Michelson, FitzGerald and Lorentz: the origins of relativity revisited

Harvey R Brown*

February 10, 2003

Abstract

It is argued that an unheralded moment marking the beginnings of relativity theory occurred in 1889, when G. F. FitzGerald, no doubt with the puzzling 1887 Michelson-Morley experiment fresh in mind, wrote to Heaviside about the possible effects of motion on inter-molecular forces in bodies. Emphasis is placed on the difference between FitzGerald's and Lorentz's independent justification of the shape distortion effect involved. Finally, the importance of the their 'constructive' approach to kinematics—stripped of any commitment to the physicality of the ether—will be defended, in the spirit of Pauli, Swann and Bell.

1 Introduction

The claim that a particular theory in science had its true origins at this or that moment of time, in the emergence of this or that fundamental insight, is bound to be contentious. But there are developments, sometimes in the unpublished writings of a key figure, which deserve more recognition and fanfare in the literature as being seminal moments in the path to a given theory. In my opinion such a moment occurred in early 1889, when George Francis FitzGerald, Professor of Natural and Experimental Philosophy at Trinity College Dublin, wrote a letter to the remarkable English auto-didact, Oliver Heaviside, concerning a result the latter had just obtained in the field of Maxwellian electrodynamics. Heaviside had shown that the electric field surrounding a spherical distribution of charge should cease to have spherical symmetry once the charge is in motion relative to the ether. In this letter, FitzGerald asked whether Heaviside's distortion result—which was soon to be corroborated by J. J. Thompson—might be applied to a theory of intermolecular forces. Some months later, this idea would be exploited in a letter by FitzGerald published in *Science*, concerning the baffling outcome of the 1887 ether-wind experiment of Michelson and Morley. FitzGerald's letter is today quite famous, but it was virtually unknown until 1967. It is famous now because the central idea in it corresponds to what came to be known as the FitzGerald-Lorentz contraction hypothesis, *or rather to a precursor of it.* This hypothesis is a cornerstone of the 'kinematic' component of the

---

*Faculty of Philosophy, University of Oxford, 10 Merton Street, Oxford OX2 7TL, U.K.; *harvey.brown@philosophy.ox.ac.uk.*

special theory of relativity, first put into a satisfactory systematic form by Einstein in 1905. But the FitzGerald-Lorentz explanation of the Michelson-Morley null result, known early on through the writings of Lodge, Lorentz and Larmor, as well as FitzGerald's relatively timid proposals to students and colleagues, was widely accepted as correct before 1905—in fact by the time of FitzGerald's premature death in 1901.

Following Einstein's brilliant 1905 work on the electrodynamics of moving bodies, and its geometrization by Minkowski which proved to be so important for the development of Einstein's general theory of relativity, it became standard to view the FitzGerald-Lorentz hypothesis as the right idea based on the wrong reasoning. I strongly doubt that this standard view is correct, and suspect that posterity will look kindly on the merits of the pre-Einsteinian, 'constructive' reasoning of FitzGerald, if not Lorentz. After all, even Einstein came to see the limitations of his own approach based on the methodology of 'principle theories'. I need to emphasise from the outset, however, that I do not subscribe to the existence of the ether, nor recommend the use to which the notion is put in the writings of our two protagonists (which was very little). The merits of their approach have, as J. S. Bell stressed some years ago, a basis whose appreciation requires no commitment to the physicality of the ether.

There is, nonetheless, a subtle difference between the thinking of FitzGerald and that of Lorentz prior to 1905 that is of interest. What Bell called the "Lorentzian pedagogy", and bravely defended, has as a matter of historical fact more to do with FitzGerald than Lorentz. Furthermore, the significance of Bell's work for general relativity, particularly in relation to the light it casts on the so-called clock hypothesis, has still not been fully appreciated.

2 FitzGerald, Michelson and Heaviside

A spherical distribution of charge at rest with respect to the ether produces, according to both intuition and Maxwell's equations, an electric field whose surfaces of equipotential surrounding the charge are spherical. But what happens when the charge distribution is in uniform motion relative to the ether? Today, we ignore reference to the ether and simply exploit the Lorentz covariance of Maxwell's equations, and transform the stationary solution to one associated with a frame in relative uniform motion. This is not a new technique; remarkably, Larmor had already used it before the turn of the 19th century.

But in 1888, the covariance group of Maxwell's equations was yet to be discovered, let alone understood physically—the relativity principle not being thought to apply to electrodynamics—, and the problem of moving sources required some hard mathematics involved in solving Maxwell's equations. These equations were taken to hold only relative to the rest frame of the ether. Oliver Heaviside did the hard mathematics and published the solution: the electric field of the moving charge distribution undergoes a distortion, with the longitudinal components of the field being affected by the motion but the transverse ones not. Heaviside [1] predicted specifically an electric field of the following form:

$$\mathbf{E} = \left(q\mathbf{r}/r^2\right)\left(1 - v^2 sin^2\theta/c^2\right)^{-3/2} \tag{1}$$

where $\mathbf{E}$ is evaluated at a point with displacement $\mathbf{r}$ from the centre of the charge distribution and θ is the angle between $\mathbf{r}$ and the direction of motion. Some years later, through further analysis by J. J. Thompson and particularly G. Searle [2], it was realised that the surface of equipotential forms an ellipsoid—coming to be known as the Heaviside ellipsoid.

The timing of Heaviside's distortion result was propitious, appearing as it did in the confused aftermath of the 1887 Michelson-Morley (MM) expriment[1]. One of Heaviside's correspondents and supporters, G. F. FitzGerald[2], found like all competent ether theorists the null result of this fantastically sensitive experiment a mystery. Null results in earlier *first-order* ether wind experiments had all been explained in terms of the Fresnel drag coefficient, which would in 1892 receive an electrodynamical underpinning of sorts in the work of H. A. L. Lorentz. But by early 1889 no one had accounted for the absence of noticeable fringe shifts in the second-order MM experiment. How could the apparent isotropy of the two-way light speed inside the Michelson interferemoter be reconciled with the seeming fact that the laboratory was speeding through the ether? Why didn't the ether wind blowing through the laboratory manifest itself when the interferometer was rotated?

The conundrum of the MM null-result was surely in the back of FitzGerald's mind when he made an intriguing suggestion in a letter to Heaviside in January 1889. The suggestion was simply that a Heaviside distortion might be applied "to a theory of the forces between molecules" of a rigid body.[3]

FitzGerald had no more reason than anyone else to believe in 1889 that these intermolecular forces were electromagnetic in origin. No one knew. But if these forces too were rendered anisotropic by the mere motion of the molecules, which FitzGerald regarded as plausible in the light of Heaviside's work, then the shape of a rigid body would be altered as a consequence of the motion. This line of reasoning was briefly spelt out, but with no *explicit* reference to Heaviside's work, in a note [9] that FitzGerald published later in the year in the American journal *Science*[4]—the first correct insight into the mystery of the MM experiment when applied to the stone block on which the Michelson interferometer was mounted. But the note sank into oblivion; FitzGerald did not bother to confirm that it was published, and seems never to have referred to it, though he did promote his deformation idea in lectures, discussions and correspondence. His relief when he discovered that Lorentz was defending essentially the same idea was palpable in a good-humoured letter he wrote to the great Dutch physicist in 1894, which mentioned that he had been "rather laughed at for my view over here".

It should be noted that FitzGerald never seems to have used the words 'contraction' or 'shortening' in connection with the proposed motion-induced change of the body. The probable reason is that he did *not* have the purely longitudinal

---

[1] See Michelson and Morley [3]. Michelson's first attempt in 1881 to perform the experiment single-handledly during study leave in Germany had ended in failure. Funds he gained from Alexander Graham Bell allowed for the construction of a novel interferometer of unprecedented sensitivity, but an elementary error in Michelson's calculations meant that the device was not quite sensitive enough to do the job, as H. A. L. Lorentz was to point out shortly after the event. It is ironic that the corresponding calculation—for the passage time of the light in the transverse arm of the interferometer—given by Michelson and Morley is also incorrect, but now the error only affected third and higher order terms and thus was innocuous. See Brown [4] for further discussion and references. A useful account of Michelson's many achievements in physics is found in Bennett *et al.* [5].

[2] For good accounts of FitzGerald's character and life, see Lodge [6] and Coey [7].

[3] To my knowledge, the first historian to call attention to this letter was B. Hunt [8].

[4] See FitzGerald [9]. The note is reprinted in Brush [10] and most of it in Bell [11].

contraction, now ubiquitously associated with the 'FitzGerald-Lorentz hypothesis', in mind. It is straightforward to show—though not always appreciated—that the MM result does not demand it. Any deformation (including expansion) in which the ratio of the suitably defined transverse and longitudinal length change factors equals the Lorentz factor $\gamma = \left(1 - v^2\right)^{-1/2}$ will do, and there are good reasons to think that this is what FitzGerald meant, despite some claims to the contrary on the part of historians. It is certainly what Lorentz had in mind for several years after 1892, when he independently sought to account for the MM result by appeal to a change in the dimensions of rigid bodies when put into motion.[5]

3 Einstein

In his masterful review of relativity theory of 1921, the precocious Wolfgang Pauli was struck by the difference between Einstein's derivation and interpretation of the Lorentz transformations in his 1905 paper [12] and that of Lorentz in his theory of the electron. Einstein's discussion, noted Pauli, was in particular "free of any special assumptions about the constitution of matter"[6], in strong contrast with Lorentz's treatment. He went on to ask:

> Should one, then, completely abandon any attempt to explain the Lorentz contraction atomistically?

It may surprise some readers to learn that Pauli's answer was negative. Be that as it may, it is a question that deserves careful attention, and one that, if not haunted him, then certainly gave Einstein unease in the years that followed the full development of his theory of relativity.

Einstein eventually came to realize that the first, 'kinematic' section of his 1905 paper was problematic; that it effectively rested on a false dichotomy. What is kinematics? In the present context it is the universal behaviour of rods and clocks in motion, as determined by the inertial coordinate transformations. And what are rods and clocks, if not, in Einstein's own words, "moving atomic configurations"?[7] They are macroscopic objects made of micro-constituents—atoms and molecules—held together largely by electromagnetic forces. But it was the second, *dynamical* section of the 1905 paper that dealt with the covariant treatment of Maxwellian electrodynamics. Einstein came to see that the first section was not wholly independent of the second, and that the treatment of rods and clocks in the first section as primitive, or "self-sustained" entities was a "sin"[8]. The issue was essentially the same one that Pauli had stressed in 1921[9]:

> The contraction of a measuring rod is not an elementary but a very complicated process. It would not take place except for the covariance with respect to the Lorentz group of the basic equations of

---

[5] A detailed historical and conceptual analysis of the responses of both FitzGerald and Lorentz to the MM expriment is found in Brown [4], which also contains a discussion of common misunderstandings of the MM experiment itself.

[6] See Pauli [13], p. 15.

[7] See Einstein [14], p. 59.

[8] See Einstein [14], pp. 59, 61, and [15], pp. 236–237.

[9] See Pauli [13], p. 15.

> electron theory, as well as those laws, as yet unknown to us, which determine the cohesion of the electron itself.

Pauli is here putting his finger on two important points: that the distinction between kinematics and dynamics is not fundamental, and that to give a full treatment of the dynamics of length contraction was still beyond the resources available in 1921, let alone 1905. And this latter point was precisely the basis of the excuse Einstein later gave for his 'principle theory' approach—modelled on thermodynamics—in 1905 in establishing the Lorentz transformations.

The singular nature of Einstein's argumentation in the kinematical part of his paper, its limitations and the recognition of these limitations by Einstein himself, have been discussed in detail elsewhere[10]. The main lesson that emerges, as I see it, is that the special theory of relativity is incomplete without the assumption that the quantum theory of *each* of the fundamental non-gravitational interactions—and not just electrodynamics—is Lorentz-covariant. This lesson was anticipated as early as 1912 by W. Swann, and established in a number of papers culminating in 1941 in his [17]. It is consistent with the didactic approach to special relativity advocated by J. S. Bell in 1976 [18], to which we return shortly.

Swann's unsung achievement [17] was in effect to spell out in detail the meaning of Pauli's 1921 warning above. His incisive point was that the Lorentz covariance of Maxwellian electrodynamics, for example, has no clear connection with the claim that this theory satisfies the relativity principle, unless it can be established that the Lorentz transformations are more than just a formal change of variables and actually codify the behaviour of moving rods and clocks in motion. But this last assumption depends for its validity on our best theory of the micro-constitution of stable macroscopic objects. Or rather, it depends on a fragment of that quantum theory (for it could not be other than a quantum theory): that at the most fundamental level all the interactions involved in the composition of matter, whatever their nature, are Lorentz covariant. It must have been galling for Einstein to recognize this point, given his life-long struggle with the quantum. It is noteworthy that although he repeats in his 1949 *Autobiographical Notes* [14] the imperative to understand rods and clocks as structured, composite bodies which he had voiced as early as 1921 [15], he makes no concession to the great strides that had been made in the quantum theory of matter in the intervening years.

4 FitzGerald and Bell's "Lorentzian pedagogy"

The insistence on this role of quantum theory in special relativity has recently been referred to as the "truncated" version of the "Lorentzian pedagogy" advocated by J. S. Bell in 1976 [18][11]. The *full* version of this pedagogy involves providing a constructive model of the matter making up a rod and/or clock and solving the equations of motion in the model. I will argue in this section of the paper that Bell's terminology is slightly misplaced: it would more appropriate still to call this reasoning the "FitzGeraldian pedagogy"!

---

[10] See Brown and Pooley [16]. It is argued here that there is in fact a significant dynamical element in Einstein's 1905 reasoning, specifically in relation to the use of the relativity principle, and that it is unclear whether Einstein himself appreciated this.

[11] *Ibid.*

Bell's model has as its starting point a single atom built of an electron circling a much more massive nucleus. Ignoring the back-effect of the electron on the nucleus, Bell was concerned with the prediction in Maxwell's electrodynamics as to the effect on the two-dimensional electron orbit when the nucleus is set gently in motion in the plane of the orbit. Using only Maxwell's equations (taken as valid relative to the rest frame of the nucleus), the Lorentz force law and the relativistic formula linking the electron's momentum and its velocity—which Bell attributed to Lorentz—he determined that the orbit undergoes the familiar longitudinal "Fitzgerald" contraction, and its period changes by the familiar "Larmor" dilation. Bell claimed that a rigid arrangement of such atoms as a whole would do likewise, given the electromagnetic nature of the interatomic/molecular forces. He went on to demonstrate that there is a system of primed variables such that the the description of the *uniformly* moving atom with respect to them is the same as the description of the stationary atom relative to the orginal variables—and that the associated transformations of coordinates are precisely the familiar Lorentz transformations. But it is important to note that Bell's prediction of length contraction and time dilation is based on an analysis of the field surrounding a (gently) *accelerating* nucleus and its effect on the electron orbit.[12] The significance of this point will become clearer in the next section.

Bell cannot be berated for failing to use a truly satisfactory model of the atom; he was perfectly aware that his atom is unstable and that ultimately only a quantum theory of both nuclear and atomic cohesion would do. His aim was primarily didactic. He was concerned with showing us that

> ... we need not accept Lorentz's philosophy [of the reality of the ether] to accept a Lorentzian pedagogy. Its special merit is to drive home the lesson that the laws of physics in any *one* reference frame account for all physical phenomena, including the the observations of moving observers.

For Bell, it was important to be able to demonstrate that length contraction and time dilation can be derived independently of coordinate transformations—independently of a technique involving a change of variables[13].

But this is not strictly what Lorentz did in his treatment of moving bodies, despite Bell's claim that he followed very much Lorentz's approach.[14]

The difference between Bell's treatment and Lorentz's theorem of corresponding states that I wish to highlight is not that Lorentz never discussed accelerating systems. He didn't, but of more relevance is the point that Lorentz's treatment, to put it crudely, is (almost) mathematically the modern change-of-variables-based-on-covariance approach but with the wrong physical interpretation. Lorentz used auxiliary coordinates, field strengths and charge and current densities associated with the observer co-moving with the laboratory, to set up states of the physical bodies and fields that "correspond" to states of these systems when the laboratory is at rest relative to the ether, both being solutions of Maxwell equations. Essentially, prior to Einstein's work, Lorentz failed to

---

[12] This point was not given due emphasis in Brown and Pooley [16].

[13] Bell [18], p. 80.

[14] Bell [18], p. 77. It is noteworthy both that Bell gives no references to Lorentz's papers, and admits on p. 79 that the inspiration for the method of integrating equations of motion in a model of the sort he presented was "perhaps" a remark of J. Larmor.

understand (even when Poincaré pointed it out) that the auxiliary quantities were precisely the quantities that the co-moving observer should be measuring, and not mere mathematical devices. But then to make contact with the actual physics of the ether-wind experiments, Lorentz needed to make a number of further complicating assumptions, the nature of which we need not elaborate here, other than to say that the whole procedure was limited in practice to stationary situations associated with optics, electrostatics and magnetostatics.[15]

The upshot was an explanation of the null results of the ether-wind experiments that was if anything mathematically simpler, but certainly conceptually much more complicated—not to say obscure—than the kind of exercise Bell was involved with in his 1976 essay. It cannot be denied that Lorentz's argumentation, as Pauli noted in comparing it with Einstein's, is dynamical in nature. But Bell's procedure for accounting for length contraction is in fact much closer to FitzGerald's 1889 thinking based on the Heaviside result, summarised in section 2 above. In fact it is essentially a generalization of that thinking to the case of accelerating bodies. It is remarkable that Bell indeed starts his treatment recalling the anisotropic nature of the components of the field surrounding a *uniformly* moving charge, and pointing out that

> In so far as microscopic electrical forces are important in the structure of matter, this systematic distortion of the field of fast particles will alter the internal equilibrium of fast moving material. Such a change of shape, the Fitzgerald contraction, was in fact postulated on empirical grounds by G. F. Fitzgerald in 1889 to explain the results of certain optical experiments.

Bell, like most commentators on FitzGerald and Lorentz, prematurely attributes to them length contraction rather than shape deformation (see above). But more importantly, it is not entirely clear that Bell was aware that FitzGerald had more than "empirical grounds" in mind, that he had essentially the dynamical insight Bell so nicely encapsulates.

Finally, a word about time dilation. It was seen above that Bell attributed its discovery to J. Larmor, who had clearly understood the phenomenon in 1900 in his *Aether and Matter* [21].[16] Indeed, it is still widely believed that Lorentz failed to anticipate time dilation before the work of Einstein in 1905, as a consequence of failing to see that the "local" time appearing in his own (second-order) theorem of corresponding states was more than just a mathematical artifice, but rather the time as read by suitably synschronized clocks at rest in the moving system. It is interesting that if one does an analysis of the famous variation of the MM experiment performed by Kennedy and Thorndike [22] in 1932, exactly in the spirit of Lorentz's 1895 analysis [23] of the MM experiment and *with no allowance for time dilation*, then the result, taking into account the original MM outcome too, is *the wrong kind of deformation for moving bodies.*[17] It can easily

---

[15] Readers interested in Lorentz's daunting theorem of corresponding states, and its development over the period 1895 to 1904, are strongly encouraged to consult the detailed and unusually perceptive analysis given by M. Janssen [19], [20].

[16] See Bell [18], pp. 79-80.

[17] Kennedy and Thorndike [22] have as the title of their paper "Experimental Establishment of the Relativity of Time", but their experiment does not imply the existence of time dilation unless it is assumed that motion-induced deformation in rigid bodies is purely longitudinal—indeed, just the usual length contraction. As mentioned above, this specific kind of deformaton

be shown that rods must contract transversely by the factor γ^{-1} and longitudinally by the factor γ^{-2}. One might be tempted to conclude that Lorentz, who had opted for purely longitudinal contraction (for dubious reasons), was lucky that it took so long for the Kennedy-Thorndike experiment to be performed!

But the conclusion is probably erroneous. In 1899, Lorentz [24] had already discussed yet another interesting variation of the MM experiment, suggested a year earlier by the French scientist A. Liénard, in which transparent media were placed in the arms of the interferometer. The experiment had not been performed, but Lorentz both suspected that a null result would still be obtained, and realised that shape deformation of the kind he and FitzGerald had proposed would not be enough to account for it. What was lacking, according to Lorentz? Amongst other things, the claim that the frequency of oscillating electrons in the light source is lower in systems in motion than in systems at rest relative to the ether. It seems that the question of the authorship of time dilation is ripe for reanalysis.[18]

5 What spacetime is not

If you visit the Museum of the History of Science in Oxford, you will find a number of fine examples of 18th and 19th century devices called *waywisers*, designed to measure distances along roads. Typically, these devices consist of an iron-rimmed wheel, connected to a handle and readout dial. The dial registers the number of revolutions of the wheel as the whole device is pushed along the road, and has hands which indicate yards and furlongs/miles. (Modern, smaller versions of the waywiser are seen in operation even today.) The makers of these original waywisers had a premonition of relativity! For the dials on the waywisers typically look like clocks. And true, ideal clocks are of course the waywisers, or hodometers, of time-like roads in spacetime.

The *mechanism* of the old waywiser is obvious; there is no mystery as to how friction with the road causes the wheel to revolve, and how the information about the number of such 'ticks' is mechanically transmitted to the dial. But the true clock is more subtle. There is no friction with spacetime, no analogous mechanism by which the clock reads off four-dimensional distance. How does it work?

One of Bell's professed aims in his 1976 paper on 'How to teach relativity' was to fend off "premature philosophizing about space and time"[19]. He hoped to achieve this by demonstrating with an appropriate model that a moving rod contracts, and a moving clock dilates, *because of how it is made up and not because of the nature of its spatiotemporal environment.* Bell was surely right. Indeed, if it is the structure of the background spacetime that accounts for the phenomenon, by what mechanism is the rod or clock informed as to what

---

is not a consequence of the MM experiment, and was still not established experimentally in 1932 (although it was widely accepted). What the Kennedy-Thorndike experiment established unequivocally, in conjunction with the MM experiment, is that the two-way light speed is (inertial) frame-independent.

[18] I am again indebted to the scholarship of M. Janssen for recognition of this point; Janssen is the first historian of the period to my knowledge to emphasize in [19], [20] the nature of Lorentz's 1899 response to Liénard's suggestion.

[19] See Bell [18], p. 80.

this structure is? How does this material object get to know which type of spacetime—Galilean or Minkowskian, say—it is immersed in?[20]

Some critics of Bell's position may be tempted to appeal to the general theory of relativity as supplying the answer. After all, in this theory the metric field *is* a dynamical agent, both acting and being acted upon by the presence of matter. But general relativity does not come to the rescue in this way (and even if it did, the answer would leave special relativity looking incomplete). Indeed the Bell-Pauli-Swann lesson—which might be called the *dynamical* lesson—serves rather to highlight a feature of general relativity that has received far too little attention to date. It is that in the absence of the strong equivalence principle, the metric $g_{\mu\nu}$ in general relativity has no automatic *chronometric* operational interpretation.[21]

For consider Einstein's field equations

$$R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R = \kappa T_{\mu\nu} \tag{2}$$

where $R_{\mu\nu}$ is the Ricci tensor, R the curvature scalar and $T_{\mu\nu}$ the stress energy tensor associated with matter fields. A possible spacetime, or metric field, corresponds to a solution of this equation, but nothing in the form of the equation determines either the metric's signature or its operational significance. In respect of the last point, the situation is not wholly dissimilar from that in Maxwellian electrodynamics, in the absence of the Lorentz force law. In both cases, the ingredient needed for a direct operational interpretation of the fundamental fields is missing.

But of course there is more to general relativity than the field equations. There is, besides the specification of the Lorentzian signature for $g_{\mu\nu}$, the crucial assumption that locally physics looks Minkowskian. (Mathematically of course the tangent spaces are automatically Minkowksian, but the issue is one of physics, not mathematics.) It is a component of the strong equivalence principle that in 'small enough' regions of spacetime, for most practical purposes the physics of the non-gravitational interactions takes its usual Lorentz covariant form.[22] In short, as viewed from the perspective of the local Lorentz frames, special relativity holds when the effects of spacetime curvature—tidal forces—can be ignored. It is this extra assumption, which brings in *quantum* physics even if this point is rarely emphasised, that guarantees that ideal clocks, for example, can both be defined and shown to survey the postulated metric field $g_{\mu\nu}$ when they are moving inertially. Only now is the notion of proper time linked to the metric. But yet more has to be assumed before the metric gains its full, familiar chronometric significance.

The final ingredient is the so-called *clock hypothesis* (and its analogue for rods).[23] This is the claim that when a clock is accelerating, the effect of motion on the rate of the clock is no more than that associated with its instantaneous velocity—the acceleration adds nothing. This allows for the identification of the integration of the metric along an *arbitrary* time-like curve—not just a

---

[20] See Brown [25].

[21] For a more detailed discussion of the claims made in this section—except those related to the clock hypothesis—see Brown and Pooley [16].

[22] See, e.g. Misner *et al.* [26], p. 386.

[23] I am assuming here that the clock hypothesis is not a consequence of the strong equivalence principle, but this is admittedly depends on how the latter is defined.

geodesic—with the proper time. This hypothesis is no less required in general relativity than it is in the special theory.[24] The only work that I am aware of that provides the rudiments of a constructive justification of this hypothesis within the theory of matter is Bell's 1976 essay [18]. Bell did not underline this aspect of his discussion, but it deserves more attention. It may be the first demonstration that clocks and rods—admittedly modelled in a crude fashion—undergo dilation and contraction which depend only on velocity and not acceleration (if I have understood Bell's calculation) as a result of the full equations of motion in the model. Ultimately, of course, it must be believed that the clock and rod hypotheses are consequences of the quantum theory of the fundamental non-gravitational interactions involved in material structure.

In conclusion, the operational meaning of the metric is ultimately made possible by appeal to quantum theory, in general relativity as much as in the special theory. The only—and significant—difference is that in special relativity, the Minkowskian metric is no more than a codification of the behaviour of rods and clocks, or equivalently, it is no more than the Kleinian geometry associated with the symmetry group of the quantum physics of the non-gravitational interactions in the the theory of matter. In general relativity, on the other hand, the $g_{\mu\nu}$ field is an autonomous dynamical player, physically significant even in the absence of the usual 'matter' fields. But its meaning as a carrier of the *physical* metrical relations between spacetime points is a bonus, the gift of the strong equivalence principle and the clock (and rod) hypothesis.

It is surely an oddity of general relativity in its standard formulation that the beautiful and far-reaching connection between these principles and the field equations for a metric field with Lorentzian signature has the appearance of a shot-gun marriage, or at least of an accident. Shouldn't we hope to see in future developments in the physics of the fundamental interations including gravity, a way in which these disparate elements of general relativity flow from a deeper, unifying principle?

6 Final remarks

It seems to be widely accepted today that Einstein owed little to the Michelson-Morley experiment in his development of relativity theory, although it cannot but have buttressed his conviction in the validity of the relativity principle (not, as is sometimes claimed, his light postulate)—or at least its applicability to electromagnetic phenomena. Be that as it may, there is no doubt about the spur the MM experiment gave to the insights gained by FitzGerald and Lorentz concerning the effects of motion on the dimensions of rigid bodies. It is my hope that commentators in the future will increasingly recognize the importance of these these insights, and that the contributions of the two pioneers will emerge from the shadow cast by Einstein's 1905 'kinematic' analysis. As Bell [11] and [18] argued, the point is not that Einstein erred, so much as that the messier, less economical reasoning based on 'special assumptions about the composition of matter' can lead to greater insight, in the manner that statistical mechanics can offer more insight than thermodynamics. The longer road, Bell reminded us, may lead to more familiarity with the country.

---

[24] See, e.g., Rindler [27], pp. 43, 116.

7 Acknowledgements

I wish to thank Julian Ławrynovicz for his invitation to and hospitality during the 2002 International Conference: *Ideas of Albert Abraham Michelson in Mathematical Physics* held in Bedlewo. I very much appreciated, amongst other things, the opportunity during the conference to visit Michelson's birthplace and learn more of his early life. I acknowledge helpful discussions of an earlier draft of this paper with Eleanor Knox and particularly Oliver Pooley and Brian Wybourne. The last of these was a source of encouragement from the start and I thank him and his colleagues at the Institute of Physics, Nicholas Copernicus University, Torun, for their interest and hospitality.

References

[1] O. Heaviside (1888), 'The electro-magnetic effects of a moving charge', *Electrician* **22**,147–148.

[2] G. F. C. Searle (1896), 'Problems in electrical convection', *Philosophical Transactions of the Royal Society of London*, Series A **187**, 675–713.

[3] A. A. Michelson and E. W. Morley (1887), 'On the Relative Motion of the Earth and the Luminiferous Ether', *American Journal of Science* **34**, 333–345.

[4] H. R. Brown (2001), 'The origin of length contraction: 1. The FitzGerald-Lorentz deformation hypothesis', *American Journal of Physics* **69**, 1044–1054. E-prints: gr-qc/0104032; PITT-PHIL-SCI00000218.

[5] J. M. Bennett, D. T. McAllister and G. M. Cabe (1973), 'Albert A. Michelson, Dean of American Optics—Life, Contributions to Science, and Influence on Modern-Day Physics', *Applied Optics* **12**, 2253–2279.

[6] O. Lodge (1905), 'G. F. FitzGerald', *Proceedings of the Royal Society* **75**, 152–60.

[7] J. M. D. Coey (2000), 'George Francis FitzGerald, 1851–1901. The Millenium Trinity Monday Memorial Discourse', *http://www.tcd.ie/Physics/History/Fitzgerald/FitzGerald.html.*

[8] B. J. Hunt (1988), 'The Origins of the FitzGerald Contraction', *British Journal for the History of Science* **21**, 61–76.

[9] G. F. FitzGerald (1889), 'The Ether and the Earth's Atmosphere', *Science* **13**, 390.

[10] S. G. Brush (1967), 'Note on the History of the FitzGerald-Lorentz Contraction', *Isis* **58**, 230–232.

[11] J. S. Bell (1992), 'George Francis FitzGerald', 1989 lecture, abridged by Denis Weare in *Physics World* **5**, 31–35.

[12] A. Einstein (1905), 'Elektrodynamik bewegter Körper, *Annalen der Physik* **17**, 891–921.

[13] W. Pauli (1981), *Theory of Relativity*, Dover, New York. Originally published as 'Relativitätstheorie', *Encyklopädie der matematischen Wissenschaften* Vol. V19, B. G. Tauber, Leibzig, 1921.

[14] A. Einstein (1969), 'Autobiographical Notes', in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*, Vol. 1, Open Court, Illinois; pp. 1–94.

[15] A. Einstein (1921), 'Geometrie und Erfahrung', *Erweite Fassung des Festvortrages gehalten an der presussischen Akademie* Berlin: Springer. Translated by S. Bargmann as 'Geometry and Experience' in A. Einstein, *Ideas and Opinions*, Crown Publishers, Inc., New York; pp. 232–246.

[16] H. R. Brown and O. Pooley, 'The origin of the spacetime metric: Bell's 'Lorentzian pedagogy' and its significance in general relativity', in *Physics Meets Philosophy at the Planck Length*, C. Callender and N. Huggett (eds.), Cambridge University Press, 2001; pp. 256–272; e-print gr-qc/9908048.

[17] W. F. G. Swann (1941), 'Relativity, the FitzGerald-Lorentz Contraction, and Quantum Theory', *Reviews of Modern Physics* **13**, 197–202.

[18] J. S. Bell (1976). 'How to teach special relativity', *Progress in Scientific Culture* **1**. The article is reprinted in J. S. Bell, *Speakable and unspeakable in quantum mechanics*, Cambridge University Press, Cambridge; pp. 67–80, from which page references in the present paper are taken.

[19] M. Janssen (1995), 'A comparison between Lorentz's Ether Theory and Special Relativity in the Light of the Experiments of Trouton and Noble', Ph.D. thesis, University of Pittsburgh.

[20] M. Janssen and J. Stachel, 'The Optics and Electrodynamics of Moving Bodies', in *Storia della Scienza* **7**, S. Petruccioli *et al.* (eds.), Istituto della Enciclopedia Italiana; to appear.

[21] J. Larmor (1900), *Aether and Matter*, University Press, Cambridge.

[22] R. J. Kennedy and E. M. Thorndike (1932), 'Experimental Establishment of the Relativity of Time', *Physical Review* **42**, 400–418.

[23] H. A. L. Lorentz (1895), *Versuch einer Thoerie der electrischen und optischen Erscheinungen in bewegten Körpern*, Brill, Leyden.

[24] H. A. L. Lorentz (1899) 'Simplified Theory of Electrical and Optical Phenomena in Moving Systems', *Koninklijke Akademie van Wetenschappen te Amsterdam*, Section of Sciences, Proceedings 1, 427–442.

[25] H. R. Brown (1993), 'Correspondence, Invariance and Heuristics in the Emergence of Special Relativity', in S. French and H. Kamminga (eds.), *Correspondence, Invariance and Heuristics; Essays in Honour of Heinz Post*, Kluwer Academic Press, Dordrecht, 1993; pp. 227-260. Republished in J. Butterfield, M. Hogarth and G. Belot (eds.) *Spacetime*, International Research Library of Philosophy, Dartmouth Publishing Company, 1996.

[26] C. W. Misner, K. S. Thorne and J. A. Wheeler (1973), *Gravitation*, W. H. Freeman, San Francisco.

[27] W. Rindler (1977), *Essential Relativity. Special, General and Cosmological* Revised second edition; Springer-Verlag, New York.