What if Einstein had not been there?
A Gedankenexperiment in Science History

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Abstract. Suppose Einstein had not existed. How, in the early twentieth century, might our understanding of space-time physics have developed? This paper proposes a reconstruction of history as it could have evolved, drawing on attested pre-Einsteinian works, and some little known post-Einsteinian ones, as well as introducing a few imaginary characters. The virtual rise of Minkowskian chronogeometry will be reviewed, focussing on:

– the discovery of the inertia of energy and the subsequent construction of velocity-dependent formulas for energy and momentum (Von Dida 1909, Hunepierre 1909),
– the application of the Erlanger program to the construction of possible space-time structures and classification of chronogeometrical groups, from von Ignatowsky (1910, 1911), Franck and Rothe (1911,1912) to more recent work by Blondeville and Prostov.

Some secondary sources will also be discussed (Zweistein, 1905). Finally, the relevance of such an approach, on educational, epistemological and cultural grounds, will be highlighted.

This paper is dedicated to the memory of Stephen J. Gould who so eloquently highlighted the contingency of natural history , notwithstanding its rationality .

1. Introduction

Gedankenexperiments consist in asking contrafactual questions - “What if . . .?”. It all started when Galileo wondered “What if air resistance did not exist?” and discovered the law of free fall [1] . This strategy was later fruitfully extended and popularised in physics, in particular by Einstein. I propose to use it here, not *in* physics, but *on* physics. Understanding the unfolding of any specific stretch of history greatly benefits from imagining how it could have developed otherwise, if such and such crucial circumstance had been different [2]. I will advance what I believe to be a plausible scenario of the history of space-time physics at the beginning of the twentieth century had Einstein not been there to make his justly celebrated 1905 breakthrough. I will rely heavily on the thorough study by Arthur Miller of pre-Einsteinian developments [3], while inventing a few characters and contributions (the latter usually corresponding to real later work). May I suggest that, when first perusing this paper, the reader should not consult the references, so as to make up his/her own mind regarding the veracity of the facts provided?

2. The Inertia of Energy

By the end of the XIXth century, the development of electromagnetic theory was leading physicists to a fully electromagnetic worldview, according to which all phenomena could ultimately be reduced to the interactions of electrical charges through the ether. All properties of matter would be explainable in electromagnetic terms. Mass itself should then have its origin in the electromagnetic field, as first suggested by Thomson (1881). Drawing on a hydrodynamical analogy, Thomson demonstrated that by electrically charging a conducting sphere, one would change its inertia. The physical reason was that the motion of body through the ether generated an electrical current that gave rise to a magnetic field acting back upon the charge; this self-induction effect would modify the inertia of the moving body (a primitive example of renormalization). Thomson found that the additional inertia was proportional to the energy of the electrostatic field, with a coefficient of proportionality $8/15c^2$. Heaviside (1889) corrected some errors of Thomson and found a coefficient $4/3c^2$ (a correct derivation of the exact factor $1/c^2$ would have to wait for Fermi, in 1922).

At all events, whatever the value of the coefficient, due to the velocity dependence of the field and its energy, the total kinetic energy of the body could no longer show the usual quadratic dependence on velocity. However, obtaining the exact formula proved rather difficult, as the calculations depended upon ad hoc assumptions concerning the cohesive forces holding the body against the internal repulsive Coulomb forces, as well as on its form and its possible deformations when set into motion. Thus ensued a decade of confusion, with various proposals as to the dependence of mass on the velocity, by Searle (1897), Wien (1900), Abraham (1903), Lorentz (1904), Langevin and Bucherer (1905); the trouble was compounded by the distinction between a “longitudinal mass” m_L and a “transversal mass” m_T. Here is a small sample of the variety of the results obtained:

$$Abraham(1903)\left\{\begin{array}{lcl} m_L &=& m_e\ \beta^{-3}[\beta(1-\beta^2)^{-1}-\tanh^{-1}\beta] \\ m_T &=& m_e\ \beta^{-3}[\frac{1}{2}(1+\beta^2)-\tanh^{-1}\beta-\frac{1}{2}\beta] \end{array}\right. . \quad (1)$$

$$Lorentz(1903)\left\{\begin{array}{lcl} m_L &=& m_e\ \frac{2}{3}(1-\beta^2)^{-3/2} \\ m_T &=& m_e\ \frac{2}{3}(1-\beta^2)^{-1/2} \end{array}\right. \quad (2)$$

$$Langevin-Bucherer(1904)\left\{\begin{array}{lcl} m_L &=& m_e\ \frac{2}{3}(1-\frac{1}{3}\beta^2)(1-\beta^2)^{-4/3} \\ m_T &=& m_e\ \frac{2}{3}(1-\beta^2)^{-1/3} \end{array}\right. \quad (3)$$

(with the definitions $\beta = v/c$ and $m_e = e^2/Rc^2$ in the usual notations, R being the radius of the spherical charged body).

Meanwhile, Kaufmann’s experiments (1902-1903) confirmed the reality of the variation of mass with velocity. However the confusion was exacerbated by the question of the agreement betwen the theoretical and the experimental results, the precision of which was clearly overrated at the time (Kaufmann claimed a 1% precision for $0.6 < \beta < 0.9$).

Many discussions on the nature and significance of this velocity dependence took place between Abraham, Lorentz, Kaufmann, Planck and Poincaré, who had written as early as 1900 that “Electromagnetic energy can be viewed as a fluid with inertia”. Finally, Planck, at a famous Köln conference (1908) concluded to a general “law of inertia of energy” by showing that the flow of any sort of energy, whether thermal, chemical, elastic, gravitational, etc., could be associated with a momentum density. At the same period, in one of the first important American contributions, Lewis and Tolman (1909) stated that the variation of mass with velocity transcended electromagnetic theory, and must be a universal feature, independent of the electric charge of the body [4]. Then, by the end of 1909, came the breakthrough. Von Dida, a student of Planck, asked himself what could be the simplest and most natural form for the inertia r of a body as a function of its velocity [5]. Here is how he proceeded, starting from first principles. Von Dida first defined the (possibly variable) inertia N of a body as the coefficient of the velocity in the expression of the momentum:

$$p = Nv. \quad (4)$$

He then *assumed* the inertia of energy in its most elementary form, asking that any variation in the energy E of the body would entail a proportional variation of its inertia N:

$$dN = \chi dE \tag{5}$$

where χ should be a universal constant. Finally, by recalling Hamilton's first equation,

$$v = \frac{dE}{dp}, \tag{6}$$

he had three relationships between the four magnitudes v, p, N, E, enabling him to express for example the last three of them in terms of the first one. One now computes quite simply $dN = \chi v dp = \chi v(dNv + Ndv)$, hence

$$\frac{dN}{N} = \frac{\chi v dv}{1 - \chi v^2}, \tag{7}$$

so that

$$N = \frac{N_0}{\sqrt{1 - \chi v^2}}. \tag{8}$$

Of course, considering the low-velocity limit leads one to identify N_0 with the mass m of the body. Since the constant χ has the dimensions of the inverse-square of a velocity, it is natural to set $\chi = c^{-2}$, defining a *universal* constant c with the dimension of a velocity, acting as a limit velocity for any massive body. Note that c is not necessarily the velocity of any specific physical agent, although of course it was ultimately to be identified with the velocity of electromagnetic waves. Von Dida finally obtained the following expressions for the dynamical properties of a body :

$$N = \frac{m}{\sqrt{1 - v^2/c^2}}, \;\; p = \frac{mv}{\sqrt{1 - v^2/c^2}}, \;\; E_{kin} = \frac{mc^2}{\sqrt{1 - v^2/c^2}} - mc^2. \tag{9}$$

(the last expression results from the fact that, because of (3), $E = \chi^{-1}N + cst$, and the requirement that for the purely kinetic energy, one should have $E_{kin}(v)|_{v=0} = 0$).

A puzzling consequence of these formulas was that they put into question the very notion of velocity. Indeed, since the momentum is no longer proportional to the velocity, the conservation of the total momentum becomes inconsistent with the usual law of addition of velocities. But the trouble with velocity was still to deepen, as we are going to see.

3. The New Conservation Laws

At the very same time, a young Swiss physicist, Albert Hunepierre, hit on a different but related approach. Attending the 1908 Köln conference already alluded to, he was stimulated by conversations with Paul Langevin on the nature of conservation laws, and by a comment of Minkowski at the end of Planck's lecture: "In my view, the law of momentum is obtained from the energy law; namely, in Lorentz's theory, the energy law depends on the reference system. We write the energy law for every possible reference system, so that we have many equations and in those are contained the law of momentum" [6]. Hunepierre's idea was to exploit this clue in order to characterize the functions $E(u)$ and $p(u)$ giving the energy and momentum of a body in terms of its velocity u (the change in notation with respect to the preceding section is purposeful). In fact, he simply revived, in very general terms, considerations going back to Huyghens. For the sake of simplicity, let us confine ourselves to the one-dimensional case, so that E and p are an even and an odd function respectively [7]. Consider a system of interacting particles, with velocities u_k $(k = 1, 2, \ldots)$; it is ruled by the conservation laws for the total energy E_{tot} and total momentum p_{tot}:

$$\left\{ \begin{array}{lcl} E_{tot} & = & \sum_k E(u_k) = Cst \\ p_{tot} & = & \sum_k p(u_k) = Cst \end{array} \right. \tag{10}$$

expressed in a particular inertial reference frame. Let us now describe the system in another inertial reference frame, moving with respect to the first one with velocity U. The crux of the argument is to *assume the additive law of combination for velocities*, so that the particles now have velocities

$$u_k^{'} = u_k + U \;\; (k = 1, 2, \ldots). \tag{11}$$

The conservation laws in this new reference frame then read

$$\left\{ \begin{array}{lcl} E'_{tot} & = & \sum_k E(u_k + U) = Cst' \\ p'_{tot} & = & \sum_k p(u_k + U) = Cst'. \end{array} \right. \tag{12}$$

If we now require the two conservation laws to hold in all equivalent inertial frames, we see, by developing the expressions (10) in power series of U, that not only are the total energy and momentum E and p conserved, but also all the quantities formed by adding up the successive derivatives of the individual energies and momenta,

$$\sum_k \frac{dE}{du}(u_k),\; \sum_k \frac{d^2E}{du^2}(u_k), \ldots, \sum_k \frac{dp}{du}(u_k),\; \sum_k \frac{d^2p}{du^2}(u_k) \ldots \tag{13}$$

But we cannot have more than two independent conservation laws; otherwise the collision process would be overdetermined (as the case of two particle makes clear). The "new" conservation laws must therefore be offshoots of the "old" ones. Since the individual velocities are arbitrary, the derivatives of the energy and momentum functions must depend on the energy and momentum for each particle separately. And the additivity of the conservation laws requires this functional dependence to be linear. Finally, the even and odd nature respectively of E and p severely restricts the possibilities. For the first derivatives, indeed, the most general expressions are:

$$\left\{ \begin{array}{l} \dfrac{dE}{du} = p \\[2ex] \dfrac{dp}{du} = m + \chi E \end{array} \right. \tag{14}$$

where we have chosen a coefficient unity in the first equation according to the usual dimensional convention, and where the constant term in the right-hand-side of the second equation is identified with the mass of the particle in order to recover the standard low-velocity Newtonian expressions. By so doing, we automatically take E to be the purely kinetic energy of the particle, since, in the case $\chi = 0$, one recovers from (11) the expressions $p = mu$ and $E = mu^2/2$. Of course, we know, with hindsight, that the coefficient χ is the same as in the previous Section, and we put $\chi = c^{-2}$. The solution of the system (11) of differential equations, given the even and odd nature of E and p respectively, is unique:

$$\left\{ \begin{array}{ll} E_{kin} & = mc^2[\cosh(u/c) - 1] \\ p & = mc \sinh(u/c) \end{array} \right. \tag{15}$$

Obviously, the comparison of the expressions (12) with those given by (7) compounded the problem concerning the very notion of velocity, to which we will return anon. But

Hunepierre's major accomplishment, in a second paper of the same year 1909, was to realize that the new form (12) of the conservation laws was inconsistent with the hitherto unquestioned additivity of mass. It suffices to consider two particles, with the same mass m. In the reference frame of their center of inertia, they have respective velocities $(u, -u)$; the internal energy of the system, that is, the kinetic energy of the particles in this very reference frame thus is $E_0 = 2mc^2\ [cosh(u/c) - 1]$. In another reference frame, moving with respect to the first one with velocity U, the particles have velocities $(u + U, -u + U)$, so that the total momentum is $p = mc \sinh(u/c + U/c) + mc \sinh(-u/c + U/c) = 2mc \cosh(u/c) \sinh(U/c)$. However, on the other hand, the momentum of the system, moving with global velocity U, should read, according to the second of equations (12), $p = Mc \sinh(U/c)$, where M is its (total) mass. We are then led to recognize that the total mass is given by $M = 2m \cosh(u/c) = 2m + E_0/c^2$. Mass is no longer additive, and any change in the internal energy of the system entails a proportional change in its mass, $\Delta m = \Delta E_0/c^2$. Hunepierre then proposed to normalize the (hitherto arbitrary) zero of energies so as to include, for any M, its "mass energy" Mc^2 in its internal energy, which enabled to write in full generality the now famous Hunepierre's equation

$$M = E_0/c^2 \tag{16}$$

which he jokingly transcribed as

$$\text{Mass} \ \propto \ \text{Innergy}. \tag{17}$$

4. The New Space-Time

By that time, that is, around 1910, it was becoming obvious that the changes in dynamics (i.e. in the expressions for the energy and momentum) required a parallel modification in kinematics (i.e. in the structure of space-time), as illustrated by the debate around the notion of velocity.

It had been known for several years that Maxwell equations were invariant under a particular set of transformations, as noted by Lorentz (1904) - who left his name to them - , and by Poincaré (1905), who emphasized the group structure of this set. Minkowski had given in 1908 a neat mathematical description of this pseudo-euclidean group. Still, this invariance property was thought to be specific to electromagnetic theory, and its interpretation was unclear. Poincaré, for instance, up to the end of his life (1912), clung to an epistemology which required Lorentz's ether, and hardly allowed for a physical significance of the Lorentz transformations [8]. However, Sommerfeld, who had been a student of Felix Klein, was familiar with the Erlanger program and the geometrical interpretation of transformation groups [9]. He thus suggested to investigate the possible transformation groups in space-time, depending only on general and abstract requirements, irrespective of the specific physical phenomena (electromagnetic, gravitational or others) taking place on a supposedly universal spatio-temporal stage.

In the early 1910s, several independent works appeared resolving this question - with, it must be said, rather awkward approaches and complicated calculations, due to a lack of familiarity with group theory (and with infinitesimal Lie algebraic methods in particular). Various contributions by von Ignatowsky (1910, 1911), Franck and Rothe (1911,1912), van Rijn (1912), Hahn (1913), etc., finally converged towards the following conclusion [10]. Under the very general assumptions of:

- homogeneity of space-time
- isotropy of space

- existence of causal relationships,

the Lorentz-type transformations (written here in the one-dimensional case)

$$\left\{\begin{array}{ll} x' & = \dfrac{x - vt}{\sqrt{1-\chi v^2}} \\ \\ t' & = \dfrac{t - \chi v x}{\sqrt{1-\chi v^2}} \end{array}\right. \tag{18}$$

with a constant $\chi \geq 0$, are the *only* possible ones. Of course, if these transformations are to be universal, and if Maxwell equations are indeed correct, one has again to identify $\chi = c^{-2}$. Under the influence of the Kleinian point of view, the theory of space-time soon became known under the very apt name of "chronogeometry", or, more precisely, "Minkowskian chronogeometry", when necessary to distinguish it from the classical conception of space-time or "Galilean chronogeometry" (which, it must be stressed, indeed is a space-with-time theory, as it already mixes space and time - or, rather, time with space, if not space with time as in the true Minkowskian case). Once this generalized geometrical perspective was adopted, it was a simple matter to understand the puzzling phenomena of so-called "length contraction" and "time dilatation" as *parallax* effects in space-time, quite analogous to the customary parallax effects in space. Langevin, in particular, was especially keen on this interpretation [11] .

5. Speeds

The confusion around the notion of speed was due to the existence of two equally natural derivations for the energy and momentum, yielding the two expressions stemming from (7) and (12) respectively, which we rewrite in the natural system of units already advocated by Planck, where $c = 1$:

$$\left\{\begin{array}{ll} E & = \dfrac{m}{\sqrt{1-v^2}} = m \cosh u \\ \\ p & = \dfrac{mv}{\sqrt{1-v^2}} = m \sinh u \end{array}\right. \tag{19}$$

It was immediately clear that the two competing notions were related through

$$v = \tanh u, \tag{20}$$

but it was generally held that the 'true' velocity was v , since, after all it entered Hamilton's equation and was consistent with the old Galilean definition of velocity as the ratio of the span of space traveled by a mobile object to the time taken, i.e. $v = \Delta x/\Delta t$. While most physicists maintained that u was merely a formal quantity, a purely mathematical device, others insisted that the limitation $v \leq c$ and the non-additivity of the velocity parameter v required u to be given a primary physical meaning - as well as a name of its own, for which the term 'rapidity' was chosen [12] . Agreement was finally reached that a single notion in Galilean chronogeometry split into two different albeit related ones in Minkowskian chronogeometry. The situation, it was understood, is quite similar to that in ordinary geometry where slope, when small, is characterized by a single parameter, while one has to distinguish angle and tangent for larger values. Finally, rapidity was given a direct physical meaning by Blondeville who discussed the operational definitions of speed; he stressed that an observer, if isolated within his own reference frame without any external clue, cannot measure the distance he covers (so that the Galilean definition is useless to him). However, he can measure his (proper) acceleration as a function of his (proper) time, and integrate it, which results, as a simple calculation shows, in the increment of his rapidity:

$$\int_A^B dt_0 \gamma_0(t_0) = u_B - u_A \qquad (21)$$

The most natural *intrinsic* measurement of speed changes thus yields directly the variation of rapidity [13]. Once the status of rapidity as a *bona fide* physical magnitude was established, it was used by Blondeville and Prostov for a much simpler derivation of the Lorentz group as the only possible chronogeometrical group (along with its singular Galilean limit) by relying on the elementary but deep lemma that any one-parameter continuous group can be additively parametrized [14].

6. Another Route to Spacetime?

No account of the birth of modern chronogeometry would be complete without mentioning a recent historical discovery, which shows that another path could have been taken. Recently, in the archives of the famous journal *Annalen der Physik*, the manuscript was uncovered of a paper submitted in 1905 by a young physicist named Albrecht Zweistein. In this paper, which was rejected for reasons we will shortly discuss, Zweistein studied the electrodynamics of moving bodies. While sticking to the customary (at the time) electromagnetic worldview of Lorentz and Poincaré, he consistently pursued an earlier argument by Cohn (1901,1902), who, following a Machian train of thought stressing 'scientific economy', had shown the superfluousness of the ether. Zweistein then completely did away with the ether and its privileged frame [15]. Instead, he proposed an interpretation of the Lorentz invariance of Maxwell equations based on two principles:

1) the classical equivalence of reference frames in uniform motion (which he called the "Principle of relativity", borrowing the terminology from Poincaré),
2) the apparently weird idea that the velocity of light was the same in all these reference frame - his 'second postulate'.

Zweistein then proceeded to analyse time and space measurements, through detailed gedankenexperiments using the exchange of light signals to synchronize distant clocks. He was able to show that the time and space coordinates defined by such operational procedures, when compared in two equivalent reference frames, were linked by the standard Lorentz transformations.

Although this was quite a clever paper, it raised strong objections and was refused for publication; it must be said that the author held no academic position, which certainly did not help. A first referee sternly condemned the dismissal of the ether as "unphysical", in line with Poincaré's position. A second one pointed to the *ad hoc* and fragile nature of the operational point of view taken by Zweistein: what, he asked, if non-electromagnetic signals, of a hitherto unknown nature, were discovered and used? Would we not have to face the possibility of a different description of space-time for these new phenomena, negating the idea of a universal spatio-temporal arena? And what if Maxwell's equations were only approximate? [In modern terms, think of the situation if the photon finally had a nonzero mass, however small; light would not travel with the invariant velocity, and any derivation of chronogeometry based on the exchange of light signals would become invalid.] The paper was resubmitted by the author a few years later, claiming priority after the development of chronogeometry as sketched above. It was then rebutted by Sommerfeld on more epistemological grounds. Here is what he wrote: "[The theory of space-time] is an *Invariantentheorie* of the Lorentz group. The name 'relativity theory' is an unfortunate choice: the relativity of space and time is not the essential thing, which is the independence of the laws of nature from the point of view of the observer" [16].

One may only wonder what the dominant interpretation and terminology of our theory of space-time would have been if Zweistein's paper had been published as early as 1905 . . .

7. Conclusions

I will now abandon fiction and propose some conclusions.

But first, in order to ward off any accusation of blasphemy or lese-majesty, let me stress that Einstein himself was quite aware that his path towards what we now know as 'special relativity theory' was not the only possible one, and that others could have originated the new space-time physics. Here is, for instance, what he wrote in his obituary to Langevin (1947): "It appears to me as a foregone conclusion that he would have developed the special relativity theory, had not that been done elsewhere" [17].

I leave it to specialists in science history to assess the degree of plausibility of the alternate account narrated here. But what is the purpose of this fictitious reconstruction? In fact, I see three areas of relevance for such considerations:

a) *educational*. The presentation of scientific notions as they unfolded historically is not the only one, nor even the best one. Alternative arguments and novel derivations should be pursued and developed, not necessarily to replace, but at least to supplement the standard ones.

b) *epistemological*. As I have tried to show, if Einstein had not existed, we still would have 'his' theory - or would it really be the same? Apart from a few details perhaps, the formalism, that is, the notations and equations, would be very similar to ours today. But the language and, underneath, the words and the ideas we would use could be rather different. Many familiar terms, beginning with 'relativity' itself, might be absent from our vocabulary. My point here is to stress the polysemy of science, even for such a highly formalized science as physics. We need to actualise the variety of potential meanings behind our symbols - would it be only to keep open impredictable future paths of development.

c) *cultural*. Science is too often perceived by lay people as a mechanistic and inhuman endeavour. We should try to offer a less absolute and less deterministic view of its development, so that science clearly appears for what it is, that is, a human venture, the solidity of which is no doubt based on its collective structure, where the role of the individuals nevertheless cannot be neglected. Think for instance of the whole mythology of the twentieth century without the figure of Einstein (it is on purpose that in the above fiction the emergence of chronogeometry has been attributed to a largely collective effort).

In more general terms, my aim here was to advocate a view of the history of science which gives due credit to the notion of contingency. This is the reason of my dedicating this paper to the memory of Stephen J. Gould who passed a few weeks ago. In his work, and especially in his book *Full House*, he stressed the importance of the notion of contingency for the history of life [18] . If you 'rewind the film', as he was fond of saying, and let it unfold again starting a billion years ago, there is no reason the story would follow the same course. Chance events, it is now accepted, have played a crucial role in the history of life, events such as the crashing of a meteorite in Yucatan 65 million years ago, which tolled the knell of dinosaurs and paved the way for mammals. Of course, as Gould repeatedly emphasised, this view is not akin to an irrationalistic one: the general features of history, would it be natural, social or scientific, must and can be

explained. But historical phenomena are so complex as to be extremely sensitive to a host of apparently secondary conditions. Of course, this should not come as a surprise to us physicists who today are familiar with deterministic chaos, sensitivity to initial conditions, etc.
Might it not be claimed, then, that Einstein was a Loren(t)z butterfly?

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References

[1] See J.-M. Lévy-Leblond, "Science's fiction", *Nature* **413**, 573 (2001).

[2] As a remarkable example, think of the 1972 novel by Philip K. Dick, *The Man in High Castle* (Vintage Books, 1992), describing America in 1962, twenty years after it has lost the war and is occupied by Nazi Germany and imperial Japan. On a more scientific side, a recent paper by E. B. Davies uses the fictional device of an imaginary Earth permanently covered with a cloud hiding the sky to investigate the role of astronomy (non-existent on this world) in the history of science (arXiv: physics/0207043).

[3] Arthur I. Miller, *Albert Einstein's Special History of Relativity, Emergence (1905) and Early Interpretation (1905-1911)*, Addison-Wesley, 1981.

[4] All of the preceding is historically correct, as can be checked from the seminal book by Arthur I. Miller (ref. 3). In particular, for the discussions of the mass variation with velocity within electromagnetic theory, see Sections 1.8 to 1.14. For Planck's law of inertia of energy, see Sections 12.5.7. For Tolman and Lewis contribution, see Section 12.2, note 4.

[5] Here we depart from history as it unfolded. The following derivation in fact is due to W.C. Davidon, *Foundations of Physics* **5**,525 (1975), who also give an account of the historical developments sketched above.

[6] While Hunepierre's character is a fiction, the remark by Minkowski is true to the facts; see A. Miller, ref.3, p. 367.

[7] The following derivation has been proposed by J.-M. Lévy-Leblond, "What is So Special in Relativity?", in "*Group-Theoretical Methods in Physics*", A. Janner & al., Lecture Notes in Physics n^{0}50, Springer-Verlag, 1976.

[8] See, for instance, the comments by O. Darrigol in his introduction to Albert Einstein, *Œuvres choisies*, vol.2 (*Relativités I*), Seuil-CNRS, 1993,P.23.

[9] See A. Miller, ref.3, p. 181 (note 41).

[10] None of these names or contributions are invented. Here are the exact references: W.I. Ignatowsky, *Archiv der Math. und Phys. III*, **17**, 1 (1910), *Phys. Z.* **11**, 972 (1910), &**12**, 776 & 779 (1911), P. Franck & H. Rothe, *Ann.Phys.* **34**, 825 (1911), *Phys.Z* **13**, 750 (1912), A.C. van Rijn van Alkemade,*Ann.Phys.* **38**, 1033 (1912), E. Hahn, *Archiv Math.Phys.* III, **21**,1 (1913). These papers were immediately neglected and forgotten and their conclusions soon rediscovered by L. A. Pars, *Phil. Mag.* **42**, 249 (1921), E. Esclangon, *C.R.Acad. Sci.* **202**, 1492 (1936), E. Le Roy, *C.R. Acad Sci.* **202**, 794 (1936), V. Lalan, *C.R. Acad. Sci.* **203**, 794 (1936)& *Bull. Sci. Math.France*, **65**,83 (1937), which were no more successful. A later independent proof, using simpler and more modern arguments, was proposed by J.-M. Lévy-Leblond,*Am. J. Phys.* **44**, 271 (1976). A general review of this long but occult line of arguments has been made by J.-P. Lecardonnel, in his Thesis, University Paris VI (Pierre et Marie Curie), 1979, and in a paper, *Bull. U. Phys.*, **615**,1171 (1979).

[11] Although Langevin did not explicitly use the idea of parallax, he nevertheless put forward a very modern view of space-time; see P. Langevin, "L'évolution de l'espace et du temps", *Scientia* **10**, (1911).

[12] Here is a serious historical question: when and by whom was the term 'rapidity' used for the first time-certainly rather late in the historical development? I would indulge in conjecturing that J.A. Wheeler could be at the origin of the word in the fifties.

[13] While this simple fact may have been known to quite a number of people, I do not know of any published mention before J.-M. Lévy-Leblond, *Am. J. Phys.* **48**, 345 (1980).

[14] It remains surprising that such a simple mathematical result was recognized so lately as a solid founding stone for chronogeometry. See J.-M. Lévy-Leblond & J.-P. Provost, *Am. J. Phys.* **47**, 1045 (1979). It may also be mentioned here that a still more general derivation of the (very few) possible chronogeometries, relaxing the linearity of the action of the group on space-time, has been given by H. Bacry & J.-M. Lévy-Leblond, *J. Math. Phys.* **9**, 1605 (1968).

[15] The important contributions of Emil Cohn and his epistemological stand are barely known today. They

probably exerted a definite influence on Einstein who thought highly of Cohn (as did the usually overcritical Pauli); see A. Miller, ref.3, p.181-182, note 42.

[16] This statement(as well as other similar ones) by Sommerfeld is well documented although it came historically much later, that is, in a 1948 review paper on philosophy and physics after 1900; see A. Miller, ref. 3, p.181, note 41.

[17] See A. Miller, ref. 3, p. 388.

[18] Stephen J. Gould, *Full House*, Harmony Books (1966).