetic waves of wave vector $\mathbf{k}$ through a triplet of orthogonal vectors $\mathbf{H}$, $\mathbf{D}$, $\mathbf{k}$ and showed that Fresnel's laws of propagation in crystals simply resulted from his equations in anisotropic dielectrics for which the permittivity ϵ became a symmetric 3×3 matrix. Hermann Helmholtz and George Francis FitzGerald later showed that Maxwell's theory provided the correct boundary conditions for deriving the intensities of reflected and refracted light at the boundary between two homogeneous media.[9]

In the same memoir of 1865, Maxwell reformulated his electromagnetic theory in a model-independent form. As he judged his early cellular model of the magnetic field to be too contrived to be true, he now regarded the magnetic field as a hidden mechanism driven by the currents regarded as generalized velocities. The (kinetic) energy of this mechanism been known as a function of the intensity and spatial configuration of the total current (including the displacement current), Lagrange's equations of motion can be formed to obtain the induction law and the electromagnetic force law. As for the displacement current, Maxwell reversed its sign to make

[8]Cf. Whittaker, ref. 4, 328.

[9]Cf. Daniel Siegel, *Innovation in Maxwell's electromagnetic theory: Molecular vortices, displacement current, and light* (Cambridge, 1991); Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford, 2000).

it the time derivative of the polarization ($\epsilon\mathbf{E}$) of the medium (including the ether in a vacuum), in harmony with Faradays concept of electric charge at the surface of a conductor as the spatial interruption of the polarization of the surrounding dielectric.[10]

Before Hertz's decisive experiments, Maxwell's electromagnetic theory had little attraction for continental physicists accustomed to the distance-action theories of André-Marie Ampère, Franz Neumann, and Wilhelm Weber. There were a few exceptions among which we find the French telegraphic engineers who arranged the French translation of Maxwell's treatise. Two of Poincaré's teachers at the Ecole Polytechnique, Cornu and Potier, contributed to the critical apparatus of this translation. The competition between Maxwell's electromagnetic theory of light with earlier mechanical theories possibly contributed to Poincaré's interest in optics.

Although Maxwell's theory of light had the evident advantage of unifying optics and electromagnetism, its superiority to earlier ether theories was not so obvious. There were indeed two kinds of phenomena, the optics of moving body and optical dispersion (also optical rotation) in which the mechanical ether theories performed better. It had long been know that the aberration of fixed stars, discovered by James Bradley in the 1720s and originally interpreted in the corpuscular theory of light could equally be explained in the wave theory of light as a mere consequence of the vector composition of the earth's velocity with the velocity of light. As Fresnel made clear, this explanation required a stationary ether (otherwise the waves would follow the motion of the ether, as sound waves follow the motion of the wind). In addition, one had to assume that refraction in the lenses of the telescope was not affected by its motion through the ether. In another (corpuscular) context, François Arago had found that prismatic refraction did not depend on the motion of the earth. In 1818, he asked Fresnel for a wave-theoretical explanation of this fact. Fresnel answered that the refraction should remain the same if the ether in a transparent body of optical index n acquired the fraction $1 - 1/n^2$ of the velocity of this body (with respect to the stationary ether in a surrounding vacuum). He explained this partial drag by the condition that the mass flux of the ether should be the same on both sides of the interface between the pure ether and the body. In Fresnel's theory, the optical index is indeed proportional to the square root of the density of the medium (the elastic constant being the same in every medium).[11]

In 1851 Hippolyte Fizeau directly confirmed the Fresnel drag by measuring the phase difference between two light beams having traveled through streams of water running in opposite directions. As Cornu explained to his Polytechnique students, this experiment provided a "direct proof of the existence of a vibrating medium other than ponderable matter." Being essentially based on a single ether-matter medium with variable macroscopic parameters of permittivity, permeability, conductivity, and bulk velocity, Maxwell's theory immediately explained Arago's result by a fully dragged ether; but it was hard to conciliate with stellar aberration, and it was totally at odd with Fizeau's result.[12]

[10]Cf. Jed Buchwald, *From Maxwell to microphysics: Aspects of electromagnetic theory in the last quarter of the nineteenth century* (Chicago, 1985).

[11]Cf. Jean Eisenstaedt, *Avant Einstein: Relativité, lumière, gravitation* (Paris, 2005); Whittaker, ref. 4; Darrigol, ref. 4; Michel Janssen and John Stachel, "The optics and electrodynamics of moving bodies," Max Planck Institut für Wissenschaftsgeschichte, preprint 265 (Berlin, 2004).

[12]Cornu, ref. 3, p. 181 of the 1882 edition of the course, p. 213 of the 1895 edition. Stokes assumed that the motion of the ether-matter medium was irrotational and therefore did not curve the rays of light; this assumption turned out to be incompatible with the boundary condition on the surface of the earth.

For the same reason, Maxwell's electromagnetic theory of light ignored optical dispersion. Its wave equations, being hyperbolic equations of second order, led to a propagation velocity independent of the frequency of the waves. In contrast, there were many theories of dispersion based on the mechanical ether. Fresnel originally suggested that the molecular structure of his ether implied a modification of the propagation velocity when the wavelength became comparable to the spacing of the molecules. More precisely, Cauchy showed that the finite spacing of the molecules implied terms of differential order higher than two in the macroscopic equation of propagation. Unfortunately, this simple theory implied that vacuum should itself be dispersive. Subsequent theories by Cauchy and by his followers Charles Briot, Emile Sarrau, and Joseph Boussinesq avoided this and other pitfalls by increasingly separating the ether from the embedded matter. In the end, Boussinesq assumed that the ether and its properties were completely independent of the inclusion of material molecules. After the discovery of anomalous dispersion, in the 1870s Wolfgang Sellmeier and Helmholtz attributed a proper frequency of oscillation to the material molecules and interpreted dispersion as the result of the coupling of the ethereal vibrations with the material oscillators.[13]

This sample of the many ether theories available at the time of Poincaré's lectures should be sufficient to convey the difficulty of conceiving an ether compatible with the known variety of optical phenomena. It also gives an idea of the skills required for the ether builders: a deep understanding of elasticity theory, a firm grasp of the phenomenological laws established by Fresnel and others, an innovative theory of electrodynamics in Maxwell's case, familiarity with the general principles of dynamics in Newtonian and Lagrangian form, and fluency in the calculus of partial differential equations.

Poincaré's first optical lectures (1887-88)

When Poincaré prepared his lectures, Hertz had not yet performed his famous experiments and there was no clear winner among the various theories of the ether. For a pedagogue, the reasonable course would have been to choose among the various competing theories and to dwell on the favorite. This is not what Poincaré did. On the contrary, he expounded no less than eight theories, by Fresnel, Cauchy, Lamé, Briot, Sarrau, Boussinesq, Neumann, and MacCullagh; and he expressed his intention to deal with Maxwell's theory in a subsequent course. This is what physicists occasionally do when they write synthetic reports about the present state of a given domain of physics, as occurred at the British Association for the Advancement of Science and in German encyclopedias in the nineteenth century. But this is not what a good teacher is supposed to do. Poincaré had the excuse of addressing students who had already attended an optics course, and he firmly believed that comparison was the road to truth:[14]

> The theories propounded to explain optical phenomena by the vibrations of an elastic medium are very numerous and equally plausible. It would be dangerous to confine oneself to one of them. That would bring the risk

[13]Cf. Darrigol, ref. 4, 244-260. For the sake of brevity, I do not discuss magneto-optics, which also played a role in the selection between various ether theories (and to which Poincaré later contributed a theory of the anomalous Zeeman effect): cf. Buchwald, ref. 10.

[14]Poincaré, ref. 2, II.

> of blind and therefore deceptive confidence in this one. We must therefore study all of them. Most important, comparison tends to be highly instructive.

Upon reading Poincaré's lectures, one wonders how, in a presumably short time, he could consult so many authors and assimilate so many theories. The answer may perhaps be found in a later remark of his:

> When I read a memoir, I am used to first glance at it quickly so as to get an idea of the whole, and then to return to the points which seem obscure to me. I find it more convenient to redo the demonstrations rather than [going through] those of the author. My demonstrations may be much inferior in general, but for me they have the advantage of being mine.

On the one hand, this method leads to a special clarity, homogeneity, and depth of Poincaré's exposition. On the other hand, it implies departures from the actual contents and intentions of the expounded theories. Not being a historian, Poincaré had more to win on the first account than he had to lose on the second. In particular, the re-demonstration strategy helped him identify shared systems of equations and mathematical structures.[15]

From a mathematical point of view, two distinct ether theories generally differ in two manners: by the partial differential equations of motion and by the boundary conditions at the interface between two media. Poincaré saw that for the most successful theories, the equations of motion in two different theories were related by a simple transformation of the vector representing the vibration. For instance, in MacCullagh's theory the equation of motion is given by equation (2):

$$\rho\ddot{\mathbf{u}} = -\nabla \times [K](\nabla \times \mathbf{u}),$$

Consequently, the vector $\mathbf{v}$ such that

$$\dot{\mathbf{v}} = [K](\nabla \times \mathbf{u}) \tag{8}$$

satisfies

$$[K]^{-1}\ddot{\mathbf{v}} = -\rho^{-1}\nabla \times (\nabla \times \mathbf{v}), \tag{9}$$

in which we recognize the equation of motion (4) of the labile-ether theory if only $[K]^{-1}$ is reinterpreted as a density and ρ^{-1} as an elastic constant. These equations remain valid in heterogeneous media for which the parameters vary in space. Consequently, the boundary conditions at the interface between two homogeneous media can be obtained by taking the limit of a continuous transition layer when the thickness of this layer reaches zero. Thanks to this subterfuge, the mathematical equivalence between the two theories becomes complete. Physical equivalence follows from the remark that the equations of motion (2) and (9) lead to the same expression of the energy density of the vibration, which gives the luminous intensity.[16]

From this equivalence between the various ether theories, Poincaré concluded that it was impossible to empirically determine the direction of the optical vibration.

[15] Poincaré to Mittag-Leffler, 5 Feb. 1889, in *PO*11, p. 69 [Les théories proposées pour expliquer les phénomènes optiques par les vibrations d'un milieu élastique sont très nombreuses et également plausibles. Il serait dangereux de se borner à l'une d'elles; on risquerait ainsi d'éprouver à son endroit une confiance aveugle et par conséquent trompeuse. Il faut donc les étudier toutes et c'est la comparaison qui peut surtout être instructive.]

[16] Poincaré, ref. 2, 399-400. Ludvig Lorenz invented the transition-layer approach.

As we will see in a moment, he refuted the theoretical basis of Stokes's determination of this direction from the polarization of diffracted light. Later, in the 1890s, Poincaré argued that Wiener's stationary-wave experiment equally failed to determine the direction of polarization because in a mechanical theory of the ether there was no reason to assume that the metallic surface reflecting the light in Wiener's device is a nodal surface for the vibrations. All one could assert was that in the electromagnetic theory of light, this surface was a nodal surface for the electric field. Although Cornu and Potier originally supported Wiener, they soon accepted Poincaré's criticism.[17]

Poincaré drew important philosophical lessons form this equivalence between the various theories of light. On the one hand, he emphasized the universality and stability of "the laws of optics and the equations that relate them analytically." This is the first characterization, of what he later called the *rapports vrais* of a theory, that is, relations that are true in any of the competing formulations of a theory and remain approximately true when this theory is replaced by a better one. On the other hand, Poincaré recognized the usefulness of "doctrines coordinating the equations of the theory," doctrines implying what he later called "indifferent hypotheses." For the optical theories, a first indifferent hypothesis is the choice of the direction of vibration. A second is the molecular versus continuum description of the ether. In his lectures of 1887-88 Poincaré adopted the molecular hypothesis, with the following explanation:

> The theory of undulations rests on a molecular hypothesis. For those who believe to be thus unveiling the cause of the law, this is an advantage; for the others, this is a reason for being suspicious. This suspicion, however, seems to me as little justified as the illusion of the believers. The hypotheses play only a secondary role. I could have avoided them; I did not because the clarity of the exposition would have suffered from it. This is the only reason. Indeed the only things I borrow from molecular hypotheses are the principle of energy conservation and the linear form of the equations, which is the general law of small movements and of all small variations.

To better prove this point, in the 1891-1892 sequel to his lectures Poincaré adopted the continuum approach in which the elastic ether is described by Cauchy's strain and stress tensors. In the foreword to his lectures of 1887-88 he pushed agnosticism so far as to question the reality of the ether:

> It matters little whether the ether really exists; that is the affair of the metaphysicians. The essential thing for us is that everything happens as if it existed, and that this hypothesis is convenient for the explanation of phenomena. After all, have we any other reason to believe in the existence of material objects? That, too, is only a convenient hypothesis; only this will never cease to be so, whereas probably the ether will some day be thrown aside as useless. On this very day, however, the laws of optics and the equations that express them analytically will remain true, at least in a first approximation. It will therefore be always useful to study a doctrine that interconnects all these equations.

[17] Otto Wiener, "Stehende Lichtwellen und die Schwingungsrichtung polarisirten Lichtes," *AP*, 38 (1890), 203-243; Poincaré, "Sur l'expérience de Wiener," *CR*, 112 (1891), 325-329; "Sur la réflexion métallique," *CR*, 112 (1891), 456-459. Cf. Scott Walter (ed.), *La correspondance entre Henri Poincaré et les physiciens, chimistes et ingénieurs* (Basel, 2007), 107-108.

In sum, for Poincaré physical hypotheses should not be taken too seriously, although they are useful for the sake of clarification and illustration. In many cases, they only are concrete means to satisfy general principles that have more direct empirical significance, for instance the energy principle or the superposition principle to which Poincaré refers in his defense of the molecular ether. As we will see in a moment, Poincaré later came to favor a more direct application of the principles.[18]

2 Diffraction theory

The Kirchhoff-Poincaré approximation

Besides clarifying the contents and interrelations of the various ether theories, Poincaré's optical lectures brought important insights into the theory of diffraction. Not only Poincaré independently recovered Kirchhoff's main results in this domain, but he addressed mathematical difficulties of which Kirchhoff was unaware, he was able to determine the physical conditions under which Kirchhoff's diffraction formula yields correct results, and he pioneered the study of cases of diffraction in which these conditions did not hold.

Fresnel's theory of diffraction is based on the intuitive idea that the vibration at a point situated beyond a diffracting screen is equal to the sum of vibrations emanating from every point of the screen's opening, with an original amplitude and phase equal to those of the vibration that the source would produce in absence of the screen. While Fresnel did not doubt the mechanical soundness of this intuition, mathematicians like Poisson regarded it as unfounded. In the course of time it nonetheless became clear that the formula gave correct predictions in most cases of diffraction. In a bulky memoir of 1851, Stokes obtained a more precise diffraction formula than Fresnels for the polarized transverse vibrations of an elastic solid representing the ether. His derivation was based on an exact representation of the vibration from an unscreened point source as a retarded integral on a plane. In order to obtain the light diffracted by a screen in this plane, Stokes simply restricted the integration to the screen's opening. Efficient though it is, this derivation has two major defects: the surface integral representation of the vibration is not unique, and the truncation of the integral rests on two unwarranted assumptions: that the vibration in the opening of the screen is the same as if the screen were not there,

[18] Poincaré, ref. 4, II, III [La théorie des ondulations repose sur une hypothèse moléculaire ; pour les uns, qui croient découvrir ainsi la cause sous la loi, c'est un avantage; pour les autres, c'est une raison de méfiance ; mais cette méfiance me paraît aussi peu justifiée que l'illusion des premiers. Ces hypothèses ne jouent qu'un rôle secondaire. J'aurais pu les sacrifier ; je ne l'ai pas fait parce que l'exposition y aurait perdu en clarté, mais cette raison seule m'en a empêché. En effet je n'emprunte aux hypothèses moléculaires que deux choses: le principe de la conservation de l'énergie et la forme linéaire des équations qui est la loi générale des petits mouvements, comme de toutes les petites variations.], I-II [Peu nous importe que l'éther existe réellement; c'est l'affaire des métaphysiciens ; l'essentiel pour nous c'est que tout se passe comme s'il existait et que cette hypothèse est commode pour l'explication des phénomènes. Après tout, avons-nous d'autre raison de croire à l'existence des objets matériels? Ce n'est là aussi qu'une hypothèse commode ; seulement elle ne cessera jamais de l'être, tandis qu'un jour viendra sans doute où l'éther sera rejeté comme inutile. Mais ce jour-là même, les lois de l'optique et les équations qui les traduisent analytiquement resteront vraies, au moins comme première approximation. Il sera donc toujours utile d'étudier une doctrine qui relie entre elles toutes ces équations.]. On Poincaré's hypotheses and "rapports vrais," cf. David Stump, Henri Poincaré's philosophy of science," *Studies in history and philosophy of science*, 20 (1989), 335-363; Igor Ly, *Géométrie et physique dans l'œuvre de Henri Poincaré*, Thèse, Université Nancy 2 (2007); João Principe da Silva, "Sources et nature de la philosophie de la physique d'Henri Poincaré," *Philosophia scientiae*, 16 (2012), 197-222; Darrigol, "Diversité et harmonie de la physique mathématique dans les préfaces de Henri Poincaré," in Jean-Claude Pont et al. (eds.), *Pour comprendre le XIXe : Histoire et philosophie des sciences à la fin du siècle* (Florence, 2007), 221-240.

and that the vibration on the unexposed side of the screen strictly vanishes.[19]

In 1882, Kirchhoff removed the first difficulty by relying on a generalization of Green's theorem that Helmholtz had given in an influential memoir on the vibrations of open organ pipes. For monochromatic sound waves of frequency kc, the wave equation has the form

$$\Delta u + k^2 u = 0. \tag{10}$$

It admits the Green functions

$$G_{\mathrm{M}}(\mathbf{r}) = -\frac{\mathrm{e}^{\mathrm{i}k|\mathbf{r}-\mathbf{r}_{\mathrm{M}}|}}{4\pi|\mathbf{r}-\mathbf{r}_{\mathrm{M}}|} \tag{11}$$

such that

$$\Delta G_{\mathrm{M}} + k^2 G_{\mathrm{M}} = \delta(\mathbf{r}-\mathbf{r}_{\mathrm{M}}). \tag{12}$$

For any two functions f and g of $\mathbf{r}$, we have

$$f\Delta g - g\Delta f = \nabla\cdot(f\nabla g - g\nabla f), \tag{13}$$

whence follows Green's theorem for the volume V delimited by the closed surface ∂V :

$$\int_{\mathrm{V}} (f\Delta g - g\Delta f)\mathrm{d}\tau = \int_{\partial\mathrm{V}} (f\nabla g - g\nabla f)\cdot \mathrm{d}\mathbf{S}, \tag{14}$$

or else

$$\int_{\mathrm{V}} [f(\Delta + k^2)g - g(\Delta + k^2)f]\mathrm{d}\tau = \int_{\partial\mathrm{V}} (f\nabla g - g\nabla f)\cdot \mathrm{d}\mathbf{S}\ . \tag{15}$$

Helmholtz specialized this identity to $f = u$ and $g = G_{\mathrm{M}}$, where u satisfies the free wave equation (10) within the volume V. Call S the boundary ∂V of this volume, V′ the complementary volume (on the other side of S), and $H_{\mathrm{M}}(u,\mathrm{S})$ the Helmholtz integral defined by

$$H_{\mathrm{M}}(u,\mathrm{S}) = \int_{\mathrm{S}} (G_{\mathrm{M}}\nabla u - u\nabla G_{\mathrm{M}})\cdot \mathrm{d}\mathbf{S}\ . \tag{16}$$

Then equation (15) implies the following Helmholtz identities:[20]

$$\text{If } \mathrm{M}\in\mathrm{V}, \qquad H_{\mathrm{M}}(u,\mathrm{S}) = u(\mathrm{M}) \tag{H}$$

$$\text{If } \mathrm{M}\in\mathrm{V}', \qquad H_{\mathrm{M}}(u,\mathrm{S}) = 0\ . \tag{H$'$}$$

Now consider Kirchhoff's diffracting device of Fig. 1, and apply the Helmholtz identity H to the volume V delimited by the closed surface $s\cup s''$ and by a very large sphere s_∞ containing the whole setup. For any point M within this volume, we have

$$u(\mathrm{M}) = H_{\mathrm{M}}(u,\mathrm{s}) + H_{\mathrm{M}}(u,\mathrm{s}'') + H_{\mathrm{M}}(u,\mathrm{s}_\infty). \tag{17}$$

The third term can be ignored because the vibration never reaches a sufficiently remote surface.[21] Kirchhoff further assumes that for a perfectly black screen

[19] Gabriel Stokes, "On the dynamical theory of diffraction," Cambridge Philosophical Society, *Transactions*, 9 (1851, read 26 Nov. 1849), 1-62. Cf. Darrigol, ref. 4.

[20] Gustav Kirchhoff, "Zur Theorie der Lichtstrahlen," Akademie der Wissenschaften zu Berlin, mathematisch-physikalische Klasse, *Sitzungsberichte*, 2 (1882), 641-692; Hermann Helmholtz, "Theorie der Luftschwingungen in Röhren mit offenen Enden," *Journal für die reine und angewandte Mathematik*, 57 (1859), 1-72.

[21] This consideration requires Kirchhoff's extension of the Helmholtz-Green theorem to non-periodic perturbations of the medium, which I omit for the sake of simplicity. I ignore the transverse vector character of the optical vibrations, which does not affect the main results.

(i) $u = 0$, $\partial u/\partial n = 0$, on the external surface s″,

(ii) $u = u_1$, $\partial u/\partial n = \partial u_1/\partial n$ on the internal surface s′

(iii) $u = u_1$, $\partial u/\partial n = \partial u_1/\partial n$ on the surface s of the opening,

wherein $\partial/\partial n$ denotes the normal derivative and u_1 the wave created by the source 1 in the absence of the screen ($u_1(\mathbf{r}) \propto \mathrm{e}^{\mathrm{i}k|\mathbf{r}-\mathbf{r}_1|}/|\mathbf{r}-\mathbf{r}_1|$). Under the assumptions (i) and (iii), equation (17) leads to the Kirchhoff diffraction formula

$$u(\mathrm{M}) = H_{\mathrm{M}}(u_1, \mathrm{s}). \tag{18}$$

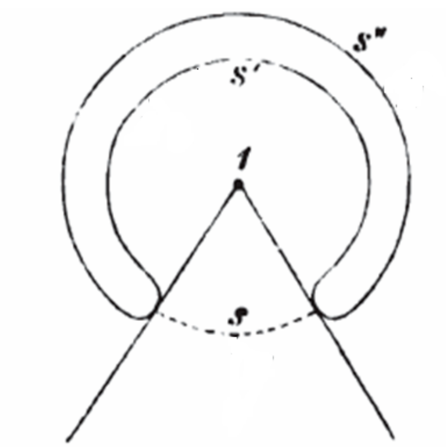

Figure 1: Kirchhoff's diffraction problem. The point source 1 is included in the crescent-shaped cavity with fully absorbing walls $\mathrm{s}' \cup \mathrm{s}''$ and opening s. The observation point is outside the cavity. From Kirchhoff, ref. 20, 80.

In his lectures of 1887-88, Poincaré obtained the same formula by similar means, although he was unaware of Helmholtz's and Kirchhoff's memoirs. Unlike Kirchhoff, he realized that the assumptions (i) and (iii) were mutually incompatible. If both assumptions were true, Poincaré reasoned, for the same integration surface $\mathrm{s}\cup\mathrm{s}''$ and for a point M contained in the volume V′ within the closed surface, the Helmholtz identity H′ would yield $H_{\mathrm{M}}(u_1, s) = 0$, which is generally untrue (consider for instance the case when the width of s is a fraction of a wavelength). Even worse, the seemingly natural conditions (i) and (ii) are also incompatible, because the Helmholtz identity H′ for a point M within (in the substance of the screen) and for the volume V delimited by the surfaces $\mathrm{s}' \cup \mathrm{s}''$, s_∞, and a small sphere s_1 centered on point 1 would then yield

$$H_{\mathrm{M}}(u_1, \mathrm{s}') = -H_{\mathrm{M}}(u_1, \mathrm{s}_1) = u_1 \;; \tag{19}$$

in the absence of the screen, the same identity (applied to the same surfaces and volume) yields

$$H_{\mathrm{M}}(u_1, \mathrm{s}') + H_{\mathrm{M}}(u_1, \mathrm{s}'') = u_1 \;; \tag{20}$$

hence the integral $H_{\mathrm{M}}(u_1, \mathrm{s}'')$ would vanish for any surface s″, which cannot be true. Unknown to Kirchhoff and to Poincaré, in 1869 the mathematician Heinrich

Weber had proved that for a function satisfying the generalization (10) of Laplace's equation, the boundary condition $u = 0$, $\partial u/\partial n = 0$ on any finite portion of a surface implies that the function u should vanish in the whole (connected) domain in which this equation holds. In order to avoid this paradox, the part of the screen invisible from the source must be slightly illuminated, as can be verified experimentally.[22]

Poincaré not only detected a fundamental inconsistency in his and Kirchhoff's theory, but he showed how to circumvent it. As we just saw, Kirchhoff's assumptions leads to $H_{\mathrm{M}}(u_1, \mathrm{s}) = 0$ for any point M within the surface $\mathrm{s} \cup \mathrm{s}''$. Although the integral $H_{\mathrm{M}}(u_1, \mathrm{s})$ generally differs from zero, it is approximately zero when the width of the opening s largely exceeds the wavelength and when M is not too close to the rim of s. Poincaré further noticed that the approximate vanishing of this integral implied the approximate validity of Kirchhoff's diffraction formula (18) for points M outside the surface $\mathrm{s} \cup \mathrm{s}''$, far enough from its rim and not too deeply within the geometric shadow of the screen. Indeed the integral $H_{\mathrm{M}}(u_1, \mathrm{s})$ and its normal derivative are continuous when the point M crosses the surface s'', and they suddenly increase by u_1 and by $\partial u_1/\partial n$ when M crosses the surface s from within (this property is analogous to the discontinuity of the electric field created by a surface charge). Hence, if the integral $H_{\mathrm{M}}(u_1, \mathrm{s})$ approximately vanishes within $\mathrm{s} \cup \mathrm{s}''$, the values of this integral and of its normal derivative are approximately zero on the exterior side of s'' and they are approximately u_1 and $\partial u_1/\partial n$ on the exterior side of s. In addition, this integral is an exact solution of the Helmholtz equation (10) outside the surface $\mathrm{s} \cup \mathrm{s}''$. Consequently, the function that takes the value u_1 within $\mathrm{s} \cup \mathrm{s}'$ and $H_{\mathrm{M}}(u_1, \mathrm{s})$ outside $\mathrm{s} \cup \mathrm{s}''$ is an approximate solution of the wave equation that approximately meets Kirchhoff's boundary conditions. Poincaré completed this reasoning by evaluating $H_{\mathrm{M}}(u_1, \mathrm{s})$ for small wavelengths and showing that in usual cases of diffraction the Kirchhoff integral provided a good approximation of the distribution of diffracted light. Unfortunately, this remarkable explanation of the otherwise surprising success of Kirchhoff's theory seems to have been forgotten.[23]

Large-angle diffraction

Poincaré was aware of a significant exception to Kirchhoff's approximation: the large-angle diffraction experiments performed by Stokes and a few others. In this case, Stokes's theory predicted that diffraction privileged vibrations perpendicular to the diffraction plane (that is, the plane containing the incoming ray and the diffracted ray). Experimenting with a grating, he found the diffracted light to be polarized in the diffraction plane, thus confirming Fresnel's choice of the direction of the vibration. In 1856, the Stuttgart Professor Carl Holtzmann confirmed Neumann's opposite choice in similar experiments. In 1861, independently of this controversy, Fizeau showed that light diffracted by extremely thin stripes on a metallic surface or by an extremely thin slit was almost completely polarized at large diffraction angles. He explained this result by interference and reflection-based phase shift: in

[22]Poincaré, ref. 2, 99-118; *Théorie mathématique de la lumière. II. Nouvelles études sur la diffraction. Théorie de la dispersion de Helmholtz* [1st semester 1891-1892], ed. M. Lamotte and D. Hurmuzescu (Paris, 1893), 182-188. For Poincaré being unaware of Kirchhoff's memoir, see ibid, introduction (2 Dec. 1888), on IV: "Dans le chapitre relatif à la diffraction, j'ai développé des idées que je croyais nouvelles. Je n'ai pas nommé Kirchhoff dont le nom aurait dû être cité à chaque ligne. Il est encore temps de réparer cet oubli involontaire; je m'empresse de le faire en renvoyant aux Sitzungsberichte de l'Académie de Berlin (1882. . .)." On Weber's theorem and diffraction, cf. Arnold Sommerfeld, *Vorlesungen über die theoretische Physik. Band IV: Optik* (Leipzig, 1950), 202.

[23]Poincaré, ref. 2, 115-118 (general reasoning), 118-130 (evaluation of $H_{\mathrm{M}}(u_1, \mathrm{s})$ in the case of a spherical screen with a spherical hole).

the slit experiments, direct (diffracted) light interferes with light reflected by the edges of the slit, in a different manner for the components of the incoming vibration perpendicular and parallel to the length of the slit because these two components undergo different phase shifts by reflection.[24]

In 1886, knowing that earlier experiments on polarization by diffraction had given conflicting results, the Lyon-based physicist Louis Georges Gouy studied the pure case of diffraction by a razor-sharp (metallic) edge. In order to get an observable amount of diffracted light at large angles, he concentrated the light from the sun or from an arc lamp on a point of the edge, and observed the diffracted light through a microscope focused on the edge (Fig. 2). He found that at large angle the diffracted light depended on the polarization of the incoming light, on the material of the edge, and on its sharpness–all against Fresnel's theory; for initially unpolarized light, the internally diffracted light was polarized perpendicularly to the diffraction plane, almost completely so when the angle took it maximal value. Like Fizeau, Gouy surmised that the phenomenon had similarity with metallic reflection, and that the reflectivity of the material and its superficial conductivity played a role when the light traveled near the edge.[25]

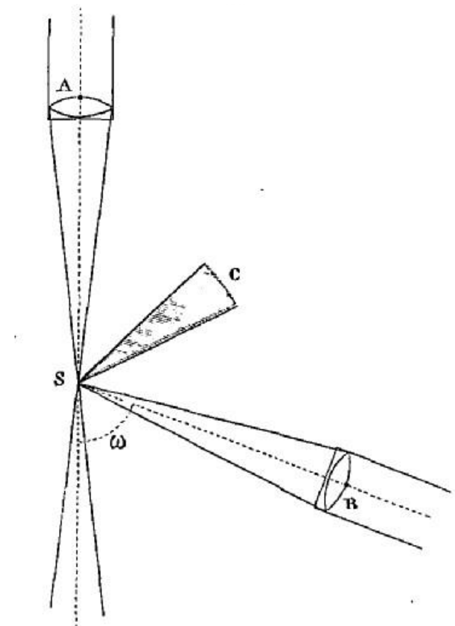

Figure 2: Diffraction of convergent light (from the lense A) by the edge (S) of a razor (CS). The diffracted light is observed at angle through a microscope of objective lens R. From Gouy, ref. 25, 148.

At the very end of his lectures of 1887-88, Poincaré summarized these results and noted that the conditions for the validity of Kirchhoff's approximation were no longer met. The true boundary conditions at the surface of the diffracting screen had to be taken into account, so that, pace Stokes, none of these experiments could

[24]Stokes, ref. 19; Carl Holtzmann, "Das polarisirte Licht schwingt in der Polarisationsebene," *AP*, 99 (1856), 446-451; Hippolyte Fizeau, "Recherches sur plusieurs phénomènes relatifs à la polarisation de la lumière," *CR*, 52 (1861), 267-278, 1221-1232.

[25]Louis Georges Gouy, "Recherches expérimentales sur la diffraction," *ACP*, 52 (1886), 145-192.

decide between Fresnel's and Neumann's choice of the direction of vibration:[26]

> This disagreement with Mr. Gouy's experiments should not surprise us, for we have said that it was impossible to find a solution of the equation $\Delta\xi + \alpha^2\xi = 0$ that satisfied exactly the conditions of the problem. Only by approximately satisfying these conditions could we build a theory of diffraction. The approximation was largely sufficient in the usual conditions of diffraction experiments because the neglected quantities are extremely small in this case. This ceases to be true in the conditions in which Mr. Gouy operated.

Poincaré returned to this question in 1892 in an attempt to explain the results that Gouy had obtained in his seductively simple device. Poincaré first gave an exact solution to the further simplified problem in which the waves converging on the edge and diverging from it are cylindrical waves (so that the problem becomes bidimensional), the metal of the edge is regarded as a perfect conductor (so that the electric lines of force are perpendicular to the surface of the metal), the edge is perfectly sharp and its angle is infinitely small. The fundamental equations of the problem are the Maxwell equations with the boundary condition that the tangential component of the electric field should vanish on the surface of the metal blade. A development of the fields into Bessel functions and some algebra lead to the following expression for the large-distance amplitude $A_{/\!/}(\omega)$ of the light diffracted at the angle ω, when the incident beam is homogeneous, comprised between the angles α and β, and polarized in the diffraction plane:

$$A_{/\!/}(\omega) = \frac{1}{2\pi}\ln\left|\frac{\tan(\frac{\omega-\alpha+\pi}{4})\tan(\frac{\omega+\beta-\pi}{4})}{\tan(\frac{\omega-\beta+\pi}{4})\tan(\frac{\omega+\alpha-\pi}{4})}\right|. \tag{21}$$

When the incident light is polarized perpendicularly to the diffraction plane, the diffracted amplitude is

$$A_{\perp}(\omega) = \frac{1}{2\pi}\ln\left|\frac{\tan(\frac{\omega-\alpha+\pi}{4})\tan(\frac{\omega+\alpha-\pi}{4})}{\tan(\frac{\omega-\beta+\pi}{4})\tan(\frac{\omega+\beta-\pi}{4})}\right|. \tag{22}$$

These formulas agree with three of Gouy's findings: the diffracted light is growingly polarized when the diffraction angle increases; the polarizations for internal and external diffraction are mutually orthogonal; and the intensities of the internally and externally diffracted light are symmetric with respect to the axis of the incoming beam. In other respects, for instance the coloration of diffracted rays and the phase difference between the parallel and perpendicular component, the predictions of the model disagree with Gouy's observations.[27]

In order to remove or alleviate these discrepancies, Poincaré studied the effect of successively removing the simplifying assumptions of his model: infinitely small angle of the diffracting edge, infinite conductivity, and infinitely sharp edge. In

[26]Poincaré, ref. 2, 401 [Ce désaccord entre les expériences de M. Gouy et la théorie de Fresnel ne doit pas nous surprendre, car nous avons dit qu'il était impossible de trouver une solution de l'équation $\Delta\xi + \alpha^2\xi = 0$ satisfaisant *exactement* aux conditions du problème. Ce n'est qu'en y satisfaisant *approximativement* que nous avons pu édifier une théorie de la diffraction. L'approximation était très largement suffisante dans les conditions habituelles des expériences de diffraction ; car les quantités négligées sont alors extrêmement petites. Il n'en est plus de même dans les conditions où M. Gouy s'était placé.]. See also Poincaré, ref. 22 (1893), 195, 213-223.

[27]Poincaré, "Sur la polarisation par diffraction," *Acta mathematica*, 16 (1892), 297-339; ref. 22 (1893), 223-226.

1892, he did this in a rough, purely indicative manner and promised a more rigorous analysis in a later memoir. Four years elapsed before Poincaré published this sequel. By that time, the Göttingen *Privatdozent* Arnold Sommerfeld had given an exact expression for the electromagnetic field in the problem in which incoming plane waves are diffracted by a perfectly conducting half-plane parallel to the wave planes. Poincaré applauded Sommerfeld's "extremely ingenious method," which relied on multivalued solutions of the equation (10) with a branching line on the trace of the diffracting half plane. He also explained the surprising agreement between his and Sommerfeld's asymptotic amplitude formulas by treating Sommerfeld's problem as a limit of the Gouy-Poincaré diffraction problem in which the microscope is focused very far from the diffracting edge. To sum up, Poincaré was first to exactly determine the asymptotic field in an electromagnetic diffraction problem. Sommerfeld was first to give an exact formula for the field near the diffracting half plane for a slightly simpler diffraction problem.[28]

The curving of Hertzian waves

This was not Poincaré's last contribution to the theory of diffraction. After Hertz's production of electromagnetic waves, most physicists soon admitted the electromagnetic nature of light. As the wavelength of Hertzian waves is much larger than the wavelength of ordinary light, these waves undergo a much larger diffraction. When in 1901 Guglielmo Marconi achieved wireless communication across the Atlantic Ocean, diffraction was one of the explanations offered for the waves' surprising ability to travel around the curved surface of the earth. This explanation long competed with Oliver Heaviside's hypothesis of a conducting, reflecting layer in the upper atmosphere, which finally won in the 1920s under the form of what is now called the ionosphere. The diffracted waves turned out to be too weak to explain the quality of long distance transmissions.[29]

Although Poincaré had suggested that diffraction allowed curved propagation around the earth before Marconi's first transatlantic transmission, he was not first to propose a theory of this process. A Cambridge mathematician of Scottish birth, Hector Munro MacDonald, did so in a prize-winning memoir of 1902. MacDonald idealized the earth as a perfectly conducting sphere and the atmosphere as a uniform perfect dielectric, and he sought a spherical-harmonic series solution of Maxwell's equations with vanishing tangential electric field and matching with the dipolar radiation field in the vicinity of the antenna. His estimate of the sum of the series led to a diffraction so large that, as Lord Rayleigh soon pointed out, in the similar problem of a point light source near the surface of a small metal ball, the source would be visible from the opposite side of the ball. In 1903, Poincaré further noted that MacDonald's formulas, being established for any wavelength, should also apply to optical wavelengths in the original earth problem. With a touch of irony, he noted:

> Then if the light remains perceptible for any wavelength and for any posi-

[28]Poincaré, "Sur la polarisation par diffraction," *Acta mathematica*, 20 (1896), 313-355; Sommerfeld, "Mathematische Theorie der Diffraction," *Mathematische Annalen*, 47 (1896), 317-374. Poincaré taught Sommerfeld's theory in 1896: cf. the notes taken by Paul Langevin, *cahier III*: "Elasticité et optique, 1896," Langevin papers, box 123, Ecole Supérieure de Physique et de Chimie Industrielles, Paris (Langevin wrote "Somerset" instead of Sommerfeld).

[29]Cf. Chen-Pang Yeang, "The study of long-distance radio-wave propagation, 1900-1919," *Historical studies in the physical sciences*, 33 (2003), 363-404; Hugh Aitken, *Syntony and spark: Origins of radio* (Princeton, 1985); Sungook Hong, *Wireless: From Marconi's black-box to the audion* (Cambridge, 2001); Aitor Anduaga, *Wireless and Empire: Geopolitics, radio industry and ionosphere in the British Empire, 1918-1939* (Oxford, 2009).

> tion of the source, this means that there is daylight during all night. This conclusion is too manifestly contradicted by experiment.

Poincaré spotted the mathematical error behind this absurdity: MacDonald had identified the limit of the sum of a non-uniformly convergent series with the sum of the limit of its terms. Poincaré concluded:[30]

> These considerations should be sufficient to show the weak point of Mr. MacDonald's reasoning. It would be important to resume the calculations in a manner that takes this difficulty into account, for we want to know whether the results obtained by Mr. Marconi can be explained by present theories and simply result from the exquisite sensibility of his coheror, or instead prove that the waves are reflected by the upper layers of the atmosphere, these layers being made conductors by their extreme rarefaction.

Poincaré returned to this problem in a series of conferences he delivered at the Ecole Supérieure des Postes et Télégraphes in May-June 1908. There he offered a simple intuitive argument leading to the exponential decay of the intensity of the light diffracted along the curved surface of the earth. In the absence of diffraction, Poincaré reasoned, the radiation emitted by the antenna OC in fig. 3 would be restricted to the right angle COF, OF being the tangent to the earth sphere at point O. Call I the intensity of the radiation emitted in a small angle FOD above OF. Owing to diffraction, a fraction α of this radiation should be found in the equal angle FHG under OF. Similarly, a fraction of the latter radiation should be found in the equal angle GKL under GH (the fraction is the same because intuitively diffraction into the shadow only depends on the intensity of light at the limit of the shadow). After n iterations, the intensity in the last equal angle is $I\alpha^n$, so that the radiation decreases exponentially with the distance from the antenna.[31]

Later in the same year Poincaré performed a more serious calculation based on the Legendre-polynomial series solution of a Fredholm equation he deduced from the boundary condition on the sphere and from Maxwell's equations. With a few approximations, he obtained a diffracted intensity (more exactly, a surface current) proportional to $\lambda^{1/4}$ if λ denotes the wavelength. In a Göttingen lecture of April 1909 he concluded: "In this manner we can explain the astonishing fact that it is possible, by means of the Hertzian waves of wireless telegraphy, to communicate from the European continent to America, for example." Alas, Poincaré soon detected an error in his asymptotic estimate of the Bessel functions of his spherical-harmonic development: he had neglected some terms which in reality canceled most of the

[30] Poincaré, "Sur la télégraphie sans fil," *Revue scientifique*, 17 (1902), 65-73, on 68 [Si alors la lumière reste sensible quelle que soit la longueur d'onde et quelle que soit la position de la source, cela veut dire qu'il fait jour pendant toute la nuit; cette conclusion est trop manifestement contredite par l'expérience.], 70 [Ces considérations suffiront, je pense, pour faire comprendre le point faible du raisonnement de M. MacDonald; il serait important de reprendre les calculs en tenant compte de cette difficulté, car il y a lieu de se demander si les résultats obtenus par M. Marconi peuvent s'expliquer par les théories actuelles, et sont dus simplement à l'exquise sensibilité du cohéreur, ou s'ils ne prouvent pas que les ondes se réfléchissent sur les couches supérieures de l'atmosphère rendues conductrices par leur extrême raréfaction.]; Hector Munro MacDonald, "The bending of electric waves round a conducting obstacle," *PRS*, 71 (1903), 251-258; Lord Rayleigh, "On the bending of waves around a spherical obstacle," *PRS*, 72 (1904), 40-41; Poincaré, "Sur la diffraction des ondes électriques: A propos dun article de M. MacDonald," *PRS*, 72 (1904), 42-52, on 42, 52.

[31] Poincaré, "Conférences sur la télégraphie sans fil," *La lumière électrique*, 4 (1908), 259-266, 291-297, 323-327, 355-359, 387-393, on 323. Cf. Jean-Marc Ginoux, "Les conférences 'oubliées' d'Henri Poincaré: les cycles limites de 1908," http://bibnum.education.fr/files/Poincare-analyse.pdf (last accessed Oct. 2012).

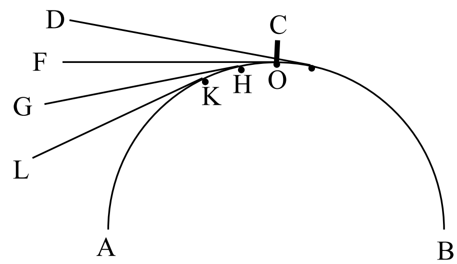

Figure 3: Poincaré's diagram for the terrestrial diffraction of Hertzian waves. The waves are emitted by the antenna OC; AOB is the trace of the earth's surface; the lines ending in D, F, G, L are successive tangents to this surface at equidistant points. From Poincaré, ref. 31, 323 (redrawn).

contribution he had retained. After correcting this error, he found that the diffracted intensity varied as $e^{-\alpha\theta R^3/\lambda^3}$, wherein θ is the angular distance between the emitter and the receiver, λ the wavelength, R the radius of the earth, and α a numerical constant. In his final memoir on this topic, he produced the full corrected calculations with an apology for his "successive palinodes." He now doubted that diffraction could explain long-distance transmission and he noted that the alternative explanation by ionization of the upper atmosphere might perhaps account for the superior quality of nighttime transmissions.[32]

In 1911, an American student of Sommerfeld, Herman William March, filed a dissertation on the propagation of telegraphic waves around the earth and obtained results contradicting Poincaré's. In a letter to Sommerfeld and in a note to the *Compte rendus*, Poincaré identified a fatal error in March's calculation. He also noted that a Trinity Wrangler, John William Nicholson, had confirmed his own exponential law. In fact, Nicholson had improved on Poincaré's method and obtained an estimate (0.696) for the numerical coefficient α in Poincaré's law. As Poincaré noted, this estimate conflicted with recent measurements of long-distance attenuation by Louis Austin's team at the U.S. Naval Wireless Telegraphic Laboratory:

> The attenuation coefficient had been found, *even at daytime*, to be hundred times smaller than the theoretical coefficient resulting from my calculation. The ordinary theory therefore does not account for the facts; something remains to be found.

The diffraction theory nonetheless survived Poincaré's death, until in 1918 the Cambridge-trained mathematician George Neville Watson invented the powerful "Watson transformation" that is now used to solve this kind of problem.[33]

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[32]Poincaré, "Anwendung der Integralgleichungen auf Hertzsche Wellen," in *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik* (Leipzig, 1910), 23-31, on 31 [Auf diese Weise wird die zunächst staunenerregende Tatsache verständlich, dass es mit Hilfe der in der drahtlosen Telegraphie verwendeten Hertzschen Wellen gelingt, vom europäischen Kontinent z. B. bis nach Amerika zu telegraphieren.]; "Sur la diffraction des ondes hertziennes," *CR*, 149 (1909), 92-93 (error corrected); "Sur la diffraction des ondes hertziennes," *Rendiconti del Circolo Matematico di Palermo*, 29 (1910), 159-269, on 268-269. Nicholson (ref. 33) spotted Poincaré's error independently of Poincaré.

[33]Herman William March, "Über die Ausbreitung der Wellen der drahtlosen Telegraphie auf der Erdkugel," *AP*, 37 (1912), 29-50; Poincaré to Sommerfeld (c. March 2012), in Walter, ref. 17, 343-344; John William Nicholson, "On the bending of electric waves round the earth," *Philosophical magazine*, 19 (1910), 276-278, 435-437, 757-760; "On the bending of electric waves round a large sphere," *Philosophical magazine*, 19 (1910), 516-537; 20 (1910), 157-172; 21 (1911), 62-68, 281-295; Poincaré, "Sur la diffraction des ondes hertziennes," *CR*, 154 (1912), 795-797, on 797 [Le coefficient d'affaiblissement a été trouvé, même de jour, cent fois plus faible que le coefficient théorique résultant

3 The nature of white light

The last and least successful intervention of Poincaré in optics *stricto sensu* occurred in a polemic with a physicist he admired particularly, the aforementioned Georges Gouy. In the 1880s Gouy had argued, against Cornu, that the velocity of light in dispersive media as measured by Fizeau's method of the toothed wheel was what we now call the group velocity, not the phase velocity. This interest in chopped or modulated waves brought him to discuss the received view of white light as a random mixture of wave trains of various lengths, origins, and frequencies (as one would expect if the source is made of randomly excited vibrators). In an influential memoir of 1886, Gouy argued that whatever be the detailed mechanism of the production of light, the ethereal motion $s(t)$ on a plane far from the source before entering an optical system could always be represented by a Fourier integral

$$s(t) = \int \tilde{s}(\omega)\mathrm{e}^{\mathrm{i}\omega t}\mathrm{d}\omega \tag{23}$$

and that the time-averaged illumination at the exit of the optical system could be obtained by superposing the illuminations caused by incoming plane monochromatic waves $\mathrm{e}^{\mathrm{i}(\omega t-\mathbf{k}\cdot\mathbf{r})}$ with the weights $|\tilde{s}(\omega)|^2$. Although this result is a trivial consequence of the linearity of the equations of propagation and of Perceval's theorem for Fourier's transforms, it has the counterintuitive consequence that no optical experiment can decide between concepts of natural light that lead to the same frequency distribution $|\tilde{s}(\omega)|^2$. In particular, there should be no way to decide whether the disappearance of fringes in an interference device with large path difference is due to the "complexity" (varying frequency) or to the "irregularity" (disrupted wave trains) of the incoming light.[34]

As Gouy knew, in 1845 Fizeau and Léon Foucault believed to have excluded the second alternative. They used a spectrometer to analyze the light issuing from a double-ray interference device (Fresnel's mirrors) fed by white light. At a given point of the zone of interference, the spectral component of pulsation ω_0 has an amplitude proportional to $1+\mathrm{e}^{\mathrm{i}\omega_0\tau}$, wherein τ denotes the delay caused by the path difference. Therefore, the observed spectrum has periodic dark lines whose number increases with the path difference (*spectre cannelé* or band spectrum). Fizeau and Foucault were able to distinguish these lines for path differences as large as seven thousand wavelengths. They concluded:

> The very restricted limits of path difference beyond which one could not [heretofore] produce the mutual influence of two rays depended only on the complexity of light. By using the simplest light that one might obtain, these limits are considerably shifted. –The existence of these phenomena of the mutual influence of two rays in the case of a large path difference is interesting for the theory of light, for it reveals in the emission of successive waves a persistent regularity that no phenomenon earlier suggested.

Gouy flatly rejected this conclusion, since in his view any irregularity in the source was equivalent to a spread in the Fourier spectrum of the vibration:[35]

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de mon calcul. La théorie ordinaire ne rend donc pas compte des faits, il y a quelque chose à trouver.]. Cf. Gérard Petiau, commentary in *PO*10, 217-219; Yeang, ref. 29, on 398.

[34]Gouy, "Sur le mouvement lumineux," *Journal de physique*, 5 (1886), 354-362. Cf. André Chappert, *L'édification au XIXe siècle d'une science du phénomène lumineux* (Paris, 2004), 247-251.

[35]Fizeau and Léon Foucault, "Mémoire sur le phénomène des interférences entre deux rayons de lumière dans

> Thus, the existence of interference fringes for large path differences does not at all imply the regularity of the incoming luminous motion. This regularity exists in the spectrum, but it is the spectral apparatus that produces it by separating more or less completely the various simple motions which heretofore only had a purely analytical existence.

Poincaré took Fizeau's defense in a note of 1895 for the *Comptes rendus* (Fizeau died the following year). Poincaré first argued that Gouy's reasoning led to the absurd consequence that a source of light, when seen through a spectroscope, should appear permanently illuminated even if the source was turned off. Indeed according to Gouy the spectroscope separates the Fourier components of the vibration, and by definition Fourier components do not depend on time. In order to avoid this paradox, Poincaré introduced the finite resolution of the spectroscope. The amplitude of vibrations at a point of the interference zone can the be written as

$$a(t) \propto \int (1+\mathrm{e}^{\mathrm{i}\omega\tau})\chi(\omega-\omega_0)\tilde{s}(\omega)\mathrm{e}^{\mathrm{i}\omega t}\mathrm{d}\omega, \tag{24}$$

wherein $\chi(\omega-\omega_0)$ characterizes the frequency selection by the spectroscope. In the case of infinite resolution, $\chi(\omega-\omega_0)=\delta(\omega-\omega_0)$, so that

$$a(t) \propto \mathrm{e}^{\mathrm{i}\omega_0 t}\tilde{s}(\omega_0)(1+\mathrm{e}^{\mathrm{i}\omega_0\tau}) \tag{25}$$

and the corresponding intensity does not depend on time. In reality, the resolution of the spectroscope is limited by its finite aperture, and the characteristic function has the form

$$\chi(\omega-\omega_0)=\widetilde{H}(\omega-\omega_0), \tag{26}$$

with $H(t)=1$ for $t_1 \leq t \leq t_2$ and $H(t)=0$ for $t<t_1$ or $t>t_2$, t_1 and t_2 being the times that light takes to travel from each extremity of the aperture to the point of observation. The resulting exit amplitude is

$$a(t) \propto \mathrm{e}^{\mathrm{i}\omega_0 t}\left[\int_{t-t_1}^{t-t_2} s(t')\mathrm{e}^{-\mathrm{i}\omega_0 t'}\mathrm{d}t' + \mathrm{e}^{\mathrm{i}\omega_0\tau}\int_{t+\tau-t_1}^{t+\tau-t_2} s(t')\mathrm{e}^{-\mathrm{i}\omega_0 t'}\mathrm{d}t'\right]. \tag{27}$$

This expression avoids the aforementioned paradox, since it vanishes if the time t is far enough from the period of activity of the source. As Poincaré regarded the oscillating factor $1+\mathrm{e}^{\mathrm{i}\omega_0\tau}$ as empirically established by Fizeau and Foucault, he required that

$$\int_{t-t_1}^{t-t_2} s(t')\mathrm{e}^{-\mathrm{i}\omega_0 t'}\mathrm{d}t' = \int_{t+\tau-t_1}^{t+\tau-t_2} s(t')\mathrm{e}^{-\mathrm{i}\omega_0 t'}\mathrm{d}t' \tag{28}$$

for any τ at which the band spectrum is still seen, and he concluded:[36]

> *The experiment of Fizeau and Foucault teaches us...that the luminous motion enjoys a certain kind of permanence expressed in equation* [(28)]... Thus, a complete analysis leads to exactly the same consequences that Mr. Fizeau's clear-sightedness had guessed in advance.

le cas de grandes différences de marche," *ACP*, 26 (1849), 136-148; "Mémoire sur le phénomène des interférences dans le cas de grandes différences de marche, et sur la polarisation chromatique produite par les lames cristallisées épaisses," *ACP*, 30 (1850), 146-159, on 159; Gouy, ref. 34, on 362.

[36] Poincaré, "Sur le spectre cannelé," *CR*, 120 (1895), 757-762, on 761.

Gouy soon protested: Poincaré had failed to appreciate that the lines of the spectrum could only be separated when the interference delay τ was smaller than the time $t_2 - t_1$ that determines the resolving power of the spectroscope. In this case, the condition (28) is trivially satisfied, no matter how irregular the original motion might be. In sum, the spectroscope is able to produce by itself all the regularity needed to observe interference. Lord Rayleigh and Arthur Schuster had independently come to the same conclusion. This made the nature of white light a matter of speculation. Poincaré silently accepted Gouy's rebuttal. As he later admitted in a different context, "Mathematics are sometimes a hinder, even a danger, when by the precision of their language they induce us to assert more than we know."[37]

4 Optics and electromagnetism

Lecturing on Maxwell

Poincaré had planned a course of lectures on the electromagnetic theory of light even before hearing about Hertz's experiments of the winter 1887-88. He delivered this course in the summer of 1888, with a brief mention of Hertz's findings. The fact that Poincaré studied Maxwell's theory and Hertz's contributions in the context of his optical lectures had important implications. On the experimental side, he interpreted Hertz's discovery as a "synthesis of light" and never missed an opportunity to discuss analogies and disanalogies between light and Hertzian waves.[38] On the theoretical side, he paid special attention to the relations between electromagnetism and optics: "I have spent much time studying the relations between electrodynamics and optics," he wrote in 1901 in an analysis of his works.[39] As we will see in a moment, he was at his best when he analyzed the difficulties of conciliating the optics of moving bodies with the electromagnetic theory of light.[40]

Poincaré's comparison between Maxwell's theory and earlier optics caused a conscious turn in his approach to physical theory. Whereas in his optical lectures he described several mechanical models of the ether and their structural interrelations, in his later courses he often relied on general principles such as the energy principle and the principle of least action to guide theoretical construction or to criticize the products of the construction. This new approach, which Poincaré later called the "physics of principles," had roots not only in thermodynamics but also in Maxwell's Treatise on electricity and magnetism of 1873. As was earlier mentioned, in the

[37]Gouy, "Sur la régularité du mouvement lumineux," *CR*, 120 (1895), 915-917; Rayleigh, "Wave theory of light," in *Encyclopaedia Britannica*, 9th ed. (New York reprint), vol. 24, 421-459, on 425; Arthur Schuster, "On interference phenomena," *Philosophical magazine*, 37 (1894), 509-545; Poincaré, on Pierre Curie and others, *CR*, 143 (1946), 989-998, on 990 [Les mathématiques sont quelques fois une gêne, ou même un danger, quand, par la précision de leur langage, elles nous amènent à affirmer plus que nous ne savons.]. Poincaré's error is the more surprising because in the Hertzian context he had insisted that multiple resonance (in which the resonator plays the role of the spectrometer) was compatible with a damped periodic motion of the electric oscillator.

[38]See, e.g., Poincaré, "La lumière et l'électricité d'après Maxwell et Hertz," *Annuaire du Bureau des longitudes* (1894), A1-A22, on A17; *La théorie de Maxwell et les oscillations hertziennes* (Paris, 1899), chap. 11: Imitation des phénomènes optiques, chap. 12: Synthèse de la lumière; Poincaré, ref. 30 (1902), 66.

[39]Poincaré, Analysis of his scientific works, in PO9, 1-14, on 10 [Je me suis beaucoup occupé des rapports entre l'électrodynamique et l'optique].

[40]As is well known, Poincaré actively contributed to the interpretation of Hertz's and related experiments, and he repeatedly lectured on this topic: Poincaré, *Electricité et optique II. Les théories de Helmholtz et les expériences de Hertz* (Sorbonne lectures, 1889-1890), ed. B. Brunhes (Paris, 1891); *Les oscillations électriques* (Sorbonne lectures, 1892-1893), ed. C. Maurain (Paris, 1894). Cf. Buchwald, *The creation of scientific effects: Heinrich Hertz and electric waves* (Chicago, 1994); Michel Atten, *Les théories électriques en France, 1870-1900. La contribution des mathématiciens, des physiciens et des ingénieurs à la construction de la théorie de Maxwell.* Thèse de doctorat (Paris: EHESS, 1992).

mature form of this theory, Maxwell avoided any specific ether mechanism and contented himself with requiring the Lagrangian form of the field equations. In the foreword to his lectures on Maxwell's theory, Poincaré described "Maxwell's fundamental idea" as follows:

> *In order to prove the possibility of a mechanical explanation of electricity, we need not worry about finding this explanation itself, we only need to know the expression of the two functions T and U which are the two components of the energy, to form the Lagrange equations for these two functions, and then to compare these equations with the experimental laws.*

Poincaré supported this assertion with the mathematical demonstration that any Lagrangian system admitted an infinite number of mechanical realizations. Poincaré generally admired the lofty abstraction he saw in Maxwell's treatise:[41]

> The same spirit pervades the entire work. The essential, namely, what must remain in common in all the theories, is brought to light. Anything that would concern only a particular theory is almost always kept silent. The reader thus faces a form nearly void of matter, a form which he at first tends to take for a fleeting and elusive shadow. However, the efforts to which he is thus condemned prompt him to think, and he at last becomes aware of the somewhat artificial character of the theoretical constructs that he formerly admired.

Lorentz's theory

Qua electromagnetic theory of light, Maxwell's theory had a limited success. On the one hand, it agreed with the measured value of the velocity of light in vacuum (or air); it reproduced Fresnel's laws for the propagation, reflection, and refraction of light; and the theoretical relation between optical index and dielectric permittivity ($\epsilon = n^2$) was roughly verified for substances of weak dispersive power. On the other hand, Maxwell's theory failed to explain dispersion, the optics of moving bodies, and magneto-optics. In the 1890s, the Dutch theorist Hendrik Antoon Lorentz, the German physicist Emil Wiechert, and the Cambridge theorist Joseph Larmor devised a new electromagnetic theory which came to be called "electron theory" after the discovery of the electron in the late 1890s. These theorists assumed that the electromagnetic ether was perfectly immobile (as Boussinesq had done in a mechanical context); that ions, electrons, and any particle of matter moved freely through it; that Maxwell's equations (for a vacuum) held in the pure ether; and that every interaction between ether and matter depended on the ions or electrons (through

[41] Poincaré, *Electricité et optique. I. Les théories de Maxwell et la théorie électromagnétique de la lumière*, Sorbonne lectures of 2nd semester 1887-88 (the date on the title page is wrong), ed. J. Blondin (Paris, 1890), XV (Poincaré's emphasis) [*Pour démontrer la possibilité d'une explication mécanique de l'électricité, nous n'avons pas à nous préoccuper de trouver cette explication elle-même, il nous suffit de connatre l'expression des deux fonctions T et U qui sont les deux parties de l'énergie, de former avec ces deux fonctions les équations de Lagrange et de comparer ensuite ces équations avec les lois expérimentales.*], XVI [Le même esprit se retrouve dans tout l'ouvrage. Ce qu'il y a d'essentiel, c'est-à-dire ce qui doit rester commun à toutes les théories est mis en lumière ; tout ce qui ne conviendrait qu'à une théorie particulière est presque toujours passé sous silence. Le lecteur se trouve ainsi en présence d'une forme presque vide de matière qu'il est d'abord tenté de prendre pour une ombre fugitive et insaisissable. Mais les efforts auxquels il est ainsi condamné le forcent à penser et il finit par comprendre ce qu'il y avait souvent d'un peu artificiel dans les ensembles théoriques qu'il admirait autrefois.].

the Lorentz force and through source terms in the field equations). This simple microphysical theory turned out to explain every known electromagnetic and optical phenomenon, including the optics of moving bodies.[42]

Stellar aberration immediately follows from the assumption of a stationary ether. Although there is no ether drag in Fresnel's sense, light waves are dragged by a moving transparent media as a consequence of interference between direct waves and waves scattered by the ions or electrons. More generally, Lorentz proved that any terrestrial optical experiment (with a terrestrial source) was independent of the motion of the earth through the ether to first order in the ratio u/c of the velocity of the earth to the velocity of light. For this purpose, he first applied the Galilean transformation $x' = x - ut$ (with concomitant field transformations) to his equations, and then introduced the "local time" $t' = t - ux'/c^2$ in order to retrieve the original form of the equations in the ether frame to first order in u/c (t denotes the absolute time, x the abscissa in the direction of the motion of the earth). As this time shift could not affect the stationary patterns of intensity observed in optical experiments, this formal invariance implied the first-order invariance of optical phenomena. For Lorentz, the local time was purely formal; it was similar to the changes of variable that one performs in order to ease the solution of some equation.[43]

Lorentz originally did not expect the invariance of optical phenomena to persist at higher orders in u/c. An experiment performed in 1887 by Albert Michelson and Eduard Morley contradicted this opinion. In a Michelson interferometer, light makes a roundtrip in the two perpendicular arms of an interferometer. If the ether is stationary and if one arm is parallel to the direction of motion of the earth through the ether, the roundtrip in this arm is larger than the roundtrip in the perpendicular arm by a factor $1/\sqrt{1-u^2/c^2}$; a second-order fringe shift should therefore be observed when the interferometer is rotated by 90°. In order to explain the absence of this fringe shift, FitzGerald and Lorentz both assumed that the parallel arm of the interferometer underwent a contraction by $\sqrt{1-u^2/c^2}$ during its motion through the ether. They both argued that the contraction actually derived from electromagnetic theory if the forces responsible for the cohesion of rigid matter behaved like electromagnetic forces with respect to the matter's motion through the ether.

Poincaré, aberration, and all that

Poincaré had a long familiarity with the optics of moving bodies. Already in his *lycée* years in Nancy, he learned about the aberration of fixed stars and its explanation by composing the velocity of light with the velocity of the earth. The fact is significant, since Poincaré later traced the dilemmas of the electrodynamics of moving bodies to the discovery of stellar aberration: "Astronomy raised the question by revealing the aberration of light." At the Ecole Polytechnique, he heard about stellar aberration both in Cornu's physics course and in Hervé Fayes astronomy course. Cornu dwelt on Fizeau's experiment, which he regarded as a proof that matter could not be the sole medium for the propagation of light. Poincaré's *répétiteur* Potier was the man who later corrected Michelson and Morley for miscalculating the path difference in their moving interferometer. In these student years Poincaré performed an ether-

[42]Cf. Whittaker, ref. 4; Buchwald, ref. 10; Darrigol, ref. 9.
[43]Cf. Whittaker, ref. 4; Darrigol, ref. 9.

drift experiment which he remembered many years later:

> I was long ago a student at the Ecole Polytechnique. I must concede that I am extraordinarily clumsy and that since then I have felt I should better stay away from experimental physics. At that time, however, I was helped by a fellow student, Mr. Favé, who is manually very adroit and who, in addition, has a very resourceful mind. We jointly tried whether the translatory motion of the earth affected the laws of double refraction. If our investigation had led to a positive result, that is, if our light fringes had been shifted, this would only have shown that we lacked experimental skills and that the build up of our apparatus was defective. In reality the outcome was negative, which proved two things at the same time: that the laws of optics are not affected by the translatory motion, and that we were quite lucky on this matter.

In 1888, Poincaré devoted the last chapter of his optical lectures to stellar aberration and other optics of moving bodies. He introduced the Fresnel drag directly in the discussion of stellar aberration, as the drag value compatible with George Biddel Airy's finding (in 1871) that water-filling did not affect the stellar aberration observed in a reflecting telescope. Poincaré then expounded Fizeau's running-water experiment, and gave a general proof that the earth's motion did not affect optical experiments on earth if the ether was dragged by transparent bodies according to Fresnel's hypothesis. Here is a modernized, infinitesimal version of this proof.[44]

The velocity of light with respect to the ether in a substance of optical index n is c/n, if c denotes the velocity of light. The absolute velocity of the ether across this substance is $\alpha\mathbf{v}$, where α is the dragging coefficient and $\mathbf{v}$ the absolute velocity of the substance (the absolute velocity being defined with respect to the remote, undisturbed parts of the ether). Therefore, the velocity of light along the element $\mathrm{d}\mathbf{l}$ of an arbitrary trajectory is $c/n + (\alpha - 1)\mathbf{v}\cdot\mathrm{d}\mathbf{l}/\mathrm{ds}$ with respect to the substance (with $\mathrm{ds} = \|\mathrm{d}\mathbf{l}\|$). To first order in u/c, the time taken by light during this elementary travel is

$$\mathrm{d}t = (n/c)\mathrm{d}s + (n^2/c^2)(1-\alpha)\mathbf{v}\cdot\mathrm{d}\mathbf{l}. \tag{29}$$

The choice $\alpha = 1$ (complete drag) leaves the time $\mathrm{d}t$ and the trajectory of minimum time invariant, as should obviously be the case. Fresnel's choice,

$$\alpha = 1 - 1/n^2 \tag{30}$$

yields

$$\mathrm{d}t = (n/c)\mathrm{d}s + (1/c^2)\mathbf{v}\cdot\mathrm{d}\mathbf{l}. \tag{31}$$

[44]Poincaré, "L'état actuel et l'avenir de la physique mathématique," *Bulletin des sciences mathématiques*, 28 (1904), 302-324, on 320 [C'est l'Astronomie, en somme, qui a soulevé la question en nous faisant connatre l'aberration de la lumière.] (I use the excellent translation in the *Bulletin of the American Mathematical Society*, 12 (1906), 240-260); "Die neue mechanik," *Himmel und Erde*, 23 (1910), 97-116, on 104 [Ich war damals Schüler der Ecole Polytechnique. Ich muss ihnen gestehen, dass ich außerordentlich ungeschickt bin, und dass ich seitdem gänzlich auf die Experimentalphysik verzichten zu müssen glaubte. Aber zu jener Zeit sprang mir ein Studiengenosse bei, M. Favé, der manuell sehr geschickt und außerdem ein sehr erfinderischer Kopf ist. Wir verbanden uns also zu Untersuchungen, ob die Gesetze der Doppelbrechung durch die Translation der Erde eine störende Modifikation erfahren. Würden unsere Untersuchungen zu einem positiven Resultat geführt haben, d. h. würden unsere Lichtfransen von ihrer Richtung abgelenkt sein, so würde das nur gezeigt haben, dass wir im Experimentieren keine Erfahrung hatten, und dass die Aufstellung unseres Apparates mangelhaft war. Indessen die Untersuchung verlief negativ, und das bewies zwei Dinge zugleich, nämlich dass die Gesetze der Optik durch die Translation nicht gestört werden, und dass wir bei der Sache viel Glück hatten.]; ref. 2, 379-397; Cornu, ref. 2, 101; Hervé Faye, *Cours d'astronomie, 1e division, 1873-1874* (autographed course, Paris: Ecole Polytechnique), 170-174. On the lycée physics course, cf. Walter, ref. 1.

If the earth is set to move at the velocity **u**, the velocity **v** is turned into **v**+**u** so that the time taken by light to travel between two fixed points of the optical setting differs only by a constant from the time it would take if the earth were not moving. Therefore, to first order in u/c interference phenomena are unchanged, and by Fermat's principle of least time the laws of reflection and refraction are also unchanged. Poincaré concluded: "In one word, optical phenomena can provide evidence only for the relative motion of the luminous source and of the ponderable matter with respect to the observer."[45]

Criticizing Lorentz and others

Poincaré returned to the optics of moving bodies in a criticism of Larmor's and Lorentz's electromagnetic theories published in 1895 in l'*Éclairage électrique* In the spirit of the physics of principles, Poincaré compared three competing electrodynamic theories by Hertz, Helmholtz-Reiff, and Lorentz under three criteria:[46]

1. The theory should account for Fizeau's partial drag.
2. The theory should be compatible with the principle of conservation of electricity.
3. The theory should be compatible with the principle of equality of action and reaction. Poincaré first showed that Hertz's electrodynamics bodies, being based on the assumption of a fully dragged ether was incompatible with Fizeau's result. Then he argued (erroneously) that the Helmholtz-Reiff theory violated the conservation of electricity.

Lastly he showed that Lorentz's theory violated the equality of action and reaction, simply remarking that in this theory electromagnetic waves from a remote source could move a charged particle without compensating recoil of the source or of the medium. In Poincaré's opinion, Lorentz's ether, being essentially immobile and being divorced from matter, was too immaterial to carry any momentum:[47]

> I find it difficult to admit that the principle of reaction is violated, even seemingly, and that this principle no longer holds if one considers the actions on ponderable matter and if the reaction of this matter on the ether is left aside.

As no known theory met Poincaré's three criteria and as the one he judged "the least defective," Lorentz's, violated the principle of reaction, Poincaré drew a radical

[45]Poincaré, ref. 2, 389-391 [En un mot les phénomènes optiques ne peuvent mettre en évidence que les mouvements relatifs par rapport à l'observateur de la source lumineuse et de la matière pondérable.]. As Eleuthère Mascart noted in the 1870s, this justification needs to be modified when double refraction and dispersion are taken into account. Poincaré also gave Boussinesq's theory of the Fresnel drag

[46]Poincaré, "A propos de la théorie de M. Larmor," *L'éclairage électrique*, 3 (1895), 5-13, 289-295; 5 (1895), 5-14, 385-392; also in *PO*9, 369-426, on 395.

[47]Ibid., 412 [Il me paraît bien difficile d'admettre que le principe de réaction soit violé, même en apparence, et qu'il ne soit plus vrai si l'on envisage seulement les actions subies par la matière pondérable et si on laisse de côté la réaction de cette matière sur l'éther.]. Poincaré had a similar objection to Larmor's ether, whose momentum density represented the magnetic field. To Oliver Lodge' failure to detect the ether motion caused by an intense magnetic field in Larmor's theory, Poincaré commented (ibid., 382): "Si le résultat avait été positif, on aurait pu mesurer la densité de l'éther et, si le lecteur veut bien me pardonner la vulgarité de cette expression, il me répugne de penser que l'éther soit si arrivé que cela." Cf. Darrigol, "Henri Poincaré's criticism of *fin de siècle* electrodynamics," *Studies in the history and philosophy of modern physics*, 26 (1995), 1-44.

conclusion: "Some day we will have to break the frame into which we endeavor to fit both the optical and the electrical phemomena."[48]

Poincaré further noted that a multitude of optical experiments led to the following "law":

> It is impossible to detect the absolute motion of matter, better said: the relative motion of ponderable matter with respect to the ether. All we can detect is the motion of ponderable matter with respect to ponderable matter.

Lorentz's theory accounted for this impossibility, but only to first order in u/c . As Poincaré knew, "a recent experiment by Michelson" confirmed the law to second order. Poincaré suspected a deep connection between this weakness of Lorentz's theory and its failure to comply with the reaction principle:[49]

> The impossibility of detecting the relative motion of matter with respect to the ether, and the probable equality of action and reaction without taking the action of matter on the ether into account, are two facts that seem obviously connected. Perhaps the two defects will be mended at the same time.

Poincaré resumed his Sorbonne lectures on electricity and optics in 1899, with an additional chapter on the optics of moving bodies. There he proved that Lorentz's theory implied the invariance of optical phenomena to first order in u/c except for the time shift $t' = t - ux'/c^2$, which he judged too small to be experimentally observable.[50] Then he described the Michelson-Lorentz experiment and the Lorentz-FitzGerald contraction with the comment:

> This strange property would seem a proper nudge [*coup de pouce*] given by nature in order to prevent that the absolute motion of the earth be revealed by optical phenomena. I cannot be satisfied with this state of affairs. Let me tell you how I see things: I regard it as very probable that optical phenomena depend on the relative motion of the material bodies in presence, optical sources and optical apparatus, and this *not only to second order in the aberration [in u/c] but rigorously.* As the precision of experiments grows, this principle will be verified in a more precise manner.

With this bet on the exact validity of the relativity principle in optics, Poincaré broke the contemporary consensus that motion through the ether remained in principle detectable at higher orders in u/c. In most physicists' mind, the ether had enough similarity with an ordinary body or substance to disturb optical phenomena when the earth rushed through it. In Poincaré's mind, the ether was too ethereal

[48]Poincaré, ref. 46, *PO*9, 409 [Il faudra donc un jour ou l'autre briser le cadre où nous cherchons à faire rentrer à la fois les phénomènes optiques et les phénomènes électriques.].

[49]Ibid., 412-413 [Il est impossible de rendre manifeste le mouvement absolu de la matière, ou mieux le mouvement relatif de la matière pondérable par rapport à l'éther; tout ce qu'on peut mettre en évidence, c'est le mouvement de la matière pondérable par rapport à la matière pondérable.][L'impossibilité de mettre en évidence un mouvement relatif de la matière par rapport à l'éther ; et l'égalité qui a sans doute lieu entre l'action et la réaction sans tenir compte de l'action de la matière sur l'éther, sont deux faits dont la connexité semble évidente. Peut-être les deux lacunes seront-elles comblées en même temps.].

[50]Poincaré was also aware of the "Liénard force," which is a first-order correction to the Lorentz force in a moving system.

to be physically detectable. Poincaré therefore criticized Lorentz's order-by-order compensations as provisional subterfuges to be replaced by a better theory:[51]

> Shall we need a new *coup de pouce*, a new hypothesis, at each order of approximation? Evidently no: a well-wrought theory should allow us to demonstrate the principle [that optical phenomena depend only on the relative motion of the implied material bodies] in one stroke and in full rigor. Lorentz's theory does not do that yet. Of all existing theories, it is the theory that is closest to this aim. We may therefore hope to make it completely satisfactory in this regard without altering it too much.

Poincaré expressed similar views in his address to the international congress of physics in Paris in 1900. After describing the fate of the imponderable fluids of older physics, he asked: "And our ether, does it really exist?" He listed a few circumstances in favor of the ether: the elimination of direct action at a distance; Fizeau's experiment, which seemed to require the interplay of two different media, the ether and the running water ("We seem to be fingering the ether"); the violation of the principle of reaction in Lorentz's theory, which seemed to require the ether to carry momentum; and the predicted effects of the ether wind in this theory. Poincaré next recalled that experimenters had failed to detect the latter effects, and he argued that this failure could not be accidental:[52]

> Experiments were performed in which the first-order terms should have been detected. The results were negative. Was it by chance? No one believed so. A general explanation was sought for, and Lorentz found it. He showed that the first-order terms canceled each other; but this was not true for the second-order terms. Then more precise experiments were done. They were also negative. Again, that could not be by chance. An explanation was needed. It was found. Explanations can always be found: Of hypotheses there is never a lack.
>
> This is not enough. Who would not see that chance still plays a too large part? Is it not a singular coincidence if a certain circumstance comes forth

[51]Poincaré, *Electricité et optique. La lumière et les théories électrodynamiques* (Sorbonne lectures of 1888, 1890, and 1899, plus the text of Poincaré, ref. 46), ed. by J. Blondin and E. Néculcéa (Paris, 1901), 536 [Cette étrange propriété semblerait un véritable "*coup de pouce*" donné par la nature pour éviter que le mouvement absolu de la terre puisse être révélé par les phénomènes optiques. Cela ne saurait me satisfaire et je crois devoir dire ici mon sentiment : je regarde comme très probable que les phénomènes optiques ne dépendent que des mouvements relatifs des corps matériels en présence, sources lumineuses ou appareils optiques et *cela non pas aux quantités près de l'ordre du carré ou du cube de l'aberration mais rigoureusement.* A mesure que les expériences deviendront plus exactes, ce principe sera vérifié avec plus de précision.] (Poincaré's emphasis) [Faudra-t-il un nouveau coup de pouce, une hypothèse nouvelle, à chaque approximation? Evidemment non: une théorie bien faite devrait permettre de démontrer le principe d'un seul coup dans toute sa rigueur. La théorie de Lorentz ne le fait pas encore. De toutes celles qui ont été proposées, c'est elle qui est le plus près de le faire. On peut donc espérer de la rendre parfaitement satisfaisante sous ce rapport sans la modifier trop profondément.].

[52]Poincaré, "Sur les rapports de la physique expérimentale et de la physique mathématique," in C. E. Guillaume and L. Poincaré (eds.), *Rapports présentés au congrès international de physique* (Paris, 1900), vol. 4, 1-29, on 21-22 [Et notre éther, existe-t-il vraiment?] [On a fait des expériences qui auraient dû déceler les termes du premier ordre; les résultats on été négatifs; cela pouvait-il être par hasard? Personne ne l'a admis; on a cherché une explication générale, et Lorentz l'a trouvée; il a montré que les termes du premier ordre devaient se détruire, mais il n'en était pas de même de ceux du second. Alors on a fait des expériences plus précises; elles ont aussi été négatives; ce ne pouvait non plus être l'effet du hasard ; il fallait une explication ; on l'a trouvée ; on en trouve toujours ; les hypothèses, c'est le fonds qui manque le moins. –Mais ce n'est pas assez; qui ne sent que c'est encore là laisser au hasard un trop grand rôle? Ne serait-ce pas aussi un hasard que ce singulier concours qui ferait qu'une certaine circonstance viendrait juste à point pour détruire les termes du premier ordre, et qu'une autre circonstance, tout à fait différente, mais tout aussi opportune, se chargerait de détruire ceux du second ordre? Non, il faut trouver une même explication pour les uns et pour les autres, et alors tout nous porte à penser que cette explication vaudra également pour les termes d'ordre supérieur, et que la destruction mutuelle de ces termes sera rigoureuse et absolue.]

> to destroy the first-order terms and then another, completely different but equally opportune circumstance takes care of the second-order terms? No, we must find a common explanation for both kinds of terms; and then there is ample reason to believe that this explanation will also work for terms of higher order and that the compensation will be rigorous and absolute.

Analyzing the crisis

In the same year 1900, Poincaré contributed a memoir on "Lorentz's theory and the principle of reaction" to a volume celebrating Lorentz's jubilee. There he developed his earlier idea that Lorentz's theory implied an intolerable violation of the reaction principle. The Maxwell-Lorentz equations for a system of charged particles of mass m moving at the velocity $\mathbf{v}$ and interacting through the fields $\mathbf{E}$ and $\mathbf{B}$ leads to the equation

$$\sum m\mathbf{v} + \int c^{-1}\mathbf{E} \times \mathbf{B} \mathrm{d}\tau = \text{constant}. \tag{32}$$

Consequently, the momentum of matter is not conserved. Unlike British field theorists, Poincaré refused to interpret the integral of $\mathbf{E} \times \mathbf{B}/c$ as the ether's momentum, even though he showed that the theorem of the center of mass could be saved by regarding this vector as the momentum density of a fictitious fluid moving at the velocity c and created or annihilated by the sources. He indeed believed that any violation of the principle of reaction *when applied to matter alone* led to absurdities. By an argument borrowed from Newton's *Principia*, if the action of a material body A on a material body B differs from the reciprocal action, a rigid combination of A and B would forever accelerate and perpetual motion would thus become possible. A Poincaré noted, this argument presupposes that the net force on the combined body is independent of the acquired motion, in conformity with the relativity principle. In general, Poincaré believed that any violation of the reaction principle had to be intimately related to a violation of the relativity principle. In order to confirm this interconnection in the case of electromagnetic interactions, he considered a Hertzian oscillator placed at the focus of a parabolic mirror and emitting radiation at a constant rate. This system moves with the absolute velocity $\mathbf{u}$ in the direction of emission, and is heavy enough so that the change of this velocity can be neglected during its recoil. For an observer at rest in the ether, the conservation of energy reads

$$S = J + (-J/c)u, \tag{33}$$

where S is the energy spent by the oscillator in a unit time, J the energy of the emitted wave train, and $-J/c$ the recoil momentum according to Lorentz's theory. For an observer moving at the velocity $\mathbf{u}$ of the emitter, the recoil force does not work, and the spent energy S is obviously the same. According to the Lorentz transformations for time and fields (to first order), this observer should ascribe the energy $J(1 - u/c)$ to the emitted radiation and the value $(-J/c)(1 - u/c)$ to the recoil momentum. Hence the energy principle is satisfied for the moving observer, but the (time integral of the) electromagnetic force acting on the emitter is modified by Ju/c^2. Poincaré regarded this difference as a first-order violation of the relativity principle, the expected counterpart of the first-order violation of the principle of reaction.[53]

In this calculation, Poincaré used Lorentz's first-order field transformations, including the local time $t' = t - ux'/c^2$, which he defined in the following manner:[54]

> I suppose that observers placed in different points [of the moving frame] set their watches by means of optical signals; that they try to correct these signals by the transmission time, but that, ignoring their translatory motion and thus believing that the signals travel at the same speed in both directions, they content themselves with crossing the observations, by sending one signal from A to B, then another from B to A. The local time is the time given by watches adjusted in this manner.

Poincaré only made this remark *en passant*, gave no proof, and did as if it belonged to Lorentz. The proof goes as follows. When B receives the signal from A, he sets his watch to zero (for example), and immediately sends back a signal to A. When A receives the latter signal, he notes the time τ that has elapsed since he sent his own signal, and sets his watch to the time $\tau/2$. By doing so he commits an error $\tau/2 - t_-$, where t_- is the time that light really takes to travel from B to A. This time, and that of the reciprocal travel are given by

$$t_- = \mathrm{AB}/(c+u) \text{ and } t_+ = \mathrm{AB}/(c-u), \tag{34}$$

since the velocity of light is c with respect to the ether (see Fig. 4). The time τ is the sum of these two traveling times. Therefore, to first order in u/c the error committed in setting the watch A is

$$\tau/2 - t_- = (t_+ - t_-)/2 = u\mathrm{AB}/c^2. \tag{35}$$

At a given instant of the true time, the times indicated by the two clocks differ by $u\mathrm{AB}/c^2$, in conformity with Lorentz' expression of the local time.

Poincaré transposed this synchronization procedure from an earlier discussion on the measurement of time, published in 1898. There he noted that the dating of astronomical events was based on the implicit postulate "that light has a constant velocity, and in particular that its velocity is the same in all directions." He also explained the optical synchronization of clocks at rest, and mentioned its similarity with the telegraphic synchronization that was then being developed for the purpose of longitude measurement. In his interpretation of Lorentz's local time, Poincaré simply transposed this procedure to moving clocks and to moving observers who could not know their motion through the ether and therefore could only do as if the velocity of light was a constant in their own frame. As we will see, the metro-

[53]Poincaré, "La théorie de Lorentz et le principe de la réaction," in *Recueil de travaux offerts par les auteurs à H. A. Lorentz à l'occasion du 25ème anniversaire de son doctorat le 11 décembre 1900, Archives néerlandaises*, 5 (1900), 252-278. In 1898, Alfred Liénard had already noted the first-order modification of the Lorentz force through a Lorentz transformation. In relativity theory, this modification is compensated by the variation of the mass of the emitter. Cf. Darrigol, "Poincaré, Einstein, et l'inertie de l'énergie," *CR*, 1 (2000), 143-153; "The genesis of the theory of relativity," in T. Damour, O. Darrigol, B. Duplantier, V. Rivasseau (eds.), *Einstein 1905-2005 : Poincaré seminar 2005* (Basel : Birkhäuser, 2006), 1-31.

[54]Poincaré, ref. 53, on 272 [Je suppose que des observateurs placés en différents points, règlent leurs montres à l'aide de signaux lumineux ; qu'ils cherchent à corriger ces signaux du temps de la transmission, mais qu'ignorant le mouvement de translation dont ils sont animés et croyant par conséquent que les signaux se transmettent également vite dans les deux sens, ils se bornent à croiser les observations, en envoyant un signal de A en B, puis un autre de B en A. Le temps local t' est le temps marqué par les montres ainsi réglées.]

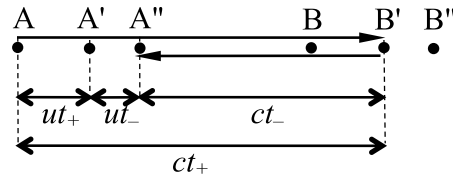

Figure 4: Cross-signaling between two observers moving at the velocity u through the ether. The points $\mathrm{A}, \mathrm{A}', \mathrm{A}'', \mathrm{B}, \mathrm{B}', \mathrm{B}''$ represent the successive positions of the observers in the ether when the first observer sends a light signal, when the second observer receives this signal and sends back another signal, and when the first observer receives the latter signal.

logical function of light had a growing importance in Poincaré's understanding of relativity theory.[55]

Poincaré generalized his worries about violations of general principles in 1904, in a talk delivered during the international exhibition in Saint-Louis, Missouri. There he described the transition from a physics of central forces and mechanical models to a physics of general principles mainly based on the energy principle, Carnot's principle, the relativity principle (so named for the first time), the principle of reaction, Lavoisier's principle of the conservation of mass, and the principle of least action. Poincaré proceeded to show that all these principles, except the last, were in danger. On the relativity principle, he had to say:

> This principle is not only confirmed by our daily experience, not only is it the necessary consequence of the hypothesis of central forces, but it appeals to our common sense with irresistible force. And yet it also is being fiercely attacked.

As Poincaré explained, the threat did not come from experiments but from theory, because even the most successful theory of electromagnetism and optics, Lorentz's theory, could only save the principle approximately and at the price of more or less artificial assumptions: "Thus the principle of relativity has in recent times been valiantly defended; but the very vigor of the defense shows how serious was the attack." Among Lorentz's relativity-saving assumptions, Poincaré singled out the "most ingenious" one, the local time, which he again justified by optical synchronization.[56]

Poincaré next considered the violation of the principle of reaction in Lorentz's theory and the ad hoc character of attempts to save it by making the ether a momentum carrier:

> We might also suppose that the motions of ordinary matter were exactly compensated by motions of the ether... The principle, if thus interpreted, could explain anything since whatever the visible motions we could imagine hypothetical motions to compensate them. But if it can explain anything, it will allow us to foretell nothing; it will not allow us to choose between

[55]Poincaré, "La mesure du temps," *Revue de métaphysique et de morale*, 6 (1898), 371-384. On the telegraphic context, cf. Peter Galison, *Einstein's clocks, Poincaré's maps: Empires of time* (New York, 2003).

[56]Poincaré, ref. 44 (1904), 310 [Celui-là non seulement est confirmé par l'expérience quotidienne, non seulement il est une conséquence nécessaire de l'hypothèse des forces centrales, mais il s'impose à notre bon sens d'une façon irrésistible; et pourtant lui aussi est battu en brèche.], 312 [Ainsi le principe de la relativité a été dans ces derniers temps vaillamment défendu, mais l'énergie même de la défense prouve combien l'attaque était sérieuse.]

> the various possible hypotheses, since it explains everything in advance. It therefore becomes useless.

Here we have a glimpse at Poincaré's refined conception of physical principles: they originate in the generalization of a multitude of experimental results; then, owing to this generality, they tend to be regarded as conventions and thus become immune to refutation; however, they can be endangered when the strategies used to save them become too artificial. In Poincaré's view, the momentum-carrying ether belonged to this sort of degenerative strategy. "This is why," he went on, "I have for a long time thought that the consequences of the theories that contradict Newton's principle [of reaction] would some day be abandoned; and yet the recent experiments on the motion of the electrons emitted by radium seem rather to confirm them."[57]

Poincaré was here alluding to Walther Kaufmann's experiments on the electric and magnetic deflection of fast electrons from radium, and to Max Abraham's explanation of these results in a new dynamics of the electron in which the mass of the electron resulted from the inertia of the accompanying electromagnetic field and therefore depended on the electron's velocity. Interestingly, Abraham obtained this theory by reinterpreting Poincaré's fictitious-fluid momentum ($\mathbf{E} \times \mathbf{B}/c$) of 1900 as a true field momentum and by integrating this momentum for the self-field of the electron. Despite his original commitment to the reaction principle, Poincaré was willing to bow to the new experimental evidence. He even sketched a future relativistic mechanics:[58]

> From all these results, if they were to be confirmed, would issue a wholly new mechanics which would be characterized above all by the fact that there could be no velocity greater than that of light, any more than a temperature below that of absolute zero. For an observer participating in a motion of translation of which he has no suspicion, no apparent velocity could surpass that of light, and this would be a contradiction, unless one recalls the fact that this observer does not use the same sort of timepiece as that used by a stationary observer, but rather a watch giving the local time.'

Poincaré formulated most of his arguments in the conditional mode, not being sure that the threatening experimental results were definitive or that the endangered theory had lost all its steam. His advice to fellow theorists was moderate: "All hope of obtaining better results is not yet lost. Let us, then, take the theory of Lorentz. Let us turn it over and over, let us modify it little by little, and all will be well, perhaps." Poincaré offered one concrete suggestion:

[57]Ibid., 314 [On peut supposer aussi que les mouvements de la matière proprement dite sont exactement compensés par ceux de l'éther, mais cela nous amènerait aux mêmes réflexions que tout à l'heure. Le principe ainsi entendu pourra tout expliquer, puisque, quels que soient les mouvements visibles, on aura toujours la faculté d'imaginer des mouvements hypothétiques qui les compensent. Mais, s'il peut tout expliquer, c'est qu'il ne nous permet de rien prévoir, il ne nous permet pas de choisir entre les différentes hypothèses possibles, puisqu'il explique tout d'avance. Il devient donc inutile.][C'est pourquoi j'ai longtemps pensé que ces conséquences de la théorie, contraires au principe de Newton, finiraient un jour par être abandonnées et pourtant les expériences récentes sur les mouvements des électrons issus du radium semblent plutôt les confirmer.]

[58]Ibid., 316-317 [De tous ces résultats, s'ils se confirmaient, sortirait une mécanique entièrement nouvelle qui serait surtout caractérisée par ce fait qu'aucune vitesse ne pourrait dépasser celle de la lumière... Pour un observateur, entraîné lui-même dans une translation dont il ne se doute pas, aucune vitesse apparente ne pourrait non plus dépasser celle de la lumière; et ce serait là une contradiction, si l'on ne se rappelait que cet observateur ne se servirait pas des mêmes horloges qu'un observateur fixe, mais bien d'horloges marquant le "temps local."]

> Instead of supposing that bodies in motion undergo a contraction in the direction of motion and that this contraction is the same whatever the nature of these bodies and the forces to which they are subjected, could not a simpler and more natural hypothesis be made? One might suppose, for example, that it is the ether which changes when it is in relative motion with respect to the material substance which permeates it; that, thus modified, it no longer transmits the disturbances with the same velocity in all directions. It would transmit more rapidly those disturbances which are being propagated parallel to the motion of the substance, be it in the same direction or in the opposite, and less rapidly those which are propagated at right angles. The wave surfaces would then no longer be spheres, but ellipsoids, and one could do without this extraordinary contraction of all bodies. I am giving this only by way of example, since the modifications that could be tried are evidently susceptible of infinite variation.

In this speculation, Poincaré seems to be taking the ether more seriously than he had done earlier, in conformity with his new willingness to make the ether a momentum carrier. We will return to this point in a moment.[59]

The dynamics of the electron

In the same year 1904, Lorentz perfected his theory in a way that answered at least one of Poincaré's objections: Lorentz now obtained the invariance of optical phenomena at every order in u/c (though only in the dipolar approximation) and through a single transformation of his equations, granted that all forces (including cohesion forces) behaved like electromagnetic forces and that electrons underwent the Lorentz contraction during their motion through the ether. As he had earlier done to first and second order, Lorentz first applied a Galilean transformation to the Maxwell-Lorentz equations in order to take into account the motion of the earth through the ether, and then applied a second transformation, which, combined with the first, brought back the equations to (nearly) the same form as they had in the ether frame. The combined transformation is nearly what Poincaré later called a Lorentz transformation. Lorentz called the transformed field states "corresponding states" and used them only as a formal intermediate step toward the true physical states in the ether. He had not caught Poincaré's hint that the transformed states were those perceived by moving observers under natural conventions.[60]

With the eye of a group theorist, Poincaré immediately saw that with a few minor corrections Lorentz's transformations exactly preserved the form of the Maxwell-Lorentz equations. From this formal invariance, he concluded that under Lorentz's

[59]Ibid., 319-320 [Tout espoir d'obtenir de meilleurs résultats n'est pas encore perdu. Prenons donc la théorie de Lorentz, retournons-la dans tous les sens: modifions-la peu à peu, et tout s'arrangera peut-être.] [Ainsi, au lieu de supposer que les corps en mouvement subissent une contraction dans le sens du mouvement et que cette contraction est la même quelles que soient la nature de ces corps et les forces auxquelles ils sont d'ailleurs soumis, ne pourrait-on pas faire une hypothèse plus simple et plus naturelle? On pourrait imaginer, par exemple, que c'est l'éther qui se modifie quand il se trouve en mouvement relatif par rapport au milieu matériel qui le pénètre, que, quand il est ainsi modifié il ne transmet plus les perturbations avec la même vitesse dans tous les sens. Il transmettrait plus rapidement celles qui se propageraient parallèlement au mouvement au milieu, soit dans le même sens, soit en sens contraire, et moins rapidement celles qui se propageraient perpendiculairement. Les surfaces d'onde ne seraient plus des sphères, mais des ellipsoïdes et l'on pourrait se passer de cette extraordinaire contraction de tous les corps. Je ne cite cela qu'à titre d'exemple, car les modifications que l'on pourrait essayer seraient évidemment susceptibles de varier à l'infini.]

[60]Hendrik Antoon Lorentz, "Electromagnetic phenomena in a system moving with any velocity smaller than light," Royal Academy of Amsterdam, *Proceedings* (1904), 809-831.

assumptions all optical and electrodynamical phenomena complied with the relativity principle:

> Lorentz's idea may be summarized thus: The reason why a common translatory motion can be imparted to the entire system without any alteration of the observable phenomena, is that the equations of an electromagnetic medium are unaltered by certain transformations which we shall call Lorentz transformations. In this way two systems, of which one is fixed and the other is in translatory motion, become exact images of each other.

Poincaré developed and exploited the Lie-group structure of the transformations in his subsequent analysis of the various models of the electron. In an action-based formulation, he determined that Lorentz's model of the contractile electron, completed with proper cohesive stresses, was the only one compatible with the Lorentz-group symmetry. Lastly, he sketched a Lorentz covariant theory of gravitation in which the gravitational force propagated with the velocity of light.[61]

In the spring of 1905, Poincaré modestly announced his *Dynamique de l'électron* as a gloss on Lorentz's results: "I have only be led to modify and complete them with regard to a few details." Yet much of his theory was novel and important: the exact invariance, its relativistic interpretation, its group-theoretical formulation, and its application to non-electromagnetic forces of cohesion or gravitation. The group-theoretical aspects are especially impressive, for they inaugurated a now pervasive style of theoretical physics. On the interpretive side, Poincaré's introduction to his memoir shows that he was not as close to modern relativity as his formal considerations would suggest. After discussing the plausibility of his covariant theory of gravitational forces, he remarked:[62]

> Even assuming, however, that [the new theory turns out to agree with astronomical tests], what conclusion should we draw? If the attraction is propagated with the velocity of light, this cannot be by a fortuitous occurrence; it must be the expression of a function of the ether. Then we will have to investigate the nature of this function and to relate it to the other functions of the fluid [ether]. We cannot be satisfied with formulae that are merely placed side by side and agree only by a lucky chance. These

[61] "Sur la dynamique de l'électron," *Rendiconti del Circolo Matematico di Palermo*, 21 (1906), 129-176, on 130 [L'idée de Lorentz peut se résumer ainsi: si on peut, sans qu'aucun des phénomènes apparents soit modifié, imprimer à tout le système une translation commune, c'est que les équations d'un milieu électromagnétique ne sont pas altérées par certaines transformations, que nous appellerons *transformations de* Lorentz; deux systèmes, l'un immobile, l'autre en translation, deviennent ainsi l'image exacte l'un de l'autre.]. Cf. Jean-Pierre Provost and Christain Bracco, "La théorie de la relativité de Poincaré de 1905 et les transformations actives," *Archive for the history of exact sciences*, 60 (2006), 337-351; "De l'électromagnétisme à la mécanique : le rôle de l'action dans le mémoire de Poincaré de 1905," *Revue d'histoire des sciences*, 62 (2009), 457-493; Michel Le Bellac, "The Poincaré group," in E. Charpentier, E. Ghys, and A. Lesne (eds.), *The scientific legacy of Poincaré* (London, 2010), 329-350. On Poincaré's attempt at a relativistic theory of gravitation, cf. Walter, "Breaking in the 4-vectors: The four-dimensional movement in gravitation, 1905-1910," in Jürgen Renn and Matthias Schemmel (eds.), *The genesis of general relativity*, 4 vols. ((Berlin, 2007), vol. 3, *Gravitation in the twilight of classical physics: Between mechanics, field Theory, and astronomy*, 193-252.

[62] Poincaré, "Sur la dynamique de l'électron," *CR*, 140 (1905), 1504-1508, on 1505 [J'ai été seulement conduit à les modifier et à les compléter sur quelques points de détail.]; ref. 61, 131 [Mais en admettant même que cette discussion tourne à l'avantage de la nouvelle hypothèse, que devrons-nous conclure? Si la propagation de l'attraction se fait avec la vitesse de la lumière, cela ne peut être par une rencontre fortuite, cela doit être parce que c'est une fonction de l'éther; et alors il faudra chercher à pénétrer la nature de cette fonction, et la rattacher aux autres fonctions du fluide. Nous ne pouvons nous contenter de formules simplement juxtaposées et qui ne s'accorderaient que par un hasard heureux; il faut que ces formules arrivent pour ainsi dire à se pénétrer mutuellement. L'esprit ne sera satisfait que quand il croira apercevoir la raison de cet accord, au point d'avoir l'illusion qu'il aurait pu le prévoir.]

> formulae must, as it were, interlock. The mind will consent only when it sees the reason for the agreement, to the point of fancying that it could have predicted this agreement.

Poincaré went on to compare his and Lorentz's theory to the Ptolemaic stage of cosmology. He speculated that a revision of light-based metrology might bring a sort of Copernican revolution in the contemporary theories of matter, electricity, and gravitation:

> Now, there may be an analogy with our problem. If we assume the relativity postulate, we find a number common to the law of gravitation and the laws of electromagnetism, and this number is the velocity of light; and this same number should appear in every other force, of whatever origin. There can be only two explanations for this state of affairs:
>
> - Either everything in the universe is of electromagnetic origin.
> - Or this constituent which is common to all the phenomena of physics is only an appearance [*une apparence*], something that comes from our methods of measurement. How do we measure? By the congruence of objects regarded as rigid, one might first reply. But this is no longer so in our present theory, if the Lorentz contraction is assumed. In this theory, two lengths are by definition equal if they are traversed by light in the same time. Perhaps by abandoning this definition Lorentz's theory would be as deeply changed as Ptolemy's system was by Copernicus' intervention.

The prominence of Poincaré's optical concerns is evident in his extract: in his relativity theory, the velocity of light is the unifying parameter. This fact may be explained in two manners: either by making the optical (electromagnetic) ether the bearer of every interaction as was done in the electromagnetic worldview of some of Poincaré's contemporaries, or by renouncing the light-based metrology that leads to the contraction of lengths through the null result of the Michelson-Morley experiment. The second alternative, the one Poincaré compares to the Copernican revolution, has sometimes been interpreted as an anticipation of Einstein's version of the relativity theory. More likely, Poincaré had in mind something like the heterogeneous ether suggested in his Saint-Louis lecture.[63]

Light and measure

Poincaré's memoir on the dynamics of the electron was a highly mathematical, unusually long and difficult work published in a mathematical journal, the *Rendiconti*

[63]Ibid., 131-132 [Ici il est possible qu'il y ait quelque chose d'analogue; si nous admettions le postulat de relativité, nous trouverions dans la loi de gravitation et dans les lois électromagnétiques un nombre commun qui serait la vitesse de la lumière; et nous le retrouverions encore dans toutes les autres forces d'origine quelconque, ce qui ne pourrait s'expliquer que de deux manières: –Ou bien il n'y aurait rien au monde qui ne fût d'origine électromagnétique. –Ou bien cette partie qui serait pour ainsi dire commune à tous les phénomènes physiques ne serait qu'une apparence, quelque chose qui tiendrait à nos méthodes de mesure. Comment faisons-nous nos mesures? En transportant, les uns sur les autres, des objets regardés comme des solides invariables, répondra-t-on d'abord; mais cela n'est plus vrai dans la théorie actuelle, si l'on admet la contraction lorentzienne. Dans cette théorie, deux longueurs égales, ce sont, par définition, deux longueurs que la lumière met le même temps à parcourir. –Peut-être suffirait-il de renoncer à cette définition, pour que la théorie de Lorentz soit aussi complètement bouleversée que l'a été le système de Ptolémée par l'intervention de Copernic.].

del Circolo Matematico di Palermo. In this text Poincaré did not address the problem of measurement in relativity theory, save for the speculation just mentioned and for a brief mention of stellar aberration and the Michelson-Morley experiment. He did not even repeat his earlier argument that Lorentz's local time represented the time measured by moving observers. There may be several reasons for this silence: Poincaré did not have yet the generalization of his first-order argument to higher orders; his memoir was focused on the dynamics of the electron, not on the effects of the earth motion; it did not involve any change of reference frame, as Poincaré interpreted the Lorentz-transformed field states as the states of a globally boosted physical system with respect to the ether frame; in this active view of the Lorentz transformation, one may imagine that the space- and time-measuring agencies belong to the boosted system, in which case the invariance of the equations describing the global system implies that the transformed field and coordinates are the measured ones.

Poincaré first addressed the metrological aspects of relativity theory in his Sorbonne lectures of winter 1906-1907 on "The limits of Newton's law." He did this in the eleventh chapter on the dynamics of electron, beginning with a discussion of stellar aberration. In conformity with his long familiarity with this phenomenon, Poincaré had introduced both the *Comptes rendus* summary and the Palermo memoir on the dynamics of the electron with the assertion that aberration seemed to allow for the determination of the velocity of the earth through the ether, not the relative velocity of the earth with respect to the observed star.[64] The explanation, first given in his Sorbonne lectures of 1906, runs as follows. On Fig. 5, $\overrightarrow{OA}$ and $\overrightarrow{OA_1}$, with $OA = OA_1 = c$, represent the absolute velocity of light for two stars diametrically opposed on the celestial sphere ("absolute" here means "with respect to the ether"); $\overrightarrow{AB} = \overrightarrow{A_1B_1}$ represents the opposite of the absolute velocity of the sun; $\overrightarrow{BC} = \overrightarrow{B_1C_1}$ the opposite of the velocity of the earth with respect to the sun at a given time of the year; $\overrightarrow{BC'} = \overrightarrow{B_1C_1'}$ the same velocity six months later (all vectors are in the same plane). The first star is observed in the directions of the vectors $\overrightarrow{OC}$ and $\overrightarrow{OC'}$, the second in the directions of the vectors $\overrightarrow{OC_1}$ and $\overrightarrow{OC_1'}$. As the angles COC' and C_1OC_1' are generally different, the angular amplitude of the apparent oscillation of the two stars in the sky are different; this difference is a second-order effect of the absolute velocity of the sun.[65]

Poincaré went on to explain why such effects of the motion of the earth or of the solar system through the ether had never been observed. He first detailed the optical synchronization of moving clocks he had given in 1900, with a new emphasis on the transitivity of this procedure: if the clock A is synchronized with the clock B, and if the clock B is synchronized with the clock C, then the clock A should be synchronized with clock C for any given choice of the positions of the three clocks; otherwise the discrepancy would give us a means to detect motion through the ether.

[64]The first occurrence of this assertion is in the Saint-Louis lecture, ref. 44 (1904), 320.

[65]Poincaré, "Les limites de la loi de Newton," Sorbonne lectures of Winter 1906-1907, ed. by M. Chopinet after notes taken by Henri Vergne, in *Bulletin astronomique publié par l'observatoire de Paris*, 17 (1953), 121-365, on 216-217 (Vergne's original notes have recently been found at the Bordeaux Observatory; I thank Scott Walter for showing me the pages concerning the light ellipsoid; these do not significantly differ from the printed version); "La dynamique de l'électron," *Revue générale des sciences pures et appliquées*, 19 (1908), 386-402, on 390-391; "La mécanique nouvelle," *Revue scientifique*, 12 (1909), 170-177, on 172. As Poincaré explained in 1908 (p. 193), the difference in the angular amplitude, if it were large enough to be observable, would be compensated by the Lorentz contraction of the instrument used to measure the angles (a divided circle would become a divided ellipse).

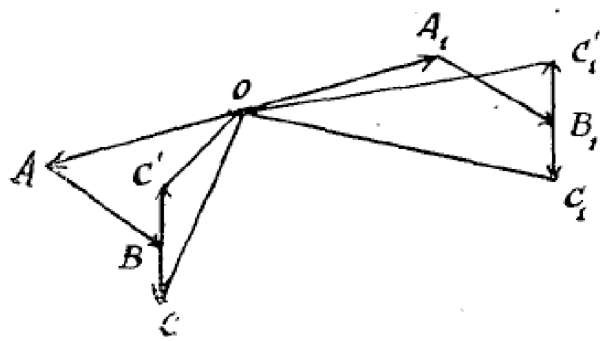

Figure 5: Poincaré's diagram for his discussion of second-order stellar aberration. From Poincaré, ref. 65 (1909), 172.

The first-order optical procedure meets this criterion, because it gives the following condition: the clocks A and B are synchronized if they indicate the same time for any two events whose true times differ by $u(x_B - x_A)/c^2$, where x_A and x_B are the abscissae of these events in the direction of the motion of the earth.[66]

In order to extend this reasoning to any order in u/c, Poincaré took into account the Lorentz contraction in the optical synchronization. He considered an observer moving with the constant velocity $\mathbf{u}$ through the ether and emitting a flash of light at time zero. At the value t of the true time, this light is located on a sphere of radius ct centered at the emission point. Poincaré next considered the appearance of this light shell when measured with Lorentz-contracted rulers belonging to the moving frame. The result is an ellipsoid of revolution, the half-axes of which have the values $a = \gamma ct$ and $b = ct$ (see Fig. 6). As the eccentricity is $e = \sqrt{1 - b^2/a^2} = u/c$, the focal distance $f = ea = \gamma ut$ is equal to the apparent distance traveled by the observer during the time t. Therefore, the Lorentz contraction is the contraction for which the position of the observer at time t coincides with the focus F of the light ellipsoid he has emitted.[67]

Now consider a second observer traveling with the same velocity $\mathbf{u}$ and receiving the flash of light at the time t_+. The position M of this observer belongs to the ellipsoid $t = t_+$, and the distance FM represents the apparent distance between the two observers, which is invariable. According to a well-known property of ellipses, we have

$$\mathrm{FM} + e\mathrm{FP} = b^2/a \, , \tag{36}$$

where P denotes the projection of M on the larger axis. The length FP being equal to the difference x' of the apparent abscissae of the two observers, this implies

$$t_+ = \gamma \mathrm{FM}/c + \gamma u x'/c^2. \tag{37}$$

[66]Poincaré, ref. 65 [1906-1907], 217-219.

[67]Strange though it may seem to Einsteinian relativists, Poincaré's light-ellipsoid admits an operational interpretation in Einstein's theory. For this purpose, we need to assume that the rest frame is equipped with optically synchronized clocks at every point. Suppose that at a given value of the common time of these clocks, two of them are reached by a light pulse earlier emitted by a point source attached to the moving frame, and suppose that at this time the extremities of a rod attached to the moving frame coincide with the position of these two clocks. If l is the length of this rod in the moving frame, the distance between the two clocks (in the rest frame) is the Einstein-contracted length l/γ. Therefore, the longitudinal dimensions of the light pulse as measured in this manner are exaggerated by a factor γ: the spherical pulse is turned into Poincaré's ellipsoid. Those who think that the implied mixture of measurements performed in two different frames is too contrived should consider that Einstein's own derivation of the contraction of lengths rests on a similar mixture.

Suppose that the two observers synchronize their clocks by cross-signaling. The traveling time of the reverse signal is

$$t_- = \gamma \mathrm{FM}/c - \gamma u x'/c^2. \tag{38}$$

Therefore, two events are judged simultaneous by these observers if and only if the true times of these events differ by

$$(t_+ - t_-)/2 = \gamma u x'/c^2. \tag{39}$$

This condition is obviously transitive.[68]

In a later use of the ellipsoid, to be found in a text of 1908, Poincaré no longer discussed the transitivity of apparent simultaneity. Instead he showed that the apparent time t'_+ could be defined so that the apparent velocity of light be equal to c in every direction. Indeed, if we take

$$\gamma t'_+ = t_+ - \gamma u x'/c^2, \tag{40}$$

from equation (37) we get

$$\mathrm{FM} = c t'_+. \tag{41}$$

Dropping the + index and calling x the abscissa of the second observer, we also have

$$x' = \gamma(x - ut), \tag{42}$$

which, together with the former equation, implies

$$t' = \gamma(t - ux/c^2). \tag{43}$$

Poincaré's ellipsoid can thus be used to derive the Lorentz transformations. Poincaré, however, rather saw the light ellipsoid as a simple geometrical means to prove that the Lorentz contraction implied the apparent isotropy of light propagation for observers using optically synchronized clock and contracted rulers. In turn, this isotropy implied the invariance of optical phenomena.[69]

As Poincaré explained in the following paragraph of his Sorbonne lectures of 1906-1907, the Lorentz transformations could be regarded as connecting the field equations in the ether frame to the apparent field equations that relate the apparent field quantities and the apparent space and time coordinates in a moving frame. Apparent quantities are those measured by moving observers ignoring their motion through the ether and therefore misestimating the true quantities defined in the ether frame. The difference with Einstein's theory is obvious: although Poincaré, like Einstein, assumed the complete invariance of *observable phenomena* when passing from an inertial frame to another, he did not require the invariance of the *theoretical description* of the phenomena. In his view, the ether frame was a privileged

[68] Ibid., 219-220. As I show in the appendix, Poincaré's claim that the Lorentz contraction is necessary to the transitivity of simultaneity is incorrect.

[69] Poincaré, ref. 65 (1908), 393. The two last equations on that page translate into $\tau = t - \gamma u x'/c^2$ and $\mathrm{FM} = \gamma^{-1} ct$. There is an obvious misprint in the latter equation: it should be $\mathrm{FM} = \gamma^{-1} c\tau$. Moreover, Poincaré calls τ "the apparent duration of transmission," whereas it is only proportional to the apparent time. This loose terminology probably results from Poincaré's focus on the isotropy of apparent propagation. His commentary to $\mathrm{FM} = \gamma^{-1} c\tau$ indeed reads: "Namely, the *apparent* duration of transmission is proportional to the *apparent* distance. This time, the compensation is *exact*, and this is the explanation of Michelson's experiment." Poincaré improved this aspect of his reasoning in lectures delivered in July 1912 at the Ecole Supérieure des Postes et des Télégraphes: *La dynamique de l'électron*, ed. by Viard and Pomey (Paris, 1913), 45-46. In this last occasion (on 44), Poincaré briefly mentioned that "mechanical phenomena were accelerated by a translatory motion." Otherwise, he never discussed time dilation: cf. Thibault Damour, *Si Einstein m'était conté* (Paris, 2005), chapter 1.

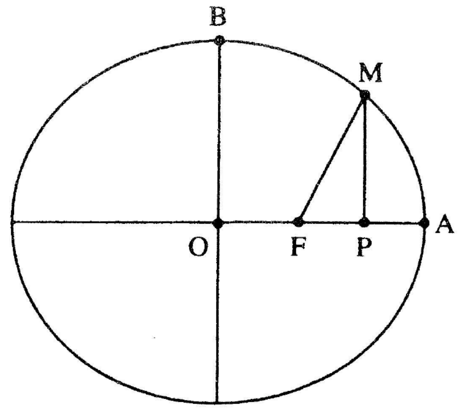

Figure 6: Poincaré's light ellipsoid ($a = \mathrm{OA}, b = \mathrm{OB}, f = \mathrm{OF}$).

frame in which true space and time were defined. The Lorentz-transformed quantities in another frame were only "apparent." Of course, the choice of the ether frame could only be conventional, since the relativity principle excluded any empirically detectable difference between the various inertial frames.[70]

This attitude explains some features of Poincaré's light ellipsoid that may seem very odd to a modern, Einsteinian reader. For Poincaré, like for Lorentz, the Lorentz contraction is meant to be a physical effect of the motion of a material body through the ether; it is not the sort of perspectival effect conceived by Einstein; measuring a light pulse at a given value of the true time with contracted rods is a natural operation because the pulse is meant to be a disturbance of the ether and because the contraction is caused by the motion of the rulers through the ether. Although this point of view contradicts Einsteinian intuitions, it is self-consistent and it leads to the correct expression of the Lorentz transformations.

The ether

One may still wonder why Poincaré chose to maintain a rigorously undetectable ether. Had he not written, some twenty years earlier, that "probably the ether will some day be thrown aside as useless"? As a supporter of Felix Klein's definition of geometry through a group of transformation, was not he prepared to define a new geometry based on the Lorentz group? Poincaré answered this question in a talk given toward the end of his life: he was perfectly aware of the possibility (exploited by Hermann Minkowski) of defining the geometry of spacetime through the Lorentz group; at the same time, he still believed that the choice of the group defining a geometry was largely conventional and that ancestral habits were important in judging the convenience of a convention:[71]

[70]Ibid., 221. For a comparison between Einstein's and Poincaré's approaches, cf. Darrigol, ref. 53; "The mystery of the Einstein-Poincaré connection," *Isis*, 95 (2004), 614-626.

[71]Poincaré, "L'espace et le temps," *Scientia*, 12 (1912), 159-170, on 170 [Quelle va être notre position en face de ces nouvelles conceptions ? Allons-nous être forcés de modifier nos conclusions ? Non certes: nous avions adopté une convention parce qu'elle nous semblait commode, et nous disions que rien ne pourrait nous contraindre à l'abandonner. Aujourd'hui certains physiciens veulent adopter une convention nouvelle. Ce n'est pas qu'ils y soient contraints ; ils jugent cette convention nouvelle plus commode, voilà tout ; et ceux qui ne sont pas de cet avis peuvent légitimement conserver l'ancienne pour ne pas troubler leurs vieilles habitudes. Je crois, entre nous, que c'est ce qu'ils feront encore longtemps.]. On Minkowski's views, see Walter, "Minkowski, mathematicians, and the

> What should be our position with regard to these new conceptions? Shall we be compelled to modify our conclusions? No, assuredly: we had adopted a convention because it seemed convenient to us and we were saying that nothing could force us to abandon it. Today some physicists want to adopt a new convention. Not that they are forced to do so. They simply judge this convention to be more convenient. Those who do not share this opinion can legitimately keep the old convention in order not to disturb their old habits. Between you and me, I believe they will do so for a long time.

For those who wanted to preserve the old conventions for space and time, the ether was still useful as a reference frame for true space and time. There is an additional reason for Poincaré's preservation of the ether. Until 1900, the only role he gave to the ether was the illustration of propagation phenomena. He denied that the ether could have any detectable motion or momentum. As long as he believed in a strict validity of both the relativity principle and the reaction principle (for matter alone), he could not take the ether very seriously. However, at the beginning of the century he came to doubt the validity of the reaction principle (when applied to matter alone). The main source of this doubt was Kaufmann's experiments and their interpretation by electromagnetic inertia. Also, Poincaré was eager to correct excessive interpretations of the conventionalism he had earlier promoted:

> 'Did you not write,' you might say if you were seeking a quarrel with me, 'did you not write that the principles, though they are of experimental origin, are now beyond the possibility of experimental attack, because they have become conventions? And now you come to tell us that the triumphs of the most recent experiments put these principles in danger.' Very well, I was right formerly, and I am not wrong today. I was right formerly, and what is taking place at present is another proof of it.

In a hardened conventionalism, the necessity of certain conventions and the superfluity of others are exaggerated. In 1902, in a refutation of Edouard Le Roy's nominalism, Poincaré defended the ether against the latter sort of exaggeration:

> It can be said, for instance, that the ether has less reality than any external body. To say that this body exists is to say that there is an intimate, robust, and persistent relation between its color, its flavor, and its odor. To say that the ether exists, is to say that there is a natural relationship between all kinds of optical phenomena. Evidently, one proposition does not weigh more than the other.

Poincaré had offered the same comparison in 1888, with the nearly opposite conclusion that the ether, unlike ordinary bodies, would probably someday be rejected.[72]

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mathematical theory of relativity," in H. Goenner, J. Renn, J. Ritter, and T. Sauer (eds.), *The expanding Worlds of General Relativity (Einstein Studies 7)* (Boston, 1999), 45-86.

[72]Poincaré, ref. 44, 322 [N'avez-vous pas écrit, pourriez-vous me dire si vous vouliez me chercher querelle, n'avez-vous pas écrit que les principes, quoique d'origine expérimentale, sont maintenant hors des atteintes de l'expérience parce qu'ils sont devenus des conventions? Et maintenant vous venez nous dire que les conquêtes les plus récentes de l'expérience mettent ces principes en danger. Et bien, j'avais raison autrefois et je n'ai pas tort aujourd'hui.]; "Sur la valeur objective de la science," Revue de métaphysique et de morale, 10 (1902), 263-293, on 293 [On peut dire par exemple que l'éther n'a pas moins de réalité qu'un corps extérieur quelconque ; dire que ce corps existe, c'est dire qu'il y a entre la couleur de ce corps, sa saveur, son odeur, un lien intime, solide et persistant ; dire que l'éther existe, c'est dire qu'il y a une parenté naturelle entre tous les phénomènes optiques, et les deux propositions n'ont évidemment pas moins de valeur l'une que l'autre.].

After Kaufmann's experiments of 1901, Poincaré admitted the possibility that the ether could carry momentum and even that all matter could be represented as a set of singularities of the ether. As we saw, in 1904 he contemplated the possibility that any inertia would be of electromagnetic origin, in which case the ether would carry every momentum in nature. In 1906, he cautiously announced "the end of matter" in a popular conference:

> One of the most astonishing discoveries announced by physicists during the past few years is that matter does not exist. I hurry to say that this discovery is not yet definitive... [If Kaufmann's results and the Lorentz-Abraham theory are correct], every atom of matter would be made of positive electrons, small and heavy, and of negative electrons, big and light... Both kinds have no mass and they only have a borrowed inertia. In that system, there is no true matter; there are only holes in the ether.

One might think that the expression "hole in the ether," which Poincaré repeated in later conferences, was meant to capture the imagination of a popular audience. This would however contradict Poincaré's understanding of the purpose of popularization, which we may infer from his appreciation of Kelvin's *Popular lectures*:

> Another remark immediately comes to mind. Where should we search for [Kelvin's] deepest ideas? In his Popular lectures. These lectures are not mere popularizations for which he would have more or less reluctantly sacrificed a few hours taken from more serious work. He did not humble himself in speaking to the people, for it is often in front of them and for them that his thoughts arose and took their most original form. Therefore, the same pages offer substance both to the beginner and to the scholar. How so? The evident reason is the nature of his mind: he did not think in formulas, he thought in images. The presence of a popular audience, the necessity to be understood from this audience naturally suggested images to him, images which were the normal generator of his thinking.

Poincaré, like Kelvin, liked to think through images, and some of his best ideas emerged when he was trying to find the right image. His seemingly naïve description of the ether reflects a move toward a more physical concept of the ether as a momentum carrier. Late in his life in a conference on materialism, he insisted: "Thus the active role is removed from matter to be transferred to the ether, which is the true seat of the phenomena that we attribute to mass. Matter no longer is; there are only holes in the ether."[73]

[73]Poincaré, "La fin de la matière," *Athenaeum*, 4086 (1906), 201-202 [L'une des découvertes les plus étonnantes que les physiciens aient annoncées dans ces dernières années, c'est que la matière n'existe pas. Hâtons-nous de dire que cette découverte n'est pas encore définitive... Ainsi tout atome matériel serait formé d'électrons positifs, petits et lourds, et d'électrons négatifs, gros et légers... Les uns et les autres sont dépourvus de masse et n'ont qu'une inertie d'emprunt. Dans ce système il n'y a pas de vraie matière. Il n'y a plus que des trous dans l'éther] (Poincaré was aware of new, contradictory experiments of Kaufmann, but judged that a definitive conclusion was premature); "Lord Kelvin," *La lumière électrique*, 1 (1908), 139-147, on 139 [On ne peut s'empêcher de faire une autre remarque. Où faut-il chercher ses idées les plus profondes? Dans ses *Popular Lectures*. Ces leçons ne sont donc pas de simples vulgarisations, en vue desquelles il aurait sacrifié plus ou moins à regret quelques heures prises sur un travail plus sérieux. Il ne s'abaissait pas pour parler au peuple, puisque c'est souvent devant et pour lui que sa pensée prenait naissance et revêtait sa forme la plus originale. C'est donc dans les mêmes pages que le lecteur novice et le savant pourront aller chercher et trouver un aliment. Comment cela se fait-il? Cela vient évidemment de la nature de son esprit, il ne pensait pas en formules, il pensait en images ; la présence de l'auditoire populaire, la nécessité de s'en faire comprendre lui suggérait naturellement l'image, qui était pour lui la génératrice habituelle de la pensée]; "Les

To summarize, after Kaufmann's experimental discovery of electromagnetic inertia and after his own discovery that moving observers measure Lorentz-transformed fields and coordinates, Poincaré employed the ether in two manner: as a momentum carrier in which momentum was globally conserved, and as a privileged reference frame in which the conventions of the usual geometry could be maintained. Whereas for Einstein the ether became superfluous in relativity theory, for Poincaré the ether offered a way to conciliate relativity theory with older intuitions and conventions. This is not to say that Poincaré was unaware of the possibility of changing the conventions to get rid of the ether. He just did not believe in the necessity of such a radical move.

Conclusions

The physics of light inspired Poincaré in four different manners: as a useful employment of his mathematical skills, as an entry into methodology and philosophy, as a touchstone for electromagnetic theory, and as a precondition for a new light-based metrology. In the first register, he greatly advanced diffraction theory both in the optical and in the Hertzian domain. In the second, he developed a pluralist view of physics in which several illustrative models were allowed to coexist even though the shared structure was the essential, stable core of the theory; he detected and defended a move of physics from specific modeling to organizing principles; he articulated a nuanced understanding of the principles of physics as provisionally convenient conventions of empirical origin. In the third register, he discussed the compatibility of the optics of moving bodies with the electromagnetic interpretation of light; this led him to the first general formulation of the relativity principle and to the full group-theoretical apparatus of relativity theory. In the fourth and last register, he recognized that the time coordinate of optically synchronized clocks should depend on the motion of the reference frame to which they are attached, and that optically measured lengths were Lorentz-contracted.

These achievements of Poincaré were deeply interrelated: the mathematical development of various ether theories helped him identify their shared mathematical structure; considerations of structure and principles inspired his critical insights into the optics and electrodynamics of moving bodies; his awareness of a new light-based metrology arose in this context. His concern with light, its behavior, its nature, and its uses thus offers a unified explanation of its most important breakthroughs in mathematical physics.

Appendix - Light-based measurement in the Lorentz-Poincaré ether theory

Consider a rod AB moving uniformly in the ether at velocity u along the x axis and making a constant angle with this axis (the rod could be one arm of a Michelson interferometer, or a materialization of the distance between two terrestrial observers). A flash of light is emitted at one end of the rod; it is reflected at the other extremity, and returns to the first end. Call x the projection of the length of the rod on the x axis, y the projection on the perpendicular axis in the plane of the rod, t_+ the time

conceptions nouvelles de la matière," in Paul Doumergue (ed.), *Le matérialisme actuel* (Paris, 1913), 49-67, on 65 [Voilà le rôle actif enlevé à la matière, pour être transféré à l'éther, véritable siège des phénomènes que nous attribuons à la masse. Il n'y a plus de matière, il n'y a plus que des trous dans l'éther.]

that the light takes to travel from A to B, and t_- the time it takes to travel from B to A. Drawing the light trajectories in the ether frame as it done for t_+ in Fig. 7 (Poincaré imagines such figures for the Michelson-Morley experiment and for the first-order local time), we have

$$c^2t_+^2 = y^2 + (x + ut_+)^2 \ , \quad c^2t_-^2 = y^2 + (x - ut_-)^2, \tag{44}$$

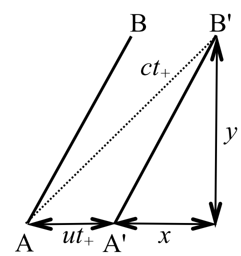

Figure 7: The path (AB') of light between the extremities of a rod AB moving at the constant velocity u.

The difference of these two equations leads to

$$t_+ - t_- = \frac{2ux}{c^2 - u^2} \ . \tag{45}$$

According to Poincaré's optical synchronization, two events occurring in A and B are judged simultaneous in the moving frame if and only if the difference of their true times is $(t_+ - t_-)/2$. Now suppose that x is proportional to the difference $x'_A - x'_B$ of the apparent abscissae of A and B. Then apparent simultaneity is transitive because

$$x'_A - x'_C = (x'_A - x'_B) - (x'_C - x'_B) \ . \tag{46}$$

This is true whatever the contraction factor for the parallel component of the rod may be. Hence Poincaré erred when, in 1906-1907, he claimed that the Lorentz value of the contraction was necessary to warrant the transitivity of apparent synchronization. Any contraction or no contraction at all would do.

However, the Lorentz value is necessary in order that the traveling times be the same in the two arms of a Michelson interferometer. That the Lorentz contraction still does the job for any orientation of the interferometer or for any angle between its two arms can be proved by showing that the round-trip time $t_+ + t_-$ does not depend on the orientation of the rod AB if we assume the Lorentz contraction

$$x = x'\sqrt{1 - u^2/c^2}, y = y' \ , \tag{47}$$

x' and y' being the two projections of the rod when it does not move (or the apparent projections in the moving frame). Adding the two equations (44) and using equation (45), we get

$$(t_+ + t_-)^2 = \frac{4}{c^2 - u^2}\left(\frac{x^2}{1 - u^2/c^2} + y^2\right). \tag{48}$$

Hence the Lorentz contraction leads to

$$(t_+ + t_-)^2 = \frac{4}{c^2 - u^2}(x'^2 + y'^2) = \frac{4l^2}{c^2 - u^2} \ , \tag{49}$$

if l denotes the length of the rod at rest. This quantity does not depend on the inclination of the rod, as was to be proved.

Possibly, sometime after showing the ellipsoid to his students Poincaré realized that the Lorentz contraction was not required for the transitivity of optical synchronization. In the 1908 version of the ellipsoid, transitivity is no longer mentioned; the new purpose of the ellipsoid is to show that "the *apparent* duration of transmission is proportional to the *apparent* distance," with the comment: "This time the compensation is *rigorous*, and this is the explanation of Michelson's experiment." Indeed, in the ellipsoid argument, it is only for Lorentz's value of the contraction that the focus F of the ellipse coincides with the apparent position of the source. And it is only in this case that FM is proportional to the apparent transmission time.

Possibly, Poincaré arrived at the light ellipsoid by geometrically interpreting the equations (44). Indeed, in terms of the non-contracted projections x' and y' given by (47), we have

$$t_+^2 = y'^2 + (\gamma^{-1}x' + ut_+)^2, \tag{50}$$

which is the equation of an elongated ellipsoid of eccentricity u/c if $\gamma = 1/\sqrt{1-u^2/c^2}$, with the focus F such that OF $= \gamma u t_+$ (and therefore coinciding with the apparent position of the source). This is exactly the Poincaré ellipsoid.

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