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HENRI POINCARÉ AND SPECIAL RELATIVITY

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1. In the later part of the last century and at the beginning of the present one, physicists had great difficulties in reconciling rational mechanics and electromagnetism. A solution was presented by A. Einstein in 1905, in the framework of a theory which was gradually accepted and given about ten years later (by Einstein) the name of *special relativity*. However, other people had worked and published earlier on these questions, most importantly for us in this paper H. A. Lorentz and H. Poincaré. The relationships between the contributions of Lorentz and Poincaré, or of Lorentz and Einstein are fairly well established, but this is not so for those of Einstein and Poincaré. In fact, for many years, there has been (and still is) some controversy as to how much Poincaré had anticipated or independently developed special relativity. Much has been written on this topic and many opinions have been expressed. To start with, I'll mention three (others will follow later):

2. *a*) In 1953, Sir Edmund Whittaker published the second volume of his *"History of Aether and Electricity"* [W]. It contains a chapter on these matters, entitled "The relativity theory of Lorentz and Poincaré". The 1905 paper of Einstein [E1] is mentioned (p. 40): it sets forth the relativity theory of Lorentz and Poincaré, with some amplifications. It also introduces the postulate of the constancy of the speed of light, which was widely accepted at first and heavily criticized later.

The second 1905 paper by Einstein on relativity [E2], containing the relation $E = mc^2$, is quoted (p. 52) as introducing a special case of that formula, already anticipated by Poincaré, stated in full generality first in 1908 by C. N. Lewis.

b) In his book on Einstein: *"Subtle is the Lord..."* [P1], published in 1982, A. Pais states (p. 21) that *Poincaré never understood the basis of special relativity.* (See footnote [7]) below for a less abrupt statement.)

c) The mathematician and mathematical physicist Shlomo Sternberg, who did not agree with that assessment, wrote in an unpublished report (1983) he had asked for the opinion of some of his physicist friends who have looked into the subject: "*most feel H. Poincaré had indeed all or almost all of the basic concepts of special relativity in advance of or independently of Einstein*".

3. In this lecture, I shall try to sketch the development of these ideas, and to present, in particular, the information known to me on the contributions, views and mutual influences of these three scientists. The literature on this topic is so abundant that practically all opinions have been held and I do not pretend to offer a new one, all the more since the background material is known. Most of it is mentioned or discussed in several places, notably in the books of Pais [P1], A. Fölsing [F2], and A. Miller [M1]. A further difficulty in this discussion stems from the relative rarity of statements by Poincaré on Einstein, and vice versa. In particular, I do not know of any officially recorded one by Poincaré on Einstein and special relativity (or, as he would have said, the *new mechanics*).

4. In the second half of the 19th century, there was general agreement on the wave nature of light: it propagates in a medium, the aether, with the same speed c in all directions for an observer at rest with respect to the aether. However, nobody knew what the aether was, nor how to ascertain to be at rest with respect to it. But, owing to the revolution of the earth around the sun, one could hope to detect a relative uniform motion of a laboratory on earth with respect to the aether. The most decisive experiment in this respect was carried out by Michelson and Morley in 1887; it was negative. It turned out to be impossible to observe any optical effect of the earth's motion relative to the aether, at any rate up to second order in v/c, where v is the assumed speed of the earth with respect to the aether.

Around 1890 the Irish physicist G. Fitzgerald and, somewhat later, independently, Lorentz proposed an explanation: "the contraction hypothesis". Recall that in the Michelson-Morley experiment two rays of light go back and forth on two rods of the same length, ℓ, at right angles, one $\overline{OA}$ moving with speed v with respect to the aether, while the other one $\overline{OB}$ is assumed to be perpendicular to the direction of motion relative to the aether, and then are brought to interfere. According to the Newtonian addition of speeds, there should be a difference between the travelling times taken by these two rays, but the experiment did not show any. However, if it is assumed that the rod $\overline{OA}$ is shortened by the factor $(1 - v^2/c^2)^{1/2}$, then the times are indeed equal. Lorentz introduces and discusses this assumption in §§89–92 of his monograph [L3].

5. In 1898, H. Poincaré published a remarkable paper: "The measure of time" [P6]. Each human being has a feeling of a flow of time, a succession of events, which appears to reflect an absolute time. But how can one define that objective time? Poincaré singles out two difficulties. First, we have no direct intuition of the equality of two intervals of time. However, this had already been pointed out and he even gives some references. But a second point had not attracted much attention so far: to say that two events at different places occur at the same time has no objective meaning. Conventions on the measure of time are needed. Simultaneity is not an absolute concept. It is difficult to separate the qualitative problem of simultaneity from the quantitative problem of the measure of time, no matter whether a chronometer is used, or whether account must be taken of a velocity of transmission, such as that of light, because such a velocity could not be measured without *measuring* a time.

I must say that when I studied special relativity, over fifty years ago, this point eventually appeared to me as a most crucial one (and one which could be grasped without any formula). Apparently, this is indeed what had also happened to Einstein in 1905, as I shall recall in a moment, but the conclusion of Poincaré indicates that he did not attach a particularly fundamental importance to it:

> To conclude: We do not have a direct intuition of simultaneity, nor of the equality of two durations. If we think we have this intuition, this is an illusion. We replace it by the aid of certain rules which we apply almost always without taking account of them.
>
> But what is the nature of these rules? No general rule, no rigorous rule; a multitude of little rules applicable to each particular case.
>
> These rules are not imposed upon us and we might amuse ourselves in inventing others; but they could not be cast aside without greatly complicating the enunciation of the laws of physics, mechanics and astronomy.
>
> We therefore choose these rules, not because they are true, but because they are the most convenient, and we may recapitulate them as follows: "The simultaneity of two events, or the order of their succession, the equality of two durations, are to be so defined that the enunciation of the natural laws may be as simple as possible. In other words, all these rules, all these definitions are only the fruit of an unconscious opportunism."

In the same vein, it seems rather telling to me that when Poincaré included this article in [P10], he did not indicate the original reference. In fact when I read it, also some fifty years ago, I wondered why Einstein was not mentioned at all, not realizing that that particular chapter had already been published in 1898.

6. In 1902, Einstein settled in Berne. After some time, he was hired as a 3rd class expert at the Patent Office. Before, in order to make ends meet, he had given some private lessons. One of his (few) students was a Rumanian, Maurice Solovine, who was more interested in general knowledge, philosophy, than in mathematics or physics *per se.* Their conversations went quickly beyond the framework of private lessons, they became friends and Solovine later proposed to meet regularly to read and discuss together some important books. Together with another friend of Einstein's, the mathematician C. Habicht, they created to that effect what they called the "Akademie Olympia". In later reminiscences on the Akademie Olympia, in the introduction of [S3], Solovine listed some of the books they had read. He singled out one which "deeply impressed us and kept us breathless for weeks on end" ("*un livre qui nous a profondément impressionnés et tenus en haleine pendant de longues semaines*"), namely, a collection of essays published by Poincaré in 1902 [P8]. It consists of various articles dealing with mathematics or physics. There is no precise record of what they actually read, but it is very likely that they devoted much attention to the two chapters most relevant for us. One, "Theories of modern physics", is part of the text of a lecture given at an international congress in Paris, 1900, though this is not indicated there. Poincaré discusses the contraction hypothesis but would like a general principle according to which it would not be possible to detect relative motion with respect to the aether. After describing the hypothetical properties of that medium, he asks whether it really exists.

Another paper they surely read, too, is "Classical Mechanics", at the beginning of which Poincaré stresses four points:

1) There is no absolute space, only relative motions.
2) There is no absolute time. The equality of two time intervals has no meaning by itself.
3) We have no direct intuition of the simultaneity of two events occurring at different locations, and he refers to [P6] for a discussion.
4) Euclidean geometry is only some sort of convention of language. Non-euclidean geometry would be less convenient, but equally legitimate.

Points 1), also stressed by E. Mach, and 4) no doubt left a mark.

Einstein later acknowledged that he owed that insight to the sharp-witted ("*scharfsinnig*") Poincaré. However it does not seem that 2) and 3) struck a chord at that time. At any rate, they were not referred to (by Einstein, or anyone else for that matter), at that time.

7. It seems that these were the only works of Lorentz or Poincaré known to Einstein when he wrote [E1]. Specifically, a letter to C. Seelig written in 1955 ([S2], p. 116) implies that he did not know [L4], [L5], [P9] at the time. ("*... as far as I am concerned, I knew only Lorentz's important work of 1895, but not his later one nor the subsequent work by Poincaré. In that sense, my work was independent.*") In the early fifties, he also told A. Pais that he had never read [P12] (see [P1], p. 171). I shall now discuss briefly the contributions of Lorentz and Poincaré or, rather, only that part which is most relevant here, up to 1906, it being understood that Einstein became aware of them only later.

In 1899 Lorentz published another paper on the contraction [L4]. He realized that for the equations of electromagnetism to remain valid in a frame in uniform relative motion v (in the x-direction), one needed not only to make the change of spatial coordinates

$$(1) \qquad x' = (x - vt)(1 - v^2/c^2)^{-1/2}, \qquad y' = y, \qquad z' = z$$

but also to define a new time in the moving frame. As such, he proposed

$$(2) \qquad t' = t - vx/c^2 .$$

He had already introduced it in [L3], pp. 49–50 and had called it "local time", in opposition to the "general time" t because it depends on the spatial coordinate x, whereas t does not. This *local time* was (and remained) for him a mathematical device to perform computations in the moving frame. There was still only one true time, the absolute time t.

In 1904, in the lecture [P9], Poincaré discusses the local time (an "ingenious idea"). He gives it a physical meaning by prescribing a rule to synchronize clocks, and states a relativity principle, but justified, or explained, by the contraction hypothesis. He also speculates on a new mechanics, in which no speed could exceed c.

In [L5], Lorentz now modifies (2) and introduces the change of coordinates

$$(3) \qquad x' = (x - vt)(1 - v^2/c^2)^{-1/2}, \qquad t' = (t - vx/c^2)(1 - v^2/c^2)^{-1/2}$$

$$(4) \qquad y' = y, \qquad z' = z.$$

and envisages a formulation of physics in which all speeds would be at most c. The contraction has to have a dynamical origin, and is later also called the hypothesis of the deformable electron: electrons were viewed as the ultimate constituents of matter, balls when at rest with respect to the aether, but becoming ellipsoids flattened in the direction of a movement with respect to the aether, whence the contraction.

In June 1905, Poincaré published a *Comptes Rendus* Note [P11] which announced a paper submitted on July 5$^{\text{th}}$ and published a year later [P12]: "Sur la dynamique de l'électron".

He introduces the linear change of coordinates of [L5] (cf. (3), (4) above), which he calls a *Lorentz transformation.* He also defines the Lorentz group and proves the invariance of the equations of electromagnetism with respect to Lorentz transformations. Invariance with respect to the latter is the principle of relativity. However, this principle of relativity needs the contraction: the speed of light is c in the aether; it appears to be so to an observer in uniform motion with respect to the aether because of the contraction. He interprets a Lorentz transformation as a rotation in 4-space, with respect to the Lorentz metric, thus initiating the 4-dimensional formalism later developed by Minkowski [M2]. He also deduces the relativistic compositions of (uniform) velocities. Since the principle of relativity is justified by the contraction, he wants to give a dynamical explanation for the latter. He develops a theory of the forces which transform the electron, from a ball when at rest with respect to the aether, to an ellipsoid, flattened in the direction of the movement relative to the aether.

This is the last technical paper by Poincaré on the "new mechanics". There remain three general lectures, and an article [P13], also of a general nature, without formulas.

8. I now come to Einstein. Within three months in 1905 he submitted three papers: on the photoelectric effect, with the hypothesis of light quanta, on the determination of the Avogadro number (his Thesis) and on Brownian motion (submitted May 10). After that he was free to turn to the problems discussed above, which had occupied him off and on since his student days. As he told in a lecture in Kyoto, December 1922, published in English in [E6], Einstein went to his friend and colleague at the Patent Office Michele Besso, saying he wanted to have a battle of ideas about a problem he had been struggling with unsuccessfully. During that discussion, he suddenly saw the key to the problem: "*My solution was an analysis of the notion of time: it is not absolutely defined but only relatively and there is an inseparable connection between time and signal velocity.*"

He explained this point to a few friends, one of whom, J. Souter, also a colleague at the Patent Office, reminisced about it 50 years later [S4]. Einstein described how an observer on the famous clock tower in Berne and one on a tower in a nearby location, Muri, could synchronize clocks and define simultaneity. In the next five to six weeks he wrote his paper [E1], received June 30th and published three months later.

The paper states two postulates as a starting point:

1) The equations of physics are the same for two observers in relative uniform motion.
2) The speed of light is constant, independent of the relative motion of the source.

From this he deduces the Lorentz transformation relating time and space coordinates of two observers in relative uniform motion, which was new to him. The Lorentz-Fitzgerald contraction is thus given a *kinematical* explanation. Einstein proves that the Lorentz transformations form a group and then deduces the relativistic addition of speeds. No frame of reference is singled out and the aether is superfluous. He also draws further dynamical consequences which I need not go into here.

9. The paper did not create right away the splash he had expected or hoped for, according to his sister Maja, but, unknown to him for some time, it aroused much interest in some quarters: Max Planck lectured on it in his seminar (1905–6), and this impressed in particular Max von Laue, who visited Einstein in 1907. W. Wien asked a student, Laub, to report on it. Laub was later the first collaborator of Einstein, and had been very surprised to learn that Einstein was just an 8-hour a day employee in some office.

It should be added that in those first years, there was no clear distinction for most physicists between the theories of Lorentz and of Einstein [0]). They were viewed as prediction equivalent [1]). One spoke of Einstein's version, or generalization, of the Lorentz theory.

At that time however a big cloud was hanging over the theory, the experiments of W. Kaufmann [K1]. There was a competing theory, by

[0]) Not quite for all, though. Recently Professor E.L. Schucking kindly drew my attention to pp. 466–8 in the book *"Lehrbuch der Optik"*, second edition, Hirzel, Leipzig, 1906, by P. Drude where the author briefly describes the views of Lorentz [L5] and Einstein [E1], accepts the latter, and does not find the objection to it by Kaufmann (already announced in 1905) convincing.

[1]) As we know, they are not. Already in [E3] Einstein suggested an experiment to check his prediction about time dilation, which has no analog in the Lorentz theory, an experiment which could be carried out conclusively only in 1937 (Ives-Stilwell, see [M1], 7.2).

Abraham, in which electrons remained rigid balls (and even a third one, by A. H. Bucherer). All predicted an increase of the mass of an electron under a relative uniform motion, but the formulas were different. In fact there were transverse and longitudinal masses. For the former, Lorentz-Einstein predicted

$$m = m_0(\sqrt{1 - v^2/c^2})^{-1/2} \sim m_0(1 + \tfrac{1}{2}\, v^2/c^2) \tag{1}$$

and Abraham

$$m = m_0(1 + \tfrac{2}{5}\, v^2/c^2) \tag{2}$$

(see e.g. [M1], 1.10.2, 1.12.3). Kaufmann announced that his experiments confirmed (2). He was quite affirmative, claiming flatly that, unless he had made an error of principle, the Lorentz-Einstein theory was incompatible with his results. Max Planck, in a lengthy report [P4], was much more cautious and thought it was too early to draw definite conclusions. In fact, that same year, he published a first paper on relativity [P3], in which he stated that the principle of relativity, "introduced by Lorentz, and in still greater generality by Einstein", brings such enormous simplifications to electromagnetism that it is worth exploring its consequences in more than one way. He does not want to rule out at that stage that it will eventually be found compatible with experiments.

Lorentz and Poincaré were quite shaken by Kaufmann's results. The former wrote to the latter (March 8, 1908): "*Unfortunately, my hypothesis of the flattening of electrons is in contradiction with Kaufmann's results and I must abandon it. I am, therefore, at the end of my Latin.*" (Cf. [M1], pp. 336–7 for a facsimile of the letter). Poincaré wondered in [P13], section V, whether the relativity principle did not have as general a validity as had been expected. Einstein, on the other hand, was unmoved. He asked for further experiments. Though he points out that the theories of Abraham and Bucherer yield curves which are significantly closer to the one observed by Kaufmann than the curve provided by the relativity theory, he concludes that, in his opinion, *the theories of Abraham and Bucherer have a rather small probability of being correct, because their basic assumptions on the mass of the moving electron are not derived from theoretical systems which encompass a greater complex of phenomena* ([E4], III, § 10). In short, let us wait until the experiments confirm my theory. He did not have to wait long since in 1908, Bucherer announced the results of new experiments, which confirmed the Lorentz-Einstein theory versus Abraham's or his own, [B3].

10. This paved the way for H. Minkowski who, about three weeks later, gave his famous Cologne lecture [M2] with its ringing introduction:

> "The views of space and time I wish to lay before you have sprung from the soil of experimental physics; herein lies their strength. They are radical. Henceforth space by itself and time by itself are doomed to fade away into mere shadows and only a kind of union of the two will preserve an independent reality."

This is also the paper where Minkowski develops the four-dimensional formalism in which we all have learned special relativity, which Einstein viewed initially as superfluous erudition ("*überflüssige Gelehrsamkeit*"), but later praised highly, see [P1], p. 152.

Poincaré was also quick to point out, in a footnote added in proofs to the reproduction of [P13] in [P14], that Bucherer's experiments confirmed "*the views of Lorentz*".

11. How relieved Lorentz was can be seen from his book *"The Theory of electrons"* [L6]. Published in 1909, it was based on lectures given at Columbia University in 1906, but he had delayed publication in order to add recent developments. However, it was written while Kaufmann's results cast a doubt on his theory and he is very cautious, presenting his views as tentative, but in a last Note, added in 1908, he refers to Bucherer's results and concludes:

> "In all probability, the only objection against the hypothesis of the deformable electron and the principle of relativity has now been removed."

The last sections of this book contain a first discussion of Einstein's theory. Let S_0 be a system at rest with respect to the aether, A_0 an observer fixed in it, S a system moving with uniform speed v with respect to S_0 and A an observer in it. Lorentz first describes how, because of the contraction, the speed of light will also be c for A. Then he draws attention to a reciprocity pointed out by Einstein: if the observer A views S_0 as moving at speed $-v$, and performs the same computations, he will get back the coordinates in S_0. Then (§ 194) he concludes in a somewhat ambivalent manner which seems to me worth quoting extensively:

> It will be clear by what has been said that the impressions received by the two observers A_0 and A would be alike in all respects. It would be impossible to decide which of them moves or stands still with respect to the ether, and there would be no reason for preferring the times and lengths measured by the one to those determined by the other, nor for saying that either of them is in possession of the „true" times or the „true" lengths. This is a point which Einstein has laid particular stress on, in a theory in which he starts from what

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

12. In April 1909, Poincaré gave in Göttingen the "Wolfskehl-lectures". This was the first of an annual series of six lectures in mathematics, physics or mathematical physics. Poincaré gave the first five in German. At the beginning of the sixth he pointed out that he had been able to do so, though in poor German, because he had crutches, namely, the formulas. In the present lecture there will be none, so he will have to give it in French. It was on "*La nouvelle mécanique*" ("The new mechanics") [P15]. It is basically the Lorentz-Poincaré theory. There is an aether, an absolute time, but the contraction makes it impossible to detect a uniform relative motion with respect to it, whence a relativity principle.

A similar, in part identical lecture was given in Lille, in August 1909 [P16]. There is no mention of Einstein at all. However, it seems rather likely that Poincaré must have become aware of Einstein's theory from about that time on: a French translation of [M2] was published in 1909 and one of [E4] in 1910 [E5]. Besides, is it not likely that he received a copy of [L6]?

In 1910 M. Planck published eight lectures given at Columbia University in 1909 [P5]. After describing Einstein's ideas on space and time, he adds:

> It need scarcely be emphasized that this new conception of the idea of time makes the most serious demands upon the capacity of abstraction and the power of imagination of the physicist. It surpasses in boldness everything previously suggested in speculative natural phenomena and even in the philosophical theories of knowledge: non-euclidean geometry is child's play in comparison. And, moreover, the principle of relativity, unlike non-euclidean geometry, which only comes seriously into consideration in pure mathematics, undoubtedly possesses a real physical significance. The revolution introduced by this principle into the physical conceptions of the world is only to be compared in extent and depth with that brought about by the introduction of the Copernican system of the universe.

On October 13, 1910, Poincaré gave a lecture (in French) in Berlin. It is similar in substance, as regards our main topic of interest, to the sixth one in Göttingen, and would hardly be worth mentioning, were it not for what the first biographer of Einstein, the Polish journalist Alexander Moszkowski, wrote about it. The latter published in 1922 a book on Einstein, based largely on their conversations [M3]. At the beginning, he relates that he attended Poincaré's Berlin lecture. This occasion is the first time he heard the name of Einstein and he was so impressed by what he learned on him that he resolved he should get acquainted with him. This took place a few years later and led to the book.

However, the text of Poincaré's lecture is available in German translation [P17] and, again, there is no mention in it of Einstein or of Einstein's point of view. It is therefore reasonable to conjecture that someone brought up Einstein's relativity in the discussion following the lecture. After all, it was widely known and accepted in Germany by that time. If so, it would of course be interesting to know what was said. Maybe it is not overly optimistic to believe that Moszkowski provides at least a glimpse of it, since he writes later in his book (p. 231), "H. Poincaré had confessed, as late as 1910, that it caused him the greatest effort to find his way into Einstein's new mechanics", which, incidentally, would imply conclusively that he knew it.

13. A few days later (October 24–29), it was Lorentz's turn to give the Wolfskehl lectures in Göttingen. The second one was entitled "*The relativity principle of Einstein*". After expressing his pleasure in lecturing at a University where Minkowski had worked, he describes his point of view (aether, absolute time, local time, contraction) and Einstein's, in which all local times are on the same footing. He asserts that both are equivalent, and the choice between them

a question of taste. The note taker was Max Born, then a young Privatdozent, who also wrote up Lorentz's lectures [L7] from his own notes. In a comment added much later (1965) to a letter of Einstein of 1920, he wrote that, as a convinced Einstein disciple, he found that attitude "absurd and reactionary" ([B1], p. 72) [2]).

14. In 1911 appeared the first paper on Einstein's relativity by a French physicist, namely, Paul Langevin, professor at the Collège de France [L1] [3]). It gives a clear description of Einstein's theory, but is not really a complete endorsement. For instance, Langevin did not want to give up the aether:

> But it should not be concluded from that, as has sometimes been done prematurely, that the notion of aether should be abandoned, that aether does not exist, is unreachable through experiment. Only a uniform speed with respect to aether cannot be detected, but every change of speed, every acceleration has an absolute meaning.

---

[2]) Lorentz had the same point of view in [L8]. However in the second edition of [L6], which keeps most of the text of the first one, he adds a note to the page quoted in 11, beginning with:

> If I had to write the last chapter now, I should certainly have given a more prominent place to Einstein's theory of relativity (§ 189) by which the theory of electromagnetic phenomena in moving systems gains a simplicity that I had not been able to attain. The chief cause of my failure was my clinging to the idea that the variable t only can be considered as the true time and that my local time t' must be regarded as no more than an auxiliary mathematical quantity. In Einstein's theory, on the contrary, t' plays the same part as t; if we want to describe phenomena in terms of x', y', z', t' we must work with these variables exactly as we could do with x, y, z, t.

Still, while acknowledging here the superiority of Einstein's point of view, he maintained to the end of his life his preference for his own framework. In a letter written in 1954, quoted in [F1], p. 251, Einstein points out that Lorentz could not give up the aether, but adds that anyone who has lived through these times understands this.

This reluctance to abandon a familiar framework has often been compared to Einstein's own attitude, later, towards quantum mechanics.

[3]) The spreading of special relativity in France was apparently very slow, notwithstanding [E5] and the French translation of [M2], which do not seem to have made much impression, and mistrust towards it persisted for a long time in some quarters. The *Journal de Physique* regularly took up an issue of a foreign periodical, listed the titles of the papers, often with a capsule review. Einstein did not come out well. The summary of [E1] ((4), 5, 1906, 490) is hardly informative:

> The author proposes to establish the electrodynamics of moving bodies by taking into account absolute movement, only assuming light propagation in the vacuum with fixed speed, independent of the source.

In the summary of [P3], it is said that the principle of relativity, just introduced by Lorentz, implies extraordinary simplifications. The summary of [B3] speaks of a confirmation of Lorentz theory. A number of papers by Einstein, or Einstein-Laub, are quoted by title only.

In a lecture at the French Math. Society in 1912 (published in his *Collected Papers*, III$_1$, p. 179), "*Sur les groupes de transformations de contact et la cinématique nouvelle*", E. Cartan attributes the postulate of the constancy of the speed of light and the new kinematics to Lorentz. In his article: "La cinématique nouvelle et le groupe de Lorentz", published in the *Encyclopédie des Sciences Mathématiques*, 1915, he speaks of the "principle of relativity" and gives as references [P12], [M2], [L5] and a paper of F. Klein. Einstein is not mentioned at all. See also footnote [4]).

and in 1913 [L2], N° 13, after describing the contraction, he adds:

> The idea that such a contraction occurs solely because a body moves seemed at first peculiar. Then Einstein showed that it only corresponds to one of the consequences of the new notions of space and time imposed by the principle of relativity.
>
> From a completely different point of view, an important remark of Poincaré has thrown light on the very mechanism of that contraction.

In other words, the true explanation is the dynamical one of Lorentz-Poincaré, rather than those kinematical ideas about space and time. [It should be added, though, that later on in [L2], Langevin adopts Einstein's point of view completely.]

15. The first Solvay Congress took place in Bruxelles (Oct. 30–Nov. 3, 1911) and is probably the first and only time Poincaré and Einstein met, after which Einstein wrote to his friend H. Zangger: "Poincaré was simply quite antagonistic (with respect to Relativity) and showed little understanding for the situation, in spite of his sharp wit" (see [S1], p. 43). Also, the few recorded exchanges between them in discussions do not show much understanding. Those were on other matters, since the Congress had been convened not to discuss the "new mechanics", on which Einstein's theory was now widely accepted, but rather radiation and quanta [S0]. In a general report ([S0], 407–435), Einstein had also included his own light quanta hypothesis but with little success. The general consensus was apparently that Einstein was wrong on that. It took several years before his views were accepted, and eventually recognized by a Nobel prize (1922). Still he created a great impression, "the strongest apart maybe from Lorentz", wrote F. A. Lindemann, one of the three secretaries of the Congress; another secretary, Maurice de Broglie, reminisced later that Einstein and Poincaré were "in a class by themselves". Einstein was now famous, a recognized leading physicist.

At the time, Einstein was professor in Prague, but people at the E.T.H. (Swiss Federal Institute of Technology) in Zurich were trying to secure a position for him. Letters of recommendation were needed and Poincaré and Marie Curie were each asked to write one. The first sentence of Poincaré's letter is well known: "Mr. Einstein is one of the most original minds I know" and gives the impression that Poincaré now has a high regard for Einstein and that, maybe, Einstein's remarks to Zangger were based on some misunderstanding, but let us read the letter in full (see [P1], 170–1 and [F1], 83–4 for the original):

"Monsieur Einstein is one of the most original minds I have known; in spite of his youth he already occupies a very honorable position among the leading scholars of his time. We must especially admire in him the ease with which he adapts himself to new concepts and his ability to infer all the consequences from them. He does not remain attached to the classical principles and, faced with a physics problem, promptly envisages all possibilities. This is translated immediately in his mind into an anticipation of new phenomena, susceptible some day to experimental verification. I would not say that all his expectations will resist experimental check when such checks will become possible. Since he is probing in all directions, one should anticipate, on the contrary, that most of the roads he is following will lead to dead ends; but, at the same time, one must hope that one of the directions he has indicated will be a good one; and this suffices. This is how one should proceed. The role of mathematical physics is to state well the questions, but it is up to the experiments to answer.

The future will show more and more the value of M. Einstein, and the university which will succeed in attracting this young master is sure to draw much honor from it."

This implies first that Einstein, according to Poincaré, does not have yet any first rate achievement to his credit. It can only be hoped, even expected, that one will occur in the future, among many mistakes. Second, as someone who has had to read and evaluate thousands of letters of recommendation, (besides having written a fair number of them), I am rather wary of letters long on general remarks and short on specifics. Well, this one does not mention even one achievement of Einstein. Replace his name by, say, H. A. Lorentz or J. Doe, and you have a letter of recommendation for H. A. Lorentz or J. Doe. Altogether, not a very informed backing of the initial and final sentences.

16. Poincaré died unexpectedly in July 1912, after an operation which had appeared to be successful, so the lecture he gave in London in early May on "Space and Time" may well be his last word on this topic [P18]. The point of view is still that of his previous lectures,

"the principle of relativity, in its old form, had to be abandoned and to be replaced by the principle of relativity of Lorentz. It is the transformations of the "Lorentz group" which do not alter the differential equations of the dynamics. If you assume that the system is referred not to fixed axes, but to axes moving by a translation, you have to admit that all bodies undergo a deformation, that a sphere, for example, transforms into an ellipsoid, the small axis of which is parallel to the translation of the axes."

but then, there seems to be a shift in his discussion of the local time:

"It is necessary that the time be profoundly modified; here are two observers the first bound to the fixed axes, the second one to the moving ones, but both believing to be at rest. Not only such a figure, which the first one views as a sphere, will appear to the second one as an ellipsoid; but two events viewed by the first one as simultaneous, will not be so anymore for the second one."

"... in the new conception, space and time are no longer distinct entities which can be considered separately, but two parts of the same whole, two parts which are so closely knit that they cannot be easily separated."

which of course echoes Minkowski [M2], see 10 above. Does this mean that he is now leaning towards Einstein's relativity? Hardly, because he concludes:

> What shall be our position in view of these new conceptions? Shall we be obliged to modify our conclusions? Certainly not; we had adopted a convention because it seemed convenient and we had said that nothing could constrain us to abandon it. Today some physicists want to adopt a new convention. It is not that they are constrained to do so; they consider this new convention more convenient; that is all. And those who are not of this opinion can legitimately retain the old one in order not to disturb their old habits. I believe, just between us, that this is what they shall do for a long time to come.

Now who can "some physicists" be, who claim the necessity to adopt new conventions about space and time in physics, which seem inconvenient and unnecessary to Poincaré? To me, there is only one answer: Einstein and those who had accepted his theory [4]).

Let us now return to the second and third opinions quoted earlier $\big($2*b*), *c*)$\big)$.

The foregoing surely leads one in the direction of *b*), except however that in order to avoid taking a stand as to whether Poincaré did or did not understand something, I would rather say: "Poincaré knew Einstein's relativity

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[4]) There is hardly any doubt that Poincaré discussed these matters with E. Picard, and some of the latter's remarks in [P2] (freely translated) strike me as amplifications of Poincaré's opinion:

> We know the importance of the principle of relativity, the starting point of which is the impossibility, claimed on the strength of a few negative experiments, to detect a relative uniform motion of a system by means of experiments in optics or electricity made inside the system. Assuming on the other hand that the idea of Lorentz and his electromagnetic equations are unassailable, one has been led to view as necessary a change of our ideas on *space* and on *time*... The mathematicians interested by a group of transformations leaving invariant the quadratic form $x^2 + y^2 + z^2 - c^2t^2$ (c speed of light) have produced elegant dissertations on this topic and have no doubt contributed to the popularity of the principle of relativity. In other times, before rejecting the traditional ideas on space and time, one might have examined very critically those on the aether and the formulation of the equations of electromagnetism; but the appetite for the new knows no bounds today... Science surely has no dogmas, and it may happen that precise experiments constrain us one day to modify some ideas which are now common sense. But is it already the time to do so?
>
> Poincaré saw the danger of those fads and, in a lecture on the new mechanics, he implored professors not to discard the old and proven mechanics... He has lived long enough to see the main protagonists of the new ideas at least ruin partially their own work.

theory but did not accept it", a conclusion which in substance also agrees with those reached in [F2] or [M1].

17. However this does not prevent the friends quoted by S. Sternberg in 2*c*) from being right in many ways. Let us go back to the period 1898–1906 and see what Poincaré had anticipated: in 1898, the relativity of simultaneity, in 1900 he questions the existence of the aether, finds the hypotheses of Lorentz too special and asks for a principle implying the impossibility of detecting a relative uniform motion with respect to the aether. In 1904, he arrives at one, but tied up to the contraction and proposes a synchronization of "local times". In 1905 he defines the Lorentz group, shows it leaves invariant the equations of electromagnetism, computes the relativistic addition of uniform speeds and introduces a four-dimensional formalism. Moreover, his early writings on this topic surely had some influence on Einstein, beyond the non-necessity of Euclidean geometry. This incessant questioning of the framework and of various implicit assumptions was in tune with Einstein's own approach and the latter also acknowledged that Poincaré, Hume and Mach had influenced him. It certainly can be said that Poincaré had anticipated or independently developed "almost all" concepts of special relativity. But the missing part, even if quantitatively small, is the crux of the matter, what Lorentz called a starting point of fascinating boldness and Planck the boldest idea ever in the philosophy of knowledge. Poincaré viewed the relativity of simultaneity as a source of non-indispensable conventions, not as forcing a fundamental reorganization of our way of apprehending space and time, a point of view quite similar to that of Lorentz, in fact, as we saw[5]).

To conclude, I'll quote one more opinion, that of Louis de Broglie, the father of the wave formulation of quantum mechanics. It is in the introduction to Volume IX of Poincaré's *Collected Papers*, which contains Poincaré's papers on mathematical physics [P21]. Note that the intent of such an introduction is to present the work under consideration in as positive a light as possible. Indeed,

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[5]) As we saw, a fundamental difference between Einstein and Lorentz-Poincaré is the fact that the former provides a *kinematical* explanation for the contraction, while Lorentz and Poincaré look for a *dynamical* one. In that respect, I would like to quote from a recent letter of Jürg Fröhlich, commenting on an earlier draft of this paper: "It deserves to be emphasized that the special theory of relativity, although it brought about a revolution in our conception of space and time, is purely kinematical. A consistent formulation of *dynamical laws* for the new mechanics turned out to be *impossible*. A relativistic formulation of the dynamical laws governing the motion of gravitating bodies was only accomplished in the *general theory of relativity*. The insight that relativistic *dynamics* forces one to go beyond the special theory of relativity and the formulation of the general theory are undoubtedly due to Einstein."

after discussing [P12], de Broglie writes that Poincaré had accomplished there a work of capital importance, but then adds:

> ... but at the same time, maybe because he was more an analyst than a physicist, he did not perceive the general point of view, based on a very fine analysis of measures of distances and duration, which the young Albert Einstein had just discovered by a stroke of genius, and which led him to a complete transformation of our ideas on space and time. Poincaré has not taken this decisive step but he is, with Lorentz, the one who contributed most to make it possible [6]).

which seems to me to be a fair short summary of the situation [7]).

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[6]) In volume 11 of Poincaré's *Collected Papers*, second part, pp. 62–71, in a paper written on the occasion of Poincaré's 100th birthday, L. de Broglie returns to this question, and elaborates on the likely reason why Poincaré did not take that decisive step, saying in part

> "... il avait une attitude un peu sceptique vis-à-vis des théories physiques, considérant qu'il existe en général une infinité de points de vue différents, d'images variées, qui sont logiquement équivalents et entre lesquels le savant ne choisit que pour des raisons de commodité. Ce nominalisme semble lui avoir parfois fait méconnaître le fait que, parmi les théories logiquement possibles, il en est cependant qui sont plus près de la réalité physique, mieux adaptées en tout cas à l'intuition du physicien et par là plus aptes à seconder ses efforts. C'est pourquoi le jeune Albert EINSTEIN, âgé alors seulement de 25 ans et dont l'instruction mathématique était rudimentaire en comparaison de celle du profond et génial savant français, est cependant arrivé avant lui à la vue synthétique qui, utilisant et justifiant toutes les tentatives partielles de ses devanciers, a balayé d'un seul coup toutes les difficultés. Coup de maître d'un esprit vigoureux guidé par une intuition profonde des réalités physiques!"

[7]) This opinion is rather consonant, except for the inclusion of Lorentz, with one expressed at a meeting of the Société Française de Philosophie held in June 1922, of which A. Pais says on p. 168 of [P1] that it coincides with his own assessment:

> "The solution anticipated by Poincaré was given by Einstein in his memoir of 1905 on special relativity. He accomplished the revolution which Poincaré had foreseen at a moment when the development of physics seemed to lead to an impasse."

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