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The origin of the spacetime metric: Bell's 'Lorentzian pedagogy' and its significance in general relativity*

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Abstract

The purpose of this paper is to evaluate the 'Lorentzian pedagogy' defended by J.S. Bell in his essay "How to teach special relativity", and to explore its consistency with Einstein's thinking from 1905 to 1952. Some remarks are also made in this context on Weyl's philosophy of relativity and his 1918 gauge theory. Finally, it is argued that the Lorentzian pedagogy—which stresses the important connection between kinematics and dynamics—clarifies the role of rods and clocks in general relativity.

1 Introduction

In 1976, J.S. Bell published a paper on 'How to teach special relativity' (Bell 1976). The paper was reprinted a decade later in his well-known book *Speakable and unspeakable in quantum mechanics*—the only essay to stray significantly from the theme of the title of the book. In the paper Bell was at pains to defend a dynamical treatment of length contraction and time dilation, following "very much the approach of H.A. Lorentz" (Bell 1987, 77).

Just how closely Bell stuck to Lorentz's thinking in this connection is debatable. We shall return to this question shortly. In the meantime we shall briefly rehearse the central points of Bell's rather unorthodox argument.

Bell considered a single atom modelled by an electron circling a more massive nucleus, ignoring the back-effect of the field of the electron on the nucleus. The question he posed was: what is the prediction in Maxwell's electrodynamics (taken to be valid relative to the rest-frame of the nucleus) as to the effect on

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the electron orbit when the nucleus is set (gently) in motion in the plane of the orbit? Using only Maxwell's field equations, the Lorentz force law and the relativistic formula linking the electron's momentum and its velocity—which Bell attributed to Lorentz—he concluded that the orbit undergoes the familiar longitudinal ("Fitzgerald" (sic)) contraction, and its period changes by the familiar ("Larmor") dilation. Bell went on to demonstrate that there is a system of primed variables such that the description of the moving atom with respect to them coincides with that of the stationary atom relative to the original variables, and the associated transformations of coordinates is precisely the familiar Lorentz transformation.

Bell carefully qualified the significance of this result. He stressed that the external forces involved in boosting a piece of matter must be suitably constrained in order that the usual relativistic kinematical effects such as length contraction be observed (see section 5 below). More importantly, Bell acknowledged that Maxwell-Lorentz theory is incapable of accounting for the stability of solid matter, starting with that of the very electronic orbit in his atomic model; nor can it deal with cohesion of the nucleus. (He might also have included here the cohesion of the electron itself.) How Bell addressed this shortcoming of his model is important, and will be discussed in section 3 below. In the meantime we note that the positive point Bell wanted to make was about the wider nature of the Lorentzian approach: that it differed from that of Einstein in 1905 in both *philosophy* and *style*.

The difference in philosophy is well-known and Bell did not dwell on it. It is simply that Lorentz believed in a preferred frame of reference—the rest-frame of the ether—and Einstein did not, regarding the notion as superfluous. The interesting distinction, rather, was that of style. Bell argues first that "we need not accept Lorentz's philosophy 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 observations of moving observers." He went on to stress that Einstein postulates what Lorentz is attempting to prove (the relativity principle). Bell has no "reservation whatever about the power and precision of Einstein's approach"; his point is that "the longer road [of FitzGerald, Lorentz and Poincaré] sometimes gives more familiarity with the country" (1987, 77).

The point, then, is not the existence or otherwise of a preferred frame—and we have *no* wish to defend such an entity in this paper. It is how best to understand, and teach, the origins of the relativistic 'kinematical' effects. Near the end of his life, Bell reiterated the point with more insistence:

> "If you are, for example, quite convinced of the second law of thermodynamics, of the increase of entropy, there are many things that you can get directly from the second law which are very difficult to get directly from a detailed study of the kinetic theory of gases, but you have no excuse for not looking at the kinetic theory of gases to see how the increase of entropy actually comes about. In the same way, although Einstein's theory of special relativity would

> lead you to expect the FitzGerald contraction, you are not excused from seeing how the detailed dynamics of the system also leads to the FitzGerald contraction." (Bell 1992, 34)

There is something almost uncanny in this exhortation. Bell did not seem to be aware that just this distinction between thermodynamics and the kinetic theory of gases was foremost in Einstein's mind when he developed his fall-back strategy for the 1905 relativity paper (see section 2).

It is the principal object of this paper to analyse the significance of what Bell calls the 'Lorentzian pedagogy' in both special relativity and general relativity. Its merit is to remind us that in so far as rigid rods and clocks can be used to survey the metrical structure of spacetime (and the extent to which they do will vary from theory to theory), their status as *structured* bodies—as "moving atomic configurations" in Einstein's words—must not be overlooked. The significance of the dynamical nature of rods and clocks, and the more general theme of the entanglement between kinematics and dynamics, are issues which in our opinion deserve more attention in present-day discussions of the physical meaning of spacetime structure.

2 Chalk and cheese: Einstein on the status of special relativity theory

Comparing the explanation in special relativity (SR) of the *non-null* outcome of the celebrated 1851 Fizeau interferometry experiment—a direct corroboration of the Fresnel drag coefficient—with the earlier treatment given by Lorentz can seem like comparing chalk and cheese.

From the perspective of SR the drag coefficient is essentially a simple consequence (to first order) of the relativistic velocity transformation law, itself a direct consequence of the Lorentz transformations. The explanation appears to be entirely kinematical. Lorentz, on the other hand, had provided a detailed dynamical account—based on his theory of the electron—of the microstructure of the moving transparent medium (water in the case of the Fizeau experiment) and its interaction with the light passing through it (Lorentz 1892). In his 1917 text *Relativity*, Einstein noted with satisfaction that the explanation of Fizeau's experiment in SR is achieved "without the necessity of drawing on hypotheses as to the physical nature of the liquid" (Einstein 1961, 57).

Yet Lorentz had achieved something remarkable, and Einstein knew it. In deriving the drag coefficient from principles contained within his theory of the electron, Lorentz was able to reconcile the null results of *first-order* ether-wind experiments (all of which incorporated moving transparent media) with the claimed existence of the luminiferous ether itself. There were few, if any, complaints that such a surprising reconciliation was obtained on the basis of *ad hoc* reasoning on Lorentz's part. But the case of the *second-order* ether-wind experiments was of course different, and it is worth noting Einstein's take on these, again as expressed in *Relativity*.

In order to account for the null result of the 1887 Michelson-Morley (M-M) experiment, Lorentz and FitzGerald assumed, says Einstein, "that the motion of the body [the Michelson interferometer] relative to the aether produces a contraction of the body in the direction of motion" (1961, 59). Einstein's claim is not quite right. In fact, both Lorentz and FitzGerald had correctly and independently realised that it was sufficient to postulate any one of a certain family of motion-induced distortions: the familiar longitudinal contraction is merely a special case and not uniquely picked out by the M-M null result.[1] But this common historical error (repeated by Bell) should not detain us, for the real issue lies elsewhere. In SR, Einstein stresses, "the contraction of moving bodies follows from the two fundamental principles of the theory [the relativity principle and the light postulate], without the introduction of particular hypotheses " (1961, 59).

The "particular hypotheses" of FitzGerald and Lorentz went beyond the phenomenological claim concerning the distortion of rigid bodies caused by motion through the ether. Both these physicists, again independently, attempted to justify this startling claim by surmising, not unreasonably, that the molecular forces in rigid bodies, and in particular in the stone block on which the Michelson interferometer was mounted, are affected by the ether-wind in a manner similar to that in which electromagnetic interactions are so affected when viewed from the ether rest-frame. Unlike their contemporary Larmor, neither FitzGerald nor Lorentz was prepared to commit himself to the claim that the molecular forces *are* electromagnetic in origin. In this sense, their courageous solution to the conundrum posed by the M-M experiment did involve appeal to hypotheses outside what Einstein referred to as the 'Maxwell-Lorentz' theory.

Indeed, it was precisely their concern with rigid bodies that would have made FitzGerald and Lorentz less that wholly persuaded by Bell's construction above, as it stands. It is not just that Bell's atomic model relies on post-1905 developments in physics. The point is rather that Bell does not discuss the forces that glue the atoms together—the analogue of the 'molecular forces'—to form a rigid body like Michelson's stone.[2] (Bell of course would have known that they were also electromagnetic in origin while, as we have seen, FitzGerald and Lorentz were uncertain and unwilling to commit themselves on this point.)

Returning to Einstein, in *Relativity* he also mentioned the case of predictions concerning the deflection of high-velocity electrons (cathode- and beta-rays) in electromagnetic fields (1961, 56). Lorentz's own predictions, which coincided with Einstein's, were obtained by assuming *inter alia* that the electron itself deforms when in motion relative to the ether. It is worth recalling that predictions conflicting with those of Lorentz and Einstein had been made by several workers; those of M. Abraham (an acknowledged authority on Maxwellian electrodynamics) being based on the hypothesis of the non-deformable electron.[3] Einstein's point was that whereas Lorentz's hypothesis is "not justifiable by any electrodynamical facts" and hence seems extraneous, the predictions in SR

[1] For a recent account of the origins of length contraction see Brown (1999).

[2] We are grateful to P. Holland for emphasising this point (private communication).

[3] Details of this episode can be found in Miller (1981, chapter 1).

are obtained "without requiring any special hypothesis *whatsoever* as to the structure and behaviour of the electron" (1961, 56, our emphasis).

Whatsoever? Not quite, as we see in the next section. But for the moment let us accept the thrust of Einstein's point. The explanations of certain effects given by Maxwell-Lorentz theory and SR differ in both style and degree of success: in some cases the Maxwell-Lorentz theory actually seems incomplete. Yet there was a stronger reason for the difference in style, and for the peculiarities of the approach that Einstein adopted in 1905 (peculiarities which should be borne in mind when evaluating claims—which resurface from time to time—that Poincaré was the true father of SR). Part of the further story emerges in 1919, in a remarkable article Einstein wrote for the London *Times* (Einstein 1919), when he characterised SR as an example of a 'principle theory', methodologically akin to thermodynamics, as opposed to a 'constructive theory', akin to the kinetic theory of gases. Like all good distinctions, this one is hardly absolute, but it is enlightening. It is worth dwelling on it momentarily.

In 1905 Einstein was faced with a state of confusion in theoretical physics largely caused by Planck's 1900 solution to the vexing problem of blackbody radiation. Not that the real implications of Planck's quantum revolution were widely appreciated by 1905, even by Planck; but that year saw Einstein himself publish a paper with the revolutionary suggestion that free radiation itself had a quantised, or granular structure (Einstein 1905a). What his light-quantum proposal undoubtedly implied in Einstein's mind was that the Maxwell-Lorentz theory was probably only of approximate, or statistical validity. Now within that theory, Lorentz, with the help of Poincaré, had effectively derived the Lorentz (coordinate) transformations as the relevant sub-group of the linear covariance group of Maxwell's equations, consistent moreover with the FitzGerald-Lorentz deformation hypothesis for rigid bodies. But if Maxwell's field equations were not to be considered fundamental, and furthermore the nature of the various forces of cohesion within rigid bodies and clocks was obscure, how was one to provide a rigorous derivation of these coordinate transformations, which would determine the behaviour of moving rods and clocks? Such a derivation was essential if one wanted, as Einstein did, to tackle the difficult problem of solving Maxwell's equations in the case of moving charge sources.

It is important to recognise that Einstein's solution to this conundrum was the result of despair, as he admits in his *Autobiographical Notes* (Einstein 1969). Einstein could not see any secure foundation for such a derivation on the basis of "constructive efforts based on known facts" (Einstein 1969, 53). In the face of this *impasse*, Einstein latched on to the example of thermodynamics. If for some reason one is bereft of the means of mechanically modelling the internal structure of the gas in a single-piston heat engine, say, one can always fall back on the laws of thermodynamics to shed light on the performance of that engine—laws which stipulate nothing about the structure of the gas, or rather hold whatever that structure might be. The laws or principles of thermodynamics are phenomenological, the first two laws being susceptible to formulation in terms of the impossibility of certain types of perpetual motion machines. Could similar, well-established phenomenological laws be found, Einstein asked, which would constrain the behaviour of moving rods and clocks without the need to know in detail what their internal dynamical structure is?

In a sense, Galileo's famous thought-experiment involving a ship in uniform motion is an impossibility claim akin to the perpetual-motion dictates of thermodynamics: no effect of the ship's motion is detectable in experiments being performed in the ship's cabin. The Galileo-Newton relativity principle was probably originally proposed without any intention of restricting it to non-electromagnetic or non-optical experiments (see Brown & Sypel 1995). In the light of the null ether-wind experiments of the late 19th century, Einstein, like Poincaré, adopted the principle in a form which simply restored it to its original universal status. In Einstein's words:

> The universal principle of the special theory of relativity [the relativity principle] ... is a restricting principle for natural laws, comparable to the restricting principle of the non-existence of the *perpetuum mobile* which underlies thermodynamics. (Einstein 1969, 57)

Turning to Einstein's second postulate, how apt, if at first sight paradoxical, was its description by Pauli as the "true essence of the aether point of view" (Pauli 1981, 5). Einstein's light postulate—the claim that relative to a certain 'resting' coordinate system, the two-way light-speed is constant (isotropic and independent of the speed of the source)—captures that phenomenological aspect of all ether theories of electromagnetism which Einstein was convinced would survive the maelstrom of changes in physics that Planck had started. Combined now with the relativity principle, it entailed the invariance of the two-way light-speed. This was not the only application of the relativity principle in Einstein's 1905 derivation of the Lorentz transformations (1905b), as we discuss in the next section.[4]

Einstein had now got what he wanted in the Kinematical Part of his 1905 paper, without committing himself therein to the strict validity of Maxwell's equations and without speculation as to the detailed nature of the cohesion forces within material bodies such as rods and clocks. But there was a price to be paid. In comparing 'principle theories' such as thermodynamics with 'constructive theories' such as the kinetic theory of gases in his 1919 *Times* article, Einstein was quite explicit both that special relativity is a principle theory, and that principle theories lose out to constructive theories in terms of explanatory power:

> ... when we say we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question. (1982, 228)

This was essentially the point Bell was to make half a century later.[5]

[4]Nor were the two principles together strictly sufficient. The isotropy of space was also a crucial, if less prominent, principle in the derivation. Detailed examinations of the logic of Einstein's derivation can be found in Brown & Maia (1993) and Brown (1993). For an investigation of the far-reaching implications of abandoning the principle of spatial isotropy in the derivation, see Budden (1997).

[5]In further elucidating the principle theory versus constructive theory distinction, we might

3 The significance of the Lorentzian pedagogy

We saw in section 1 that Bell was aware in his 1976 essay of the limitations of the Maxwell-Lorentz theory in accounting for stable forms of material structure. He realised that a *complete* analysis of length contraction, say, in the spirit of the Lorentzian pedagogy would also require reference to forces other than of electromagnetic origin, and that the whole treatment would have to be couched in a quantum framework. But it is noteworthy that Bell did not seem to believe that *articulation* of a *complete* dynamical treatment of this kind was a necessary part of the Lorentzian pedagogy. In order to predict, on dynamical grounds, length contraction for moving rods and time dilation for moving clocks, Bell recognised that one need not know exactly how many distinct forces are at work, nor have access to the detailed dynamics of all of these interactions or the detailed micro-structure of individual rods and clocks. It is enough, said Bell, to assume Lorentz covariance of the complete dynamics—known or otherwise—involved in the cohesion of matter. We might call this the *truncated* Lorentzian pedagogy.

It is at this important point in Bell's essay that one sees something like a re-run of the thinking that the young Pauli brought to bear on the significance of relativistic kinematics in his acclaimed 1921 review article on relativity theory (Pauli 1981). Pauli was struck by the "great value" of the apparent fact that, unlike Lorentz, Einstein in 1905 had given an account of his kinematics which was free of assumptions about the constitution of matter. He wrote:

> Should one, then, completely abandon any attempt to explain the Lorentz contraction atomistically? We think that the answer to this question should be No. 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 electron theory, as well as of those laws, as yet unknown to us, which determine the cohesion of the electron itself. (Pauli 1981, 15)

Both Pauli and Bell seem then to contrast the dynamical underpinning of relativistic kinematics with Einstein's 1905 argument. But it seems to us that once

consider the Casimir effect (attraction between conducting plates in the vacuum). This effect is normally explained on the basis of vacuum fluctuations in QED—the plates merely serving as boundary conditions for the q-number photon field. But it can also be explained in terms of, say, Schwinger's source theory, which uses a c-number electromagnetic field generated by the sources in the plates so that the effect is ultimately due to interactions between the microscopic constituents of the plates. (For a recent analysis of the Casimir effect, see Rugh *et al.* (1999).) It might appear that the relationship between the first approach and the second is similar to that between Einstein's formulation of SR and the 'Maxwell-Lorentz theory': the QED approach is simpler that Schwinger's and makes no claims as to the microscopic constitution of the plates (other than the claim that they conduct). But this appearance is misleading. Both approaches are equally 'constructive' in Einstein's sense; it is just that one appeals to the quantum structure of the vacuum and the other to fluctuating dipole moments associated with atoms or molecules in the plates.

the Lorentzian pedagogy relinquishes detailed specification of the dynamical interactions involved—in other words once it takes on the truncated form—the difference between it and Einstein's approach, although significant, can easily be overstated. Indeed, we regard it as just wrong to construe Einstein's 1905 'kinematical' derivation of the Lorentz transformations as free of assumptions about the constitution of matter, despite the distance between SR and the Maxwell-Lorentz theory that Einstein urges (see above) in his *Relativity*.

This is best seen in the second application of the relativity principle in Einstein's argument. The first application, it will be recalled, establishes the invariance of the two-way light-speed, given the light postulate. Adopting the Einstein convention for synchronising clocks in both the moving and rest frames, this entails that the linear coordinate transformations take on the form of the Lorentz transformations up to a non-trivial scale or conformal factor. Einstein is now faced with the problem of reducing this factor to unity (a problem which bedevilled Lorentz virtually throughout the development of his theory of the electron). Einstein achieves this (as did Poincaré independently) by a second appeal to the relativity principle, in order to guarantee the group property of the transformations—in particular to ground the claim that the form of the transformation does not depend on the choice of frames. This, together with an appeal to the principle of spatial isotropy, does the trick. The details need not concern us; the interesting question is how this second application of the relativity principle should be understood.

The coordinate transformations encode the behaviour of moving ideal rulers and clocks under the crucial and universally accepted assumption that these devices retain their rest lengths and periods respectively under boosts. Suppose now that the coordinate transformations between frames S and S' are different in form from their inverse. We expect in this case either the length contraction factor or the time dilation factor (if any), or both, to differ when measured relative to S and when measured relative to S'. And this would imply a violation of the relativity principle. Specifically, it would be inconsistent with the claim that the dynamics of all the fundamental non-gravitational interactions which play a role in the cohesion of these bodies satisfy the relativity principle. Thus the *dynamical* relativity principle constrains the form of the *kinematical* transformations, because such kinematics encodes the universal dynamical behaviour of rods and clocks in motion.

It was clearly of importance to Bell that the Lorentzian pedagogy relied on physics specified relative to a *single* inertial frame in order to account for the "observations of moving observers", and in particular the very validity of the relativity principle itself. But ultimately that physics amounted to the claim that the complete theory of the construction of matter is Lorentz covariant, of which the relativity principle *inter alia* is a consequence. Einstein on the other hand started with the relativity principle and the light postulate, and derived (using the isotropy of space) Lorentz covariance. In comparing these two approaches, two points must not be lost sight of. The first is that Einstein's argument is dynamical, since kinematics and dynamics in this context cannot be disentangled.[6] The second point is that his 'principle theory' approach to relativistic kinematics ruled out the truncated Lorentzian pedagogy as a possible starting point for Einstein.

4 Einstein's unease about rods and clocks in special relativity

The extent to which Einstein understood the full dynamical implications of his 1905 derivation of the Lorentz transformations is perhaps unclear. Specifically it is not clear that he recognised the role that rods and clocks can be seen to play in the derivation as *structured* bodies. What is clearer is that he harboured, or developed, a sense of unease about the status of these bodies in his initial formulation of SR. Einstein made use of these devices in the first instance to operationalise the spatial and temporal intervals, respectively, associated with inertial frames, but he never explained where they come from. In an essay entitled 'Geometrie und Erfahrung' (Einstein 1921), Einstein wrote:

> It is ... clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures, which must not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these concepts must still be employed as independent concepts; for we are still far from possessing such certain knowledge of the theoretical principles of atomic structure as to be able to construct solid bodies and clocks theoretically from elementary concepts. (Einstein 1982, 236, 237)

Einstein's unease is more clearly expressed in a similar passage in his 1949 *Autobiographical Notes*:

> One is struck [by the fact] that the theory [of special relativity] ... introduces two kinds of physical things, i.e., (1) measuring rods and clocks, (2) all other things, e.g., the electromagnetic field, the material point, etc. This, in a certain sense, is inconsistent; strictly speaking measuring rods and clocks would have to be represented as solutions of the basic equations (objects consisting of moving atomic configurations), not, as it were, as theoretically self-sufficient entities. However, the procedure justifies itself because it was clear from the very beginning that the postulates of the theory are not strong enough to deduce from them sufficiently complete equations ... in order to base upon such a foundation a theory of measuring rods and clocks. ... But one must not legalize the mentioned sin so far as to

[6] The entangling of kinematics and dynamics is not peculiar to the relativistic context: details of a similar dynamical derivation of the Galilean transformations due to the fictional Albert Keinstein in 1705 are given in Brown (1993).

> imagine that intervals are physical entities of a special type, intrinsically different from other variables ('reducing physics to geometry', etc.). (1969, 59, 61)

It might seem that the justification Einstein provides for the self-confessed 'sin' of treating rods and clocks as 'irreducible', or 'self-sufficient' in 1905 is different in the two passages. In the 1921 essay, Einstein is saying that the constructive physics of atomic aggregation is still too ill-defined to allow for the modelling of such entities, whereas in the 1949 passage the point is that his 1905 postulates were insufficient in the first place to constrain the theory of matter in the required way—which is little more than a re-statement of the problem. But as we have seen, it was precisely the uncertainties surrounding the basic constructive principles of matter and radiation that led Einstein in 1905 to base his theory on simple, phenomenological postulates.

Now there are two ways one might interpret these passages by Einstein. One might take him to be expressing concern that his 1905 derivation fails to recognise rods and clocks as complex, structured entities. We argued in the previous section that this is not the case. While the derivation is independent of the details of the laws which describe their internal structure, it is completely consistent with the true status of rods and clocks as complex solutions of (perhaps unknown) dynamical equations. In fact, the derivation implicitly treats them as such when the second appeal to the relativity principle is made.

Alternatively, one might take Einstein to be concerned about the fact that his postulates could not account for the availability of rods and clocks in the world in the first place. If this is his concern, then it is worth noting that the possibility of the existence of rods and clocks likewise does not follow from the mere assumption that all the fundamental laws are Lorentz covariant. It is only a full-blown quantum theory of matter, capable of dealing with the formation of stable macroscopic bodies that will fill the gap.

The significance of this point for the truncated Lorentzian pedagogy and the constructive theory of rods and clocks in Einstein's thought are themes we will return to in section 6. In the meantime, two points are worth making. First, it is perhaps odd that in his *Autobiographical Notes*, Einstein makes no mention of the advances that had occurred since 1905 in the quantum theory of matter and radiation. The understanding of the composition of bodies capable of being used as rods and clocks was far less opaque in 1949 than it was in 1905. Second, there is a hint at the end of the second passage above that towards the end of his life, Einstein did not view geometrical notions as fundamental in the special theory. An attempt to justify this skepticism, at least in relation to four-dimensional geometry, is given in section 6 below.

5 A digression on rods and clocks in Weyl's 1918 unified field theory

In discussing the significance of the M-M experiment in his text *Raum-Zeit-Materie* (Weyl 1918a), Hermann Weyl stressed that the null result is a consequence of the fact that "the interactions of the cohesive forces of matter as well as the transmission of light" are consistent with the requirement of Lorentz covariance (Weyl 1952, 173). Weyl's emphasis on the role of "the mechanics of rigid bodies" in this context indicates a clear understanding of the dynamical underpinnings of relativistic kinematics. But Weyl's awareness that rigid rods and clocks are structured dynamical entities led him to the view that it is wrong to define the "metric field" in SR on the basis of their behaviour.

Weyl's concern had to do with the problem of accelerated motion, or with deviations from what he called "quasi-stationary" motion. Weyl's opinion in *Raum-Zeit-Materie* seems to have been that if a clock, say, is undergoing non-inertial motion, then it is unclear in SR whether the proper time read off by the clock is directly related to the length of its world-line determined by the Minkowski metric. For Weyl, clarification of this issue can only emerge "when we have built up a **dynamics** based on physical and mechanical laws" (1952, 177). This theme was to re-emerge in Weyl's responses to Einstein's criticisms of his 1918 attempt at a unified field theory. Before turning to this development, it is worth looking at Weyl's comments on SR.

In a sense Weyl was right. The claim that the length of a specified segment of an arbitrary time-like curve in Minkowski spacetime—obtained by integrating the Minkowski line element ds along the segment—is related to proper time rests on the assumption (now commonly dubbed the 'clock hypothesis') that the performance of the clock in question is unaffected by the acceleration it may be undergoing. It is widely appreciated that this assumption is not a consequence of Einstein's 1905 postulates. Its justification rests on the contingent dynamical requirement that the external forces accelerating the clock are small in relation to the internal 'restoring' forces at work inside the clock. (Similar considerations also hold of course in the case of rigid bodies.)

Today we are more sanguine about the clock hypothesis than Weyl seems to have been in *Raum-Zeit-Materie*. There is experimental confirmation of the hypothesis for nuclear clocks, for instance, with accelerations of the order 10^8cm/s^2. But the question remains as to whether the behaviour of rods and clocks captures the full significance of the "metric field" of SR. Suppose accelerations exist such that for no known clock is the hypothesis valid (assuming the availability of the external forces in question!). Mathematically, one can still determine—using the prescription above—the length of the time-like worldline of any clock undergoing such acceleration if it does not disintegrate completely. From the perspective of the Lorentzian pedagogy, should one say that such a number has no physical meaning in SR? We return to this issue in the next section.

Weyl's separate publication of a stunning, though doomed unification of gravitational and electromagnetic forces (Weyl 1918b) raised a number of intriguing questions about the meaning of space-time structure which arguably deserve more attention than they have received to date (see, however, Ryckman 1994). Space prevents us from giving more than a sketch of the theory and its ramifications; our emphasis will be on the role of the Lorentzian pedagogy in evaluating the theory.

Weyl started from the claim that the pseudo-Riemannian space-time geometry of Einstein's general relativity is not sufficiently local in that it allows the comparison of the lengths of distant vectors. Instead, Weyl insisted that the choice of unit of (spacetime) length at *each* point is arbitrary: only the ratios of the lengths of vectors *at the same point* and the angles between them can be physically meaningful. Such information is invariant under a *gauge transformation* of the metric field: $g_{ij} \to g'_{ij} = e^{2\lambda(x)} g_{ij}$ and constitutes a conformal geometry.

In addition to this conformal structure, Weyl postulated that spacetime is equipped with an affine connection that preserves the conformal structure under infinitesimal parallel transport. In other words, the infinitesimal parallel transport of all vectors at p to p' is to produce a similar image at p' of the vector space at p.[7] For a given choice of gauge, the constant of proportionality of this similarity mapping will be fixed. Weyl assumed that it differed infinitesimally from 1 and thereby proceeded to show that the coefficients of the affine connection depended on a one-form field ϕ_i in addition to the metric coefficients g_{ij} in such a way that the change in any length l under parallel transport from p (coordinates $\{x^i\}$) to p' (coordinates $\{x^i + dx^i\}$) is given by:

$$dl = l\phi_i dx^i. \tag{1}$$

Under the gauge transformation $g_{ij} \to g'_{ij} = e^{2\lambda} g_{ij}$, $l \to e^{\lambda} l$. Substituting this into (1) gives:

$$\phi_i \to \phi'_i = \phi_i + \lambda_{,i},$$

the familiar transformation law for the electromagnetic four-potential. Weyl thus identified the gauge-invariant, four-dimensional curl of the geometric quantity ϕ_i with the familiar electromagnetic field tensor.

For a given choice of gauge a comparison of the length of vectors at distant points can be effected by integrating (1) along a path connecting the points. This procedure will in general be path independent just if the electromagnetic field tensor vanishes everywhere.

As is well-known, despite his admiration for Weyl's theory, Einstein was soon to spot a serious difficulty with the non-integrability of length (Einstein 1918). In the case of a static gravitational field, a clock undergoing a round-trip in space during which it encountered a spatially varying electromagnetic potential

[7] It is worth noting at this point that Weyl could, and perhaps should have gone further! As the keen-eyed Einstein was to point out, it is in the spirit of Weyl's original geometric intuition to allow for the relation between tangent spaces to be a weaker affine mapping: why insist that it be a similarity mapping? Einstein made this point in a letter to Weyl in 1918. For details see Vizgin (1994, 102).

would return to its starting point ticking at a rate different to that of a second clock which had remained at the starting point and which was originally ticking at the same rate. An effect analogous to this 'second clock effect' would occur for the length of an infinitesimal rod under the same circumstances. But it is a fact of the world—and a highly fortunate one!—that the relative periods of clocks (and the relative lengths of rods) do not depend on their relative histories in this sense.

Before looking at Weyl's reply to this conundrum, it is worth remarking that it was apparently only in 1983 that the question was asked: what became of Einstein's objection once the gauge principle found its natural home in quantum mechanics? C.N. Yang pointed out that because the non-integrable scale factor in quantum mechanics relates to phase, the second clock effect could be detected using wavefunctions rather than clocks, essentially what Aharonov and Bohm had discovered (Aharonov & Bohm 1959; see also Ehrenberg & Siday 1949).[8] We note that Yang's question can be inverted: is there a full analogue of the Aharonov-Bohm effect in Weyl's gauge theory? The answer is yes, and it indicates that there was a further sting in Einstein's objection to Weyl that he and his contemporaries failed to spot. The point is that the second clock effect obtains in Weyl's theory even when the electromagnetic field vanishes everywhere on the trajectory of the clock, so long as the closed path of the clock encloses some region in which there is a non-vanishing field. This circumstance highlights the difficulty one would face in providing a dynamical or 'constructive' account of the second clock effect in the spirit of the full Lorentzian pedagogy.[9] Weyl's theory seems to be bedevilled by non-locality of a very striking kind.

The precise nature of Weyl's response to Einstein's objection would vary in the years following 1918 as he went on to develop new formulations of his unified field theory based on the gauge principle (see Vizgin 1994). But the common element was Weyl's rejection of the view that the metric field could be assigned operational significance in terms of the behaviour of rods and clocks. His initial argument was an extension of the point he made about the behaviour of clocks in SR: one cannot know how a clock will behave under accelerations and in the presence of electromagnetic fields until a full dynamical modelling of the clock under these circumstances is available. The price Weyl ultimately paid for the beauty of his gauge principle—quite apart from the complicated nature of his field equations—was the introduction of rather tentative speculations concerning a complicated dynamical adjustment of rods and clocks to the 'world curvature' so as to avoid the second clock effect and its analogue for rods.

We finish this section with a final observation on the nature of Weyl's theory,

[8] Yang (1984). Yang recounts this incident in Yang (1986, 18).

[9] It is worth noting that in 1923, Lorentz himself wrote, in relation to the rod analogue of the second clock effect in the Weyl theory, that this "would amount to an action of an electromagnetic field widely different from anything that could reasonably be expected" (Lorentz 1937, 375). But whether Lorentz was concerned with the dynamical problem of accounting for how Maxwell's electrodynamics could in principle have such an effect on physical bodies like rods—a consideration which one would not expect to be foreign to Lorentz' thinking!—or simply with the empirical fact that such an effect is non-existent, is not entirely clear from Lorentz's comments.

with an eye to issues in standard general relativity to be discussed shortly. We noted above that Weyl's connection is not a metric connection. It is a function not only of the metric and its first derivatives, but also depends on the electromagnetic gauge field: in particular, for a fixed choice of gauge, the covariant derivative of the metric does not vanish everywhere. What does this imply?

The vanishing of the covariant derivative of the metric—the condition of metric compatibility—is sometimes introduced perfunctorily in texts on general relativity, but Schrödinger was right to call it "momentous" (Schrödinger 1985, 106). It means that the local Lorentz frames associated with a space-time point p (those for which, at p, the metric tensor takes the form $\mathrm{diag}(1, -1, -1, -1)$ and the first derivatives of all its components vanish) are also local inertial frames (relative to which the components of the connection vanish at p).[10] If the laws of physics of the non-gravitational interactions are assumed to take their standard special relativistic form at p relative to such local Lorentz charts (the local validity of special relativity), then metric compatibility implies that gravity is not a force in the traditional sense—an agency causing deviation from natural motion—, in so far as the worldlines of freely falling bodies are geodesics of the connection.

The full physical implications of the non-metric compatible connection in Weyl's theory remain obscure in the absence of a full-blown theory of matter. Weyl's hints at a solution to the Einstein objection seem to involve a violation of minimal coupling, i.e. a violation of the prohibition of curvature coupling in the non-gravitational equations, and hence of the local validity of special relativity. But it seems that the familiar insight into the special nature of the gravitational interaction provided by the strong equivalence principle—the encapsulation of the considerations given in the previous paragraph—is lost in the Weyl theory.

6 The case of general relativity

There is a recurrent, Helmholtzian theme in Einstein's writings concerning Euclidean geometry: he claims that, as it is understood in physics, it is the science "des possibilités de deplacement des corps solides" (Einstein 1928), or of "the possibilities of laying out or juxtaposing solid bodies" (Einstein 1981, 163; see also Einstein 1982, 234–235.).

But consider a universe consisting of some large number of mass points interacting by way of the Newtonian gravitational potential. Few would deny that a well-defined theory of such objects can be constructed within the framework of Newtonian mechanics (or its recent Machian counterparts such as Barbour & Bertotti (1982)). In such a world, there is nothing remotely resembling rigid bodies or rulers which allow for a direct operational significance to be assigned to inter-particle distances. Yet these distances are taken to obey the algebraic relationships of Euclidean geometry; either because this is a foundational assumption of the theory (as in the Machian approach), or because this is true of

[10]See Misner *et al.* (1973, 313), Ehlers (1973, 16–20) and Stewart (1991, 57).

the particles' coordinate differences when referred to the privileged coordinate systems with respect to which the laws take on a canonical form. Moreover, the Euclidean constraint on the instantaneous configuration of the particles is formally the same as in a more ramified pre-general relativistic (quantum) theory of matter which in principle allows for non-gravitational forces as well, and hence for the possibility of the existence of stable, rigid bodies.

Einstein was not oblivious to this point. He stressed that the accepted theory of matter itself (even pre-special relativistic theory) rules out the possibility of completely rigid bodies, and that atomistic conceptions exclude "the idea of sharply and statically defined bounding surfaces of solid bodies." Einstein realised that such physics "must make use of geometry in the establishment of its concepts", concluding that "the empirical content of geometry can be stated and tested only in the framework of the whole of physics" (Einstein 1982, 163).

Rigid bodies furnish what is an already independently meaningful spatial geometry with an (approximate) direct operational significance, and the fact that they have this role is a consequence of the theory of matter. It should be noted here that a necessary condition for this state of affairs is that the dynamical equations governing the non-gravitational interactions satisfy the so-called Euclidean symmetries. But this more complicated and no doubt more correct way of looking at things surely weakens the literal reading of Einstein's original account of Euclidean geometry above as the science of the possible arrangement of rigid bodies in space. The behaviour of rigid bodies under displacements does not *define* so much as *instantiate* the spatial geometry which might even have primordial status in the foundations of the theory of such bodies. And this point leads to another observation which is of considerable relevance to this paper.

The fact that rods or rulers function as surveying devices for the primordial Euclidean geometry is not because they 'interact' with it; the latter is not a dynamical player which couples to matter. In arranging themselves in space, rigid bodies do not 'feel' the background geometry. To put it another way, a rod is not a 'thermometer of space'. Nor is it in the intrinsic nature of such bodies to survey space. It is the theory of matter which in principle tells us what kind of entities, if any, can serve as accurate geometrical surveying devices, and our best theories tell us rigid bodies will do. One only has to consider the consequences of a violation of the Euclidean symmetries in the laws of physics to dispel any doubts in this connection. (All of this is to say that 'what is a ruler?' is as important a question in physics as 'what is a clock?' An answer to this last question ultimately depends on specifying very special devices which 'tick' in synchrony with an independently meaningful temporal metric, a metric that might nonetheless be specified by the *dynamics* of the total isolated system of which the devices are a part.[11])

Turning now to the geometry of special relativity, what is of interest is the behaviour of rods and clocks in relative motion. While the Minkowski geometry does not play a primordial role in the dynamics of such entities, analogous to

[11] See, for example, Barbour (1994, sections 3, 4 & 12).

the role which might be attributed to the three-dimensional Euclidean geometry constraining relative distances, it is definable in terms of the Lorentz covariance of the fundamental dynamical laws.[12] Hence spacetime geometry is equally not simply 'the science of the possible behaviour of physical rods and clocks in motion'. All of the qualifications analogous to those we were forced to consider in the case of Euclidean geometry apply. In the context of SR, rods and clocks *are* surveying devices for a four-dimensional geometric structure. But this is a structure defined in terms of the symmetries of dynamical laws. If matter and its interactions are removed from the picture, Minkowski spacetime is not left behind.[13] Rods and clocks do not interact with a background metric field: they are not thermometers of spacetime structure.

There is a temptation to extend this lesson to general relativity (GR) in the following way. One might want to say that it is the local validity of special relativity in GR—as defined at the end of the previous section—that accounts for the existence of a metric field of Lorentzian signature whose metric connection coincides with the connection defining the inertial (free-fall) trajectories. The real new dynamics in GR has to do not with the metrical properties of spacetime but with the (generally) curved connection field and its coupling to matter. In particular, a defender of the Lorentzian pedagogy might be forgiven for accepting the maxim: no matter, no metric (without, however, excluding the connection). As we have argued, this is surely the right maxim for special relativity. In a universe entirely bereft of matter fields, even if one were to accept the primordial existence of inertial frames, it is hard to attribute any meaning in special relativity to the claim that empty spacetime retains a Minkowski metric as an element of reality, or equivalently to the claim that the inertial frames are related by Lorentz transformations. (On this point Einstein seems to have been somewhat inconsistent. It is difficult to reconcile the remarks on special relativity in his *Autobiographical Notes*, where he warns about the reduction of physics to geometry, with the claim in Appendix 5 of his *Relativity*—which he added to the fifteenth edition of the book in 1952—that with the removal of all "matter and field" from Minkowski space, this space would be left behind.[14])

Adopting this position in GR has fairly drastic consequences in the case of the vacuum (matter-free) solutions to Einstein's field equations, of which 'empty'

[12] A similar position is defended by DiSalle (1995, 326). Our analysis of Euclidean geometry, and of the role of spacetime geometry in general relativity (see below) differs, however, from DiSalle's.

[13] A more recent example in physics of an absolute geometrical structure with clear dynamical underpinnings is that of the projective Hilbert space (ray space) in quantum mechanics. A (curved) connection in this space can be defined for which the anholonomy associated with closed curves is the geometric phase of Aharonov and Anandan. This geometric phase encodes a universal (Hamiltonian-independent) feature of Schrödinger evolution around each closed path in ray space, in a manner analogous to that in which the Minkowski geometry in SR encodes the universal behaviour of ideal rods and clocks arising out of the Lorentz covariant nature of the laws of physics. For a review of geometric phase, see Anandan (1992); further comparisons with Minkowski geometry are spelt out in Anandan (1991).

[14] Actually, Einstein says that what remains behind is "inertial-space or, more accurately this space together with the associated time" , but subsequent remarks seem to indicate he meant the "rigid four-dimensional space of special relativity" (1982, 171).

Minkowski spacetime itself is one solution. It entails that while the *flatness* of spacetime in this case (and the curvature in other less trivial solutions)—essentially an affine notion—may be said to have physical meaning, the *metric* structure of the spacetime does not (Brown 1997). (The metric might retain a meaning if one could adopt Feynman's 1963 suggestion that such vacuum solutions are correctly viewed as obtained by taking solutions involving sources and matter-free regions and allowing these regions to become infinitely large (Feynman *et al.* 1995, 133–134). Feynman's view was that gravitational fields without sources are probably unphysical, akin to the popular view that all electrodynamical radiation comes from charged sources. Now the analogy with electromagnetism is arguably not entirely happy given that gravity is itself a source of gravity. Moreover, such an interpretation seems clearly inapplicable to finite vacuum spacetimes. Be that as it may, Feynman seemed to be happy with the flat Minkowski vacuum solution, perhaps because he could not entirely rid himself of his alternative 'Venutian' view of gravity as a massless spin-2 field on an absolute Minkowski background.)

But the temptation to take the Lorentzian pedagogy this far should perhaps be resisted. It overlooks the simple fact that the metric field in GR (defined up to the diffeomorphic 'gauge' freedom) appears to be a *bona fide* dynamical player, on a par with, say, the electromagnetic field. Even if one accepts—possibly either for Machian reasons (see Barbour 1994) or with a view to quantum gravity—the four-metric as less fundamental than the evolving curved three-metrics of the Hamiltonian approach to GR, it is nonetheless surely coherent to attribute a metric field to spacetime whether the latter boasts matter fields or not. If absolute Euclidean distances can exist in Newtonian universes bereft of rigid bodies, so much more can the dynamical metric field in GR have claim to existence, even in the non-generic case of the universal vacuum. Any alternative interpretation of the metric field in GR would seem to require an account of the coupling of a connection field to matter which was not mediated by the metric field as it is in Einstein's field equations. We know of no reason to be optimistic that this can be achieved.

Where does this leave the role of the Lorentzian pedagogy in GR? In our opinion, it still plays a fundamental role in understanding in dynamical detail how rods and clocks survey the metric field. To see this, let us consider the following claim made by Torretti, in his magnificent 1983 foundational text on *Relativity and Geometry*. Torretti formulates the basic assumption of GR as:

> The phenomena of gravitation and inertia ... are to be accounted for by a symmetric $(0,2)$ tensor field $\mathbf{g}$ on the spacetime manifold, which is linked to the distribution of matter by the Einstein field equations and defines a Minkowski inner product on each tangent space. (1983, 240)

It follows immediately from this hypothesis that the Minkowski inner product on tangent spaces induces a local approximate Minkowski geometry on a small neighbourhood of each event. Torretti claims that one can thereby "account for the Lorentz invariance of the laws of nature referred to local Lorentz charts." The successes of special relativity follow, says Torretti, from this local Minkowski geometry (1983, 240).

In our view, this claim is a *non sequitur*. It is mysterious to us how the existence of a local approximate Minkowski geometry entails the Lorentz covariance of the laws of the non-gravitational interactions. Theories postulating a Lorentzian metric but which violate minimal coupling would involve non Lorentz covariant laws. Equally, the primordial Euclidean geometry in the Newtonian theory of mass points discussed at the start of this section does not entail that the corresponding 'laws of nature' (if there are any non-gravitational interactions in the theory!) satisfy the Euclidean symmetries. There is something missing in Torretti's account, and this problem reminds one to some degree of the plea in some of Grünbaum's writings for an account of why the **g** field has the operational significance that it does. (A critical analysis of Grünbaum's arguments, with detailed references, is given by Torretti (1983, 242–247).)

It seems to us that the local validity of special relativity in GR cannot be derived from what Torretti takes to be the central hypothesis of GR above, but must be independently assumed. Indeed, it often appears in texts as part of the strong equivalence principle, taken as a postulate of GR (for example, Misner *et al.* 1973, 386). The assumption, which is intimately related to the postulate of minimal coupling in GR, is that relative to the local Lorentz frames, insofar as the effects of curvature (tidal forces) can be ignored, the laws for the non-gravitational interactions take their familiar special relativistic form; in particular, the laws are Lorentz covariant. It is here of course that the full Lorentzian pedagogy can in principle be used to infer the possibility of the existence of material devices which more or less accurately survey the local Minkowski geometry. In particular, it explains why ideal clocks, which are chosen initially on dynamical grounds, act as hodometers, or 'way-wisers' of local Minkowski spacetime, and hence measure the lengths of time-like curves over the extended regions in curved spacetime that they traverse. (A very clear account of how the 'hence' in this statement was probably first understood by Einstein is given by Torretti (1983, 149–151). Elsewhere, Torretti (1983, 312, footnote 13) notes that as early as 1913 Einstein recognised that the operational significance of the **g** field, and in particular the significance of the null cones in the tangent spaces, required the "separate postulate" of the local validity of special relativity. It seems from the above that Torretti did not wish to follow Einstein in this respect.)

To conclude, the fact that general relativistic spacetimes are locally Minkowskian only acquires its usual 'chronometric' operational significance because of the independent assumption concerning the local validity of special relativity. Our main claim in this section is that this point can only be understood correctly by an appeal to the Lorentzian pedagogy. Despite the fact that in GR one is led to attribute an independent real existence to the metric field, the general relativistic explanation of length contraction and time dilation is simply the dynamical one we have urged in the context of special relativity.

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