Minkowski's Proper Time and the Status of the Clock Hypothesis[1]

[1] I am grateful to Stephen Lyle, Vesselin Petkov, and Graham Nerlich for their comments on the penultimate draft; any remaining infelicities or confusions are mine alone.

Discussion of Hermann Minkowski's mathematical reformulation of Einstein's Special Theory of Relativity as a four-dimensional theory usually centres on the ontology of spacetime as a whole, on whether his *hypothesis of the absolute world* is an original contribution showing that spacetime is the fundamental entity, or whether his whole reformulation is a mere mathematical *compendium loquendi*. I shall not be adding to that debate here. Instead what I wish to contend is that Minkowski's most profound and original contribution in his classic paper of 100 years ago lies in his introduction or discovery of the notion of *proper time*.[2] This, I argue, is a physical quantity that neither Einstein nor anyone else before him had anticipated, and whose significance and novelty, extending beyond the confines of the special theory, has become appreciated only gradually and incompletely. A sign of this is the persistence of several confusions surrounding the concept, especially in matters relating to *acceleration*. In this paper I attempt to untangle these confusions and clarify the importance of Minkowski's profound contribution to the ontology of modern physics. I shall be looking at three such matters in this paper:

1. The conflation of proper time with the time co-ordinate as measured in a system's own rest frame (*proper frame*), and the analogy with *proper length*.
2. Misconceptions that Special Relativity (SR) applies only to objects in inertial motion, and *not to accelerated systems*, and that therefore one must introduce General Relativity to solve Langevin's Twin Paradox.
3. Misconceptions surrounding the status of the so-called *clock hypothesis* (CH), according to which the instantaneous rate of a clock depends only on its instantaneous speed, and not on its acceleration. I shall argue that the CH is a criterion for ideal clocks that is implicit in SR, and does not have the status of an independent assumption; and that it also performs this role in GR as a consequence of the strong equivalence principle.

1. Proper Time, Local Time and Proper Length

The first symptom of this under-appreciation of the novelty of proper time I wish to discuss is that it is often taken to be the time co-ordinate measured by a clock at rest in an inertial frame: in its own frame, therefore, *proper*, as opposed to *local time*. 'Local time', of course, was originally Lorentz's term for the transformed co-ordinate time t' of an inertial frame of reference moving with velocity v with respect to the stationary frame. By Lorentz's own admission, the chief cause of his failure to discover Special

[2] Here I concur with Roberto Torretti, who rates Minkowski's introduction of proper time as "probably his most important contribution to physics" (1983, p. 96).

Relativity "was my clinging to the idea that only the variable *t* can be considered as the true time, and that my local time *t´* must be regarded as no more than an auxiliary mathematical quantity".[3] Einstein's correction lay in seeing that all of these time co-ordinates or local times are on a par and equally entitled to be regarded as the true time. It is perhaps for these reasons that the illusion arose that proper time is simply the local time of the system's own rest frame, the time co-ordinate as measured in an inertial frame in which the system is at rest. And this in turn could explain why it is not uncommon to find the introduction of proper time attributed to Einstein, even by authors with a keen sense of the history of physics.[4]

The conflation of proper time with co-ordinate time in a system's own rest frame is also perhaps fostered by the numerical equivalence of the value of the proper time elapsed for a body moving along an inertial path with the value measured by the time-co-ordinate in its rest frame. Thus it is often said that proper time is simply time measured in a body's "proper frame", *as if a body keeps its own inertial frame while accelerating!* There are two confusions here: first, the idea that a body "has" an inertial frame, when a reference frame is just a point of view for representing the body's motion, and (according to the principle of relativity) one can represent this motion equivalently from *any* inertial frame; and second, of course, the idea that the body could stay in the same inertial frame even though it is accelerating, and therefore moving *non-inertially*. At any rate, this is a confused idea of proper time, which is *not* a time co-ordinate and was not introduced by Einstein, but by Minkowski, in his famous paper of 1908 (Lorentz et al. 1923, 73-91). Introducing the concept, he asks us to imagine at any point P (*x*, *y*, *z*, *t*) in spacetime a worldline running through that point, so that the magnitude corresponding to the timelike vector *dx*, *dy*, *dx*, *dt* laid off along the line is

$$d\tau = \sqrt{}(c^2 dt^2 - dx^2 - dy^2 - dz^2)/c \qquad (1)$$

Proper time is now defined as the integral of this quantity along the world line in question: "The integral $\tau = \int d\tau$ of this quantity, taken along the worldline from any fixed starting point P_0 to the variable endpoint P, we call the *proper time* of the substantial point at P." (85) Thus for Minkowski spacetime, the proper time is:

$$\tau = \int_P d\tau = \int_P \sqrt{}(c^2 dt^2 - dx^2 - dy^2 - dz^2)/c \qquad (2)$$

or, equivalently,

[3] Quoted from (Torretti 1983, p.).
[4] See for example J.-P. Provost, C. Bracco and B. Raffaelli 2007, p. 498: "If one starts with SR, …the first important notion, as Einstein told us in 1905, is the proper time τ".

$$\tau = \int_{P_0}^{P} \{1 - 1/c^2\,[(dx/dt)^2 + (dy/dt)^2 + (dz/dt)^2]\}^{1/2}\, dt \tag{3}$$

As defined, the proper time cannot be evaluated without adopting a system of co-ordinates. But because of the signature of the Minkowski metric, the interval $d\tau$ and its integral τ are both *invariants*, so that their values are independent of what system of co-ordinates is adopted. Thus for the time elapsed along any worldline, τ gives a measure that is *independent of the co-ordinates*, even if a particular frame must be adopted in order to calculate its value. The whole content of relativity theory can now be framed in terms of such invariants, so that co-ordinates are no longer regarded as primitive, as they had been in Einstein's way of conceiving SR. For instance, as Minkowski proceeded to explain, x, y, z and t —the components of the vector OP, where O is the origin— are considered as functions of the proper time τ, and the first derivative of the components of this vector with respect to the proper time, $dx/d\tau$, $dy/d\tau$, $dz/d\tau$ and $dt/d\tau$, are those of the *velocity vector* at P, which is also a four-dimensional invariant. As is well known, the resulting four-dimensional co-ordinate-free rendering of special relativity is of immense utility for the further development of relativity theory, even if Einstein did not at first appreciate its significance.

The misidentification of proper time as the time co-ordinate in its rest frame is also encouraged by an analogy with *proper length*, which is the length of a body in its rest frame. The analogy, it is often claimed, is *perfect*, and the invariance of proper time is no objection. For just as the length of a path joining two events in timelike separation is invariant under change of frame, so is the length of a curve joining two events in spacelike separation.[5] This can be seen by comparing the expressions for proper time and proper length written in the tensor form necessary for general relativistic spacetimes. Here the appropriate generalization of the expression given by Minkowski for his flat spacetime to curved spacetimes is[6]

$$\tau = \int_P d\tau = \int_P (-g_{\alpha\beta}\, dx^\alpha\, dx^\beta)^{1/2} \tag{4}$$

The flat-space expression for the proper length should be generalized so that it is the exact analogue of proper time, a line integral along a curve joining two spacelike separated events:

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[5] Cf. the article on proper length in Wikipedia (http://en.wikipedia.org/wiki/Proper_length: March 24, 2009). The author suggests a generalization of proper length so that (in either Special or General Relativity) it is given by the line integral $L = c \int_P \sqrt{[-g_{\mu\nu}\, dx^\mu\, dx^\nu]}$, where $g_{\mu\nu}$ is the metric tensor for the spacetime with +--- signature, normalized to return a time, and P is the spacelike path. The author also notes that "Proper length has also been used in a more restricted sense to help with discussions of length contraction by textbooks, where it is defined as the length of an object when measured by someone at rest relative to that object."

[6] Cf. Misner, Thorne and Wheeler, 1973, 393.

$$L = \int_P ds = c \int_P (g_{\alpha\beta}\, dx^\alpha\, dx^\beta)^{1/2} \qquad (5)$$

There is no doubt that this defines a proper distance, that between the endpoints of a path in spacetime along which no process can travel, a spacelike curve. An arbitrary curve joining two spacelike separated events, however, is *not generally the length of an object*. It can only be the length of an object if all the points on the curve are simultaneous in some given reference frame; for an object, such as a body or wave front, is a three dimensional object existing at a given time. And while the path integral along such an arbitrary curve is indeed the length of a path, and is independent of the choice of reference frame, it has no particular physical significance. Proper length is correctly defined as the path integral, *not along an arbitrary curve* joining the endpoints of the path at the same time, but *along the shortest curve*, which is a straight line joining them in the frame at which they are at rest. If (elapsed) proper time were the strict analogue of this, it would be the longest time between two timelike separated events, which would be the time in a frame of reference at rest, i.e. the co-ordinate time in a body's rest frame. It is precisely this interpretation that I am contesting: proper time, according to Minkowski's definition above, *is not a co-ordinate time*; and it is not defined for only the shortest path, i.e. only within the body's rest frame, but is defined *for any timelike curve in spacetime*. Because proper length is the interval between two events *at the same co-ordinate time*, it is specific to a particular reference frame. Proper time, as defined by Minkowski, is not.

Thus *proper time* has a fundamentally different character from proper length. Although both are invariant under change of frame, proper length is the length of an object in its own rest frame, whereas proper time is independent of frame. In this respect proper length is analogous to proper mass. (It differs from the latter, however, in that proper mass seems to be an essential characteristic of an elementary body (such as an electron), whereas proper length is a contingent one.) At any rate, there is a fundamental dissymmetry between duration and length in Special Relativity, somewhat obscured by talk of their embodiments in observers' clocks and rods. For whereas an observer's clock measures proper time elapsed along a path, a dynamical variable specifiable independently of reference frame, the proper length of the observer's measuring rod is specific to the inertial frame in which the observer is at rest. Thus we see that, ironically, there is a sense in which Minkowski's introduction of proper time undermines his famous pronouncement at the beginning of his paper about the demise of time:

> Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality. (Minkowski 1908, 75)[7]

2. Acceleration and Appeals to General Relativity

Relatedly, it is often stated that Special Relativity (SR) applies only to objects in inertial motion, and not to accelerated systems, for which an appeal to General Relativity (GR) is necessary. This one finds especially in treatments of the Twin Paradox (a.k.a. the Clock Paradox), where it is claimed that a proper resolution of the paradox must therefore involve General Relativity.[8] In this connection one finds references to Einstein's "Dialogue on Objections to the Theory of Relativity" (1918), which is interpreted as having shown how to solve the paradox using General Relativity, through an application of the Equivalence Principle (EP). On this misreading it is thought that since the acceleration of the travelling twin is responsible for the difference in the twins' ages (i.e. in the proper times of their journeys), and accelerations fall outside the scope of the special theory, and since (by the EP) this acceleration will be equivalent to a *gravitational time dilation*, the paradox must receive its explanation in GR.

On the contrary, the paradox receives a complete explanation within SR, which is perfectly applicable to accelerated motions.[9] This was already made clear by Arnold Sommerfeld in a very succinct note on Minkowski's paper of 1908, published in (Lorentz *et al*. 1913), with an English translation appearing in 1923 and again in 1952:

> [T]he element of proper time $d\tau$ is not a complete differential. Thus if we connect two world-points O and P by two different world-lines 1 and 2, then
>
> $$\int_1 d\tau \neq \int_2 d\tau$$
>
> If 1 runs parallel to the t-axis, so that the first transition in the chosen system of reference signifies rest, it is evident that

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[7] This famous pronouncement of Minkowski's is echoed by Einstein in his essay "The Problem of Space, Ether and the Field in Physics": "Hitherto it had been silently assumed that the four-dimensional continuum of events could be split up into time and space in an objective manner… With the discovery of the relativity of simultaneity, space and time were merged in a single continuum … " (1954, 281-82).

[8] Cf. this analysis on the *Encyclopedia Britannica* internet site: "The answer is that the paradox is only apparent, for the situation is not appropriately treated by special relativity. To return to Earth, the spacecraft must change direction, which violates the condition of steady straight-line motion central to special relativity. A full treatment requires general relativity, which shows that there would be an asymmetrical change in time between the two sisters. Thus, the "paradox" does not cast doubt on how special relativity describes time, which has been confirmed by numerous experiments." http://qa.britannica.com/eb/article-252886. The number of similar confusions on individual physicists' web sites seems to have decreased sharply of late; but there is no excuse for anyone who has read Misner, Thorne and Wheeler (1973): they have a separate section (6.1) titled "Accelerated Observers Can be Analyzed Using Special Relativity" (p. 163).

[9] See my thorough treatment of the twin paradox in SR in my (2008).

$$\int_1 d\tau = t, \quad \int_2 d\tau < t$$

> On this depends the retardation of the moving clock compared with the clock at rest. (Lorentz *et al*. 1913, 71, and 1952, 94)

As for Einstein's 1918 paper, there Einstein is concerned to defend the consistency of the explanation of time dilation given in SR, and to show its compatibility with an equivalent explanation in GR. He argues that the differences in the time of the two journeys is an "inevitable result" of the special theory of relativity. In his version, there are two identical clocks, U_1 and U_2, and U_2 is accelerated until it reaches a velocity $-v$ relative to U_1, travels with this velocity for a while, and is then decelerated until its motion is reversed, moving with a velocity v back to rejoin U_1. U_2 will then be retarded with respect to U_1 by an amount $-\Delta t$. To the objection that all motion is relative, he rejoins that in SR this is so only for systems in mutual relative unaccelerated motion. But since here the clock U_2 is accelerated, no contradiction to SR is forthcoming. The clock U_2 will be running behind U_1: as in Sommerfeld's argument above, $\int_2 d\tau = \int_1 d\tau - \Delta t < \int_1 d\tau$.

But his imaginary opponent objects that in General Relativity one is not constrained to take only reference frames in inertial motion: one could adopt a co-ordinate frame co-moving with the accelerated clock. Einstein responds by applying the Equivalence Principle to show how things would appear from that frame. Now a static homogeneous gravitational field appears, and clock U_1 undergoes acceleration in free fall up to a velocity v (at which point the field disappears), while the clock U_2 is prevented from moving by the action of an external force. U_1 then travels with this velocity for a while, until a static homogeneous gravitational field appears, directed in the opposite direction to before, in which it decelerates in free fall until its motion is reversed, moving with a velocity $-v$ back to rejoin U_2, which had remained at rest. Einstein explains that although the clock will be retarded while undergoing the inertial legs of its journey, resulting in U_1 being retarded by the same amount $-\Delta t$ as was U_2 in the previous scenario, when it is free-falling in the third leg of its journey it is located at a higher gravitational potential than U_2, and, as a consequence of general relativistic time dilation, it will be speeded up by exactly $2\Delta t$, thus completely disposing of the paradox.

The moral of the story is that time dilation in SR arises from the fact the twins *trace different paths through spacetime* because of the different accelerations they undergo, they but it is *not a direct effect of the accelerating motion itself*. In the idealized scenario of the twin paradox, the time dilation due to the acceleration of the travelling twin is not normally considered, but it could be made arbitrarily small compared to that produced by the inertial motions. The time dilation in the second scenario considered by Einstein is produced not by the acceleration, but by the difference in gravitational potential at two points in the field. Thus in the SR case, it is the difference in the paths that results in a time dilation for the accelerated twin; and analogously in the GR case, the compensating gravitational time dilation is due to the difference in gravitational potential at two points in the field rather than being an effect of the accelerating motion itself. This is what Einstein showed in 1918.[10]

3. Acceleration and the Clock Hypothesis

A much more subtle set of issues surrounds the "clock hypothesis", for which the previous two sections set the scene. Wolfgang Rindler renders it as follows:

> If an *ideal* clock moves non-uniformly through an inertial frame, we shall *assume* that acceleration as such has no effect on the rate of the clock, i.e. its instantaneous rate depends only on its instantaneous speed *v* according to the above rule. This we call the *clock hypothesis*. It can also be regarded as the definition of an "ideal" clock. (Rindler 1977, 43).

What Rindler refers to here as "the above rule" is the standard formula for time dilation in Special Relativity, representing the time interval T recorded by a clock in an inertial frame S in terms of the time interval T_o recorded by a clock at rest in a frame S moving inertially with speed v:

$$T = \gamma\, T_o \;= T_o\,/(1 - v^2/c^2)^{1/2} \qquad (6)$$

This, however, is not a good definition of the clock hypothesis, since here T and T_o are both coordinate times, so that strictly speaking formula (6) only applies to inertial motions, not accelerated ones. Thus a better definition is to say that an ideal clock is one that measures proper time as given by formula (2) above:

$$\tau = \int_P d\tau \;=\; \int_P \sqrt{(c^2dt^2 - dx^2 - dy^2 - dz^2)}/c \qquad (2)$$

or in the generalized form also appropriate to General Relativity that we gave above,

$$\tau = \int_P d\tau \;=\; \int_P (-g_{\alpha\beta}\, dx^\alpha\, dx^\beta)^{1/2} \qquad (4)$$

On the necessity and status of this hypothesis, opinion is divided. Rindler claims it is an assumption that it is necessary to make in order to get from "purely kinematic laws about acceleration" to the dynamics of really accelerated systems (1966, 28) and Harvey Brown claims something similar in his recent book (Brown 2005, 9). Brown adds that it is the clock hypothesis that "allows for the

[10] An updated treatment of Einstein's solution is given by Jones and Wanex (2006), who demonstrate that the SR and GR paradox solutions are identical for finite accelerations (as well as the infinite ones shown by Møller (1955)), by using the destination distance as the key observable parameter.

identification of the integration of the metric along an arbitrary time-like curve—not just a geodesic—with the proper time. This hypothesis is no less required in general relativity than it is in the special theory." (Brown 2005, 9)

On the other hand, Jim Hartle holds that Minkowski's formula for the proper time holds "even for accelerating clocks, i.e., when the velocity is dependent on the time" (Hartle 2003, 62), and he makes no use of the clock hypothesis in his textbook. Roberto Torretti allows that the clock hypothesis "may be viewed as a conventional definition of what we mean by clock accuracy, and hence by physical time" (1996, 96), but argues that "Special Relativity would doubtless have been rejected or, at any rate, deeply modified, if the clock hypothesis were not fulfilled—to a satisfactory approximation—by the timepieces actually used in physical laboratories." And according to Misner, Thorne and Wheeler, "one *defines an 'ideal'… clock* to be one which measures … proper time as given by $(-g_{\alpha\beta}\, dx^{\alpha}\, dx^{\beta})^{1/2}$…" (1973, 393).

On these latter views, provided a given process approximates well enough an ideal clock, the clock hypothesis seems to amount to little more than the desideratum that, with the metric locally Minkowskian, the predictions of SR should agree with experimental fact. So the question is, why should it be necessary to state it as an independent hypothesis? Two main sets of considerations have been adduced. As we have seen, one kind of justification has been that, since many natural clocks are subject to accelerations which result in their failing to satisfy the clock hypothesis, we need to appeal to the hypothesis in passing from the kinematics of acceleration of ideal clocks to the dynamics of really moving clocks. A second kind of justification has to do with the different status of Special Relativity within the General Relativistic context, where, as is shown by the example of Weyl's unified theory of electromagnetism and gravitation, the metric could be locally Minkowskian and yet the rate of clocks could be path-dependent in such a way that the instantaneous rate would depend on the way the clock had been accelerated hitherto, contrary to the clock hypothesis. This, it is argued, proves the independence of the clock hypothesis from the assumption that spacetime is locally Minkowskian.

Let's look at the question of ideal clocks first. Rindler stresses that it is not the case that any natural process serving as a clock will meet the condition stated in the clock hypothesis. He gives the example of "a spring-driven pendulum clock whose bob is connected by two coiled springs to the sides of the case (so that it works without gravity)" (43), pointing out that it "will clearly increase its rate as it is accelerated upward". Rindler allows that "certain natural clocks (vibrating atoms, decaying muons,) conform very accurately to the clock hypothesis", and observes that in general "this will happen if the clock's internal driving forces greatly exceed the accelerating force" (43). Similarly, Harvey Brown states that the "key issue is the comparison of the magnitude of the external force producing the acceleration and that of the forces at work in the internal mechanism of the clock." (Brown 2005, 95) As he points out, "an important part of the history of time has been the search for accurate clocks which withstand buffeting" (94). Exemplary in this regard were the wonderful timepieces constructed by John Harrison in the eighteenth century in an effort to win the Admiralty Prize for a clock accurate enough for use in determining longitude on board ship (see Sobel 1995 for an engaging account). But Brown's discussion of this issue is given in the course of an inquiry into what happens when clocks are no longer moving inertially (94). He concludes that the justification of the clock hypothesis "rests on accelerative forces being small in the appropriate sense", i.e. if they are "small in relation to the internal restorative forces of the clock" (95). The "effect of motion on the clock depends accumulatively only on its instantaneous speed, not its acceleration" (95), provided the accelerative force is small in comparison with the restorative forces. Similarly, Brown and Pooley write

> 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. (Brown and Pooley 2001, 264-5)

But it seems to me that this way of describing the situation conflates two distinct issues: the clock hypothesis as a criterion for an ideal clock, an ideal clock being a clock that will keep proper time; and the separate problem of whether the restorative acceleration of the mechanism within any real system acting as a clock is sufficiently great (relative to the acceleration undergone by the system) that the system will be able to approximate such an ideal clock.[11] As Misner, Thorne and Wheeler write (2001, 393), once one has defined an ideal clock, "one must then determine the accuracy to which a given … clock is ideal by using the laws of physics to analyze its behavior". So, I maintain (*contra* Brown), it is not the justification of the clock hypothesis that depends on the accelerative force being small in

[11] There is an additional problem with Brown and Pooley's formulation, in that it assumes some kind of gap between proper time and the integral of the line-element *ds* along the segment; but proper time was *defined* as that by Minkowski. So the authors are using "proper time" in a different sense, as the time read by a real clock in its own rest frame. But the issue of whether an actual clock will read proper time is not the same issue as whether an ideal one will.

comparison with the restorative forces, but the justification of whether a given naturally occurring periodic process approximates sufficiently well the behaviour of an ideal clock.

As an example, consider the case discussed by Misner, Thorne and Wheeler (393) of an atomic clock accelerated to 2*g* in an airliner (with the airliner not in free fall, and accelerating to avoid a mid-air collision). As they explain, in order to determine empirically whether the clock "will still measure proper time $d\tau = (-g_{\alpha\beta}\, dx^{\alpha}\, dx^{\beta})^{1/2}$ along its world line to nearly the same accuracy as if it were freely falling"—i.e. to determine whether it approximates an ideal clock—"one can analyze the clock in its own 'proper reference frame' (§13.6 [pp. 327-332]), with Fermi-Walker transported basis vectors, using the standard local Lorentz laws of quantum mechanics as adapted to accelerated frames (local Lorentz laws plus an 'inertial force', which can be treated as due to a potential with a uniform gradient)" (393). In the same vein they give a proof that a pendulum clock at rest on the Earth's surface is ideal (394-5).

This distinction between an ideal clock's being implicitly determined by theory, and a real world time-piece's time-keeping qualities being determined by how well it will be able to conform to such an ideal clock, is not something new. Newton's idea was that the equability of absolute time was a direct correlate of equable motions, the paradigm for which was an inertially moving body marking off equal lengths.[12] Thus an inertially moving body is an ideal clock in Newtonian physics.[13] But as Newton himself conceded, it is quite conceivable that no actual body in the world moves perfectly equably: "It is possible that there is no uniform motion by which time may have an exact measure." (1999, 410) But this would not preclude the calculation of forces on the presupposition of this exact relation between uniform motion and time. Now, of course, if a clock is subject to too violent accelerations, it will cease to function as an accurate clock. Brown quotes Eddington to this effect: "We may force it into the track by continually hitting it, but that may not be good for its time-keeping qualities!" (Eddington, quoted in Brown 2005, 94). But as Misner *et al*. remark (2003, 393), whether it is pushed beyond the point where it can still keep good time "depends entirely on the construction of the clock—and not at all on any 'universal influence of acceleration on the march of time.' Velocity produces universal time dilation; acceleration does not."

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[12] After writing this sentence, I discovered almost exactly the same sentiment expressed by Torretti: "The First Law of Motion provides the paradigm of a physical process that keeps Newtonian time, and this is enough to ensure that the latter concept is physically meaningful, even if no such process can ever be exactly carried out in the world. … The flow of Newtonian time can therefore be read directly from the distance marks the body passes by as it moves along the ruler." (1983, 12).

[13] For a discussion of this aspect of Newton's absolute time, see Arthur 1995, 2007; Barbour 1989.

Minkowski's Proper Time and the Clock Hypothesis

Now let me turn to the claim that the clock hypothesis has a role in the transition from kinematical to dynamical considerations. Wolfgang Rindler, after defining the clock hypothesis in his (1960), writes: "SR has no machinery to *prove* any but purely kinematic laws about acceleration. All other such laws it can merely subject to the test of invariance, which requires that a physical law shall have the same form in all inertial frames." (1960, 28-29) This is a puzzling claim. As we saw above, Special Relativity can certainly be *applied* to accelerating bodies, whatever force is the source of the acceleration. Rindler appears to mean that SR, in the form of the requirement of Lorentz covariance, is only a constraint on the formulation of dynamical laws. But in what sense does that make it "purely kinematic"? By way of justification he asks us to consider a standard clock being moved along an arbitrary *n*-sided polygonal path with uniform velocities along the sides and instantaneous velocity changes at the vertices. Then according to formula (6) above, "the total time increment indicated by the clock will be

$$T = \sum_{i=1}^{n} (1 - v_i^{\,2}/c^2)^{1/2}\, \Delta t_i \qquad (7)$$

where v_i and Δt_i refer to the *i*th side and are measured in the reference frame." (29) He claims that this merely *suggests* taking the limit as $n \to \infty$ to get the law for a completely arbitrary motion between times t_i and t_2:

$$T = \int_{t1}^{t2} \sqrt{(1 - v^2/c^2)}\, dt \qquad (8)$$

Rindler notes that this is "consistent with the relativistic demand for invariance", since (8) is equivalent to formula (2) above. But, he claims, (8) is not a unique generalization of (7), in that "more complicated laws could easily be devised which incorporate an acceleration effect and which are also invariant and reduce to [(7)] in the polygonal case" (29). Thus "it is idle to pretend that the polygonal and continuous paths ultimately become equivalent in all physical respects":

> Consider, for example, a short thin tube placed perpendicularly across the path and containing a ball at the intersection. If this tube is moved transversely over the continuous path the ball will be displaced by centrifugal forces, but this will not happen on the corresponding polygonal path no matter how large its number of sides. (29)

Thus, according to Rindler, at no point in the polygonal path is there a force on the ball, but in the curved path it experiences a centrifugal force.

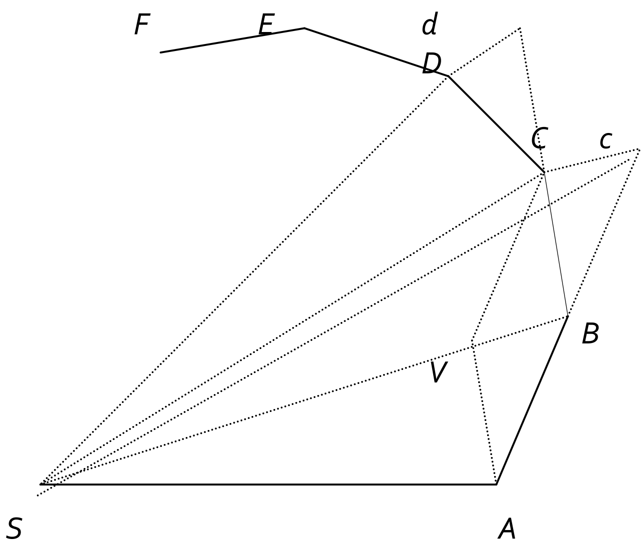
But this is a most unfortunate example. For the above polygonal construction is precisely that used by Isaac Newton in giving one of the very first derivations of the formula for centrifugal force![14] The success of such derivations, I contend, wholly undermines the distinction Rindler is trying to make here between a "kinematical" approach to acceleration, and one that is properly dynamical. Of course, the point is that in the polygonal model one assumes that the body (in Rindler's example, the tube containing the ball) is deflected instantaneously and discontinuously at each vertex by a discrete impulse $m\Delta v$ acting toward the centre in such a way as to produce a new inertial motion $m(v + \Delta v)$ between that vertex and the next (by application of the parallelogram law). The ball in Rindler's tube will experience each successive impulse. When one takes the limit as $n \rightarrow \infty$ and $\Delta t_i \rightarrow 0$, the successive increments of velocity effectively become elements of velocity dv directed towards the centre during an interval dt, with the result that one has effectively integrated from first principles to obtain an expression for the acceleration towards the centre, dv/dt. Newton used precisely this procedure in his masterwork, the *Principia*, to derive Kepler's Area Law:[15]

> Let the time be divided into equal parts, and in the first part of the time let a body by its inherent force describe the straight line *AB*. In the second part of the time, if nothing hindered it, this body would (by law 1) go straight on to *c*, describing line *Bc* equal to *AB*, so that —when radii *AS*, *BS* and *cS* are drawn to the centre— the equal areas *ASB* and *BSc* would be described. But when the body comes to *B*, let a centripetal force act with a single but great impulse and make the body deviate from the straight line *Bc* and proceed in the straight line *BC*...

[14] Newton's derivation is given in the Waste Book of 1666 (ULC MS Add. 4004), transcribed in Herivel 1965, 128-131. For an explication, see Brackenridge 1995, 45-51.

[15] As I. B. Cohen mentions in his introduction to the *Principia* (Newton 1999, 71), "the issue of the mathematical rigour of Newton's polygonal analysis has been, and still remains, a subject of debate among scholars." Recent analyses include Nauenberg 1998, Pourciau 2003, and Arthur 2009.

Figure 1



> Now let the number of triangles be increased and their width decreased indefinitely, and their ultimate perimeter *ADF* will (by lem. 3, corol. 4) be a curved line; and thus the centripetal force by which the body is continually drawn back from the tangent of this curve will act uninterruptedly, while any areas described, *SADS* and *SAFS*, which are always proportional to the times of description, will be proportional to the times in this case. *Q.E.D*. (Newton 1999, 445)

The case is the same in Special Relativity. One can begin, as Rindler chooses to do, with the time dilation formula (6) for a time interval in one inertial frame relative to another, and consider a polygonal "orbit" created by successive impulses acting on a body. In the limit, one recovers Minkowski's formula for the proper time, (2). Or one can begin with the differential form for proper time (1), and simply integrate along the path to obtain (2), as did Minkowski. As Torretti expresses it, "The clock hypothesis implies that the time measured by our clock between any two events *P* and *Q* is none other than the proper time along the clock's worldline from *P* to *Q*." (1983, 96). Thus, I conclude, it is no more necessary to postulate the clock hypothesis as a separate assumption pertaining to dynamics than it is in classical (Galilean invariant) mechanics: in SR, it is simply the condition that defines an ideal clock. I should add: this does not mean that the CH is true by definition: it means that *if a real clock does not perform as an ideal clock*, even though the theory predicts that it should (its restorative force greatly exceeding the accelerative force etc.), then—provided there isn't some unsuspected force in operation, or similar exculpatory explanation—*there must be something wrong with the theory*.

In passing I note that, in their insistence that the clock hypothesis is a separate assumption in SR, Rindler and Brown could appeal to the authority of Einstein. For in the notes he made on Minkowski's original "*Raum und Zeit*" paper, Arnold Sommerfeld attributes a remark to Einstein that may indicate the origin of the idea that the clock hypothesis is needed—at any rate it is the first statement of it that I have been able to find. Right after mentioning Minkowski's remark that $d\tau$ is not a complete differential, and noting that this shows that the proper times of two motions connecting 2 world points will generally differ (as discussed in section 2 above), Sommerfeld adds:

> This assertion is based, as Einstein has stressed, on the (unprovable) assumption that the moving clock actually indicates the proper time, i.e. that at each instant it gives the time that corresponds to the instantaneous state of velocity, regarded as constant. The moving clock must naturally have been moved with acceleration (with changes of velocity or direction) in order to be compared with the stationary clock at the world-point *P*.[16]

This remark appears to me to indicate that Einstein is here equating "proper time" with "time in an inertial frame", very much as Rindler did in his discussion of the clock hypothesis in SR discussed above. The fact that there is an instantaneous velocity at each instant, which is the velocity with which the body would continue its motion if (counterfactually) no force were acting on it, does not entail that there is no acceleration, any more than the fact that Zeno's moving arrow does not move in each instant of its motion entails that the arrow is not really moving. To reiterate, in the context of the global Minkowski spacetime of SR, the fact that an ideal clock indicates proper time follows straightforwardly, and is not a separate "unprovable assumption".

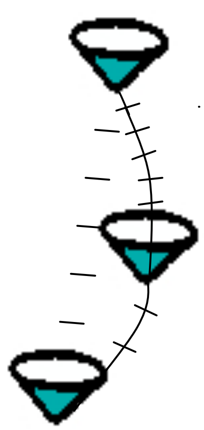
But what of Rindler's claim that "more complicated laws could easily be devised which incorporate an acceleration effect and which are also invariant and reduce to [(7)] in the polygonal case" (29)"? I cannot see how this can be done for classical mechanics or SR. In the polygonal model, the times are proportional to the lengths between the vertices for inertial motions, and the impulses are assumed instantaneous, so that the time increments will still add linearly; and this will apply even in the limit. Newton's construction, indeed, is perfectly general, and his proof of Kepler's Area Law is valid for a curved segment of the orbit *whatever the force law*, provided the force acts towards the centre and the orbit remains outside the centre. But it seems that Rindler has GR in mind, for in this connection he

[16] Lorentz *et al.* 1952, 94. Sommerfeld's notes were included in the first German edition published in 1913 (Lorentz *et al*. 1913, 71). I have translated from the original German: "Dieser Aussage liegt, wie Einstein hervorgehoben hat, die (unbeweisbare) Annahme zu Grunde, daß die bewegte Uhr tatsächlich die Eigenzeit anzeigt, d. h. jeweils diejenige Zeit gibt, die dem stationär gedachten, augenblicklichen Geschwindigskeitzustand entspricht. Die bewegte Uhr muß natürlich, damit sie mit der ruhenden im Weltpunkte *P* verglichen werden kann, beschleunigt (mit Geschwindigskeits- oder Richtungsänsderungen) bewegt worden sein." Unfortunately, Sommerfeld does not give a reference for the attribution of this remark to Einstein.

immediately refers to a proof by Møller "on the basis of the field equations of general relativity that certain idealized mechanical and atomic oscillators do, in fact, satisfy the clock hypothesis" (29-30).

In fact, it is in the context of GR that we see the motivation for thinking that the clock hypothesis might be a separate element in the theory. For it is with the publication in 1918 of Hermann Weyl's celebrated attempt at a unified theory of gravitational and electromagnetic forces (Weyl 1918b) that the status of the "clock hypothesis" is first called into question. This is the paper that introduces the idea of *gauge symmetry* into modern physics, and its significance for the clock hypothesis has been discussed in an illuminating way by Harvey Brown and Oliver Pooley in their (2001), as well as by Roger Penrose in his recent tour-de-force (2005).[17] The connection with the clock hypothesis is that in Weyl's theory (as Einstein pointed out in criticizing the theory) the proper time elapsed for a clock carried on a round-trip would not only vary with the path through spacetime, as in SR, but in such a way that if the travelling clock had encountered regions of space containing a varying electromagnetic potential in a static gravitational field, it would return to its starting point ticking *at a different rate* than one that had remained at the starting point.

Figure 2 *The speeding up of the rate of the moving clock* (after Penrose 2005, 452)



The leading idea of Weyl's paper is that the Riemannian geometry assumed by Einstein in his GR is insufficiently local, since it "enables us to compare, with respect to their length, not only two vectors at the same point, but also the vectors at any two points" (Lorentz et al, 1923, 203). As Brown and Pooley explain, Weyl insisted instead that "only the ratios of the lengths of vectors at the same point and the angles between them can be physically meaningful" (265). The result is a *conformal geometry* in which there is no absolute scaling for spatial and temporal distances, so that the metric is only given up to a

[17] See also the discussion of Torretti in his (1983, 189-190).

proportionality. In this scheme a Lorentz metric $\mathbf{g}$ is still required as a constraint on all the local physics, providing us with the local Lorentz group that is to act in the neighbourhood of each point. As a result, the null cones of Minkowski geometry still perform the same role that they do in Einstein's theory; this is the so-called conformal structure. Thus transformations of the form $\mathbf{g} \rightarrow \lambda\mathbf{g}$ are permissible, where λ is a scalar function on the spacetime (Penrose 2005, 451). These are called *conformal rescalings*. Weyl posited some structure additional to the conformal structure (the Minkowskian null cones) , namely a *gauge connection*, a bundle connection that would have the Maxwell field tensor $\boldsymbol{F}$ as its *curvature* (452). This curvature represents the (conformal) time scale change as the difference between two infinitesimal paths from a point *p* to a neighbouring point *p*′. Consequently, as Penrose explains, "in Weyl's geometry there are no 'ideal clocks'. The rate at which any clock measures time would depend on its history" (2005, 451). Of course, this turns out to be in conflict with the empirical evidence, which is why Weyl's theory was set aside.

Nevertheless, for Brown and Pooley the significance of this rival theory to Einstein's lies not in its truth or falsity, but in its very possibility. For the possibility of such a second time dilation effect of dynamical origin shows that Einstein has made two independent assumptions relevant to proper time: (i) that locally the metric is Minkowskian, and (ii) that the consecutive elements $d\tau$ in consecutive LIFs (local inertial frames that are locally Lorentz) are all that contribute to total proper time elapsed, i.e. that there is no contribution over and above that of the instantaneous velocities in timelike paths that are not geodesics. [reference] Moreover, they see Weyl's argument in his 1918b as being of a piece with a pre-existing concern he had concerning the treatment of accelerated motion in SR:

> 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). (Brown and Pooley 2001, 264; Brown 2005, 115)

Now, as we saw above, this concern seems misplaced in the context of Special Relativity. Weyl himself seems to have realized this, for in his discussion of SR in his (1949) he writes that "it can be shown that the metrical structure of the world is already completely determined by its inertial and causal structure, that therefore mensuration need not depend on clocks and rigid bodies, but that light signals and mass points moving under the influence of inertia alone will suffice" (103). But, as Brown is at pains to point out, the status of SR changes between its first appearance as a global theory in 1905 (or its 1908 generalization by Minkowski), and its later subsumption into GR as holding only locally: what are accelerations resulting from gravity in the first theory are reconfigured as *inertial motions*, motions along geodesics in GR.[18] As a consequence, it is by no means a foregone conclusion that a natural clock π satisfying the condition for an ideal clock in SR, namely that it measures along its worldline the proper time $\int_P d\tau$ defined on the Minkowski metric *U* of each of the successive tangent spaces to its trajectory (the local Lorentz charts), will also do so in the global metric of GR. As Torretti explains, if the clock is a freely falling particle in a real inhomogeneous gravitational field,

> the several flat metrics which thus approximately hold good in the domain of each local Lorentz chart cannot be regarded as the restriction to their respective domains of a global Minkowski metric, any more than the Euclidean metrics that show up, say, in the street plans of Mannheim and Manhattan are the restrictions to these boroughs of a Euclidean metric defined on the entire Earth. On each domain *U* the worldline of π satisfies, within the said margin of error, the variational law [$\delta \int d\tau = 0$]; yet the law does not make any sense beyond the boundary of *U*. (1983, 151)

Not only must the global metric $\mathbf{g}$ be approximated on a small neighbourhood of each point by the local flat metric $\boldsymbol{\eta}$, we must also stipulate that "if $\int d\tau$ is now made to stand for the length of π's worldline as determined by $\mathbf{g}$—i.e. for the proper time measured along it by a natural clock at π—i.e. the variational principle $\delta \int d\tau = 0$ is obeyed by the freely falling particle between any two events in its history." (151) This is the *Geodesic Principle*. As Torretti observes, this was regarded by Einstein as "the core of the Hypothesis of Equivalence", or strong equivalence principle. The geodesic equation of motion for test bodies is now more commonly regarded as a theorem, derivable from the vanishing of the covariant derivative of the energy-momentum tensor.[19]

So let's reconsider the logic of the argument from the failure in Weyl's theory of the clock hypothesis to the conclusion that it is an extra assumption in Einstein's. To be sure, the contrast with Weyl's theory is informative, and highlights the fact that the conforming of clocks to the clock hypothesis is a contingent matter, and also that it is implicit in Einstein's GR. But this is not sufficient to show that it is an *independent* hypothesis, rather than something that is already built into the theory. So let us dig deeper into the contrast between Weyl's theory and Einstein's. The fact that the clock

[18] "The special theory of 1905, together with its refinements over the following years, is, in one important respect, *not* the same theory that is said to be a restriction of the general theory in the limit of zero gravitation (i.e. zero tidal forces, or space-time curvature). [I]n this picture, local inertial co-ordinate systems are freely falling systems. They are not in Einstein's 1905 theory." (Brown 2005, 15; cf. also p. 88)

[19] See Misner *et al.* 1973, 471-80; Brown 2005, 161-2. A very clear discussion and derivation of the geodesic principle is given by Stephen Lyle in his (2008, 36-46). He shows that the "principle" follows from Einstein's equations, if we assume an almost point-like particle in zero-pressure matter dust that is not jostled by other particles, has no torsion, and no electric charge (43-4).

hypothesis fails in Weyl's theory, despite the fact that the latter shares its conformal structure with GR, is due to the fact that Weyl's gauge connection is not a metric connection. As Brown and Pooley remark, "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." (267). The vanishing of the latter is the condition of *metric compatibility*, a condition which Brown and Pooley claim that "Schrödinger was right to call 'momentous'" (267). This condition, they continue,

> means that the local Lorentz frames associated with a space-time point *p* (those for which, at *p*, the metric tensor takes the form 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*.)" (267)

Thus it is because GR obeys this condition that the laws for the non-gravitational interactions take their familiar Lorentz covariant form relative to the local Lorentz frames. But this in turn is the content of the Strong Equivalence Principle (SEP), usually taken (e.g. by Misner *et al*. 1973) to be one of the essential postulates of GR:

> *in any and every local Lorentz frame, anywhere and anytime in the universe, all the (non-gravitational) laws of physics must take on their familiar special-relativistic forms*. (Misner *et al*. 1973, 386)

In his (2005), Brown seems to concur with this estimation of the SEP. He argues that some authors[20] have held that the "chronometric significance accorded to the Minkowski metric in SR is automatically recovered locally in GR". Against this, perhaps with the example of Weyl's theory in mind, he argues that "it is only through the SEP that such chronometric significance can be given to the tangent space geometry in the first place" (2005, 170).

This concurs with the point of view of Stephen Lyle, who shows how this chronometric significance is delivered as follows: he assumes in order de Sitter spacetime, Schwarzschild spacetime, and the static homogeneous gravitational field, and shows in each case that by an application of the SEP one can "carry over from theories in SR that govern our clocks and rulers", to demonstrate that "the metric delivers the lengths and times that would be measured by our clocks and rulers" (2009, 5). The CH is not then an independent unprovable assumption, but a provable theorem. (In the case of de Sitter spacetime it is approximate, due to the fact that the inertial transformations, if they exist, are only approximately linear in de Sitter spacetime).

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[20] Brown (2005, 170, n. 51; and Brown and Pooley (2001,) specifically singles out Roberto Torretti, a claim which the above discussion and quotation might appear to throw into doubt. But I shall not discuss that here.

Minkowski's Proper Time and the Clock Hypothesis

To sum this up somewhat figuratively, it is the metric connection in Einstein's theory that threads together the various tangent spaces to a timelike spacetime path in such a way that the path has the same chronometric significance as in SR, and this is what the SEP achieves. Again, the contrast with Weyl's theory is instructive. For there the path dependence of the gauge connection is what spoils that chronometric role, and indeed makes it impossible to define an ideal clock, since "the rate at which any clock measures time would depend upon its history" (Penrose 451). It is the SEP in Einstein's GR that allows for proper time to have the same chronometric role it does in SR.

This relates in a suggestive way to another concern of Brown's, namely that GR explains something that previously should have been considered "a miracle", namely that bodies not under the action of external forces should "conspire to move in straight lines at uniform speeds while being unable, by *fiat*, to communicate with each other" (2005, 15). Famously, of course, Einstein's GR takes the mystery out of the numerical equivalence of inertial mass and gravitational mass by simply identifying them, the root idea behind the equivalence principle. But now we can see this as having the effect of enshrining Newton's idea that absolute time is the measure of time beaten out by a body undergoing inertial motion—which Einstein and Minkowski had simply imported into Special Relativity—in a perfectly consistent way in General Relativity. This is why the SEP manages to preserve the chronometric significance of the metric in SR: it preserves the relation of inertia to time assumed in modern physics. This relates in an interesting way to Einstein's criticism of Weyl's unified theory. For as Penrose points out, it is not just that spectral frequencies will depend on an atom's history: so will particle masses! Given the Einstein relation $E = mc^2$ together with the De Broglie relation $E = h\nu$, it follows that every particle of rest mass m will have an associated natural frequency mc^2/h. "Thus, in Weyl's geometry, not just clock rates but also a particle's *mass* will depend upon its history." (Penrose 2005, 453). The ideal clocks of special and general relativity, by contrast, in preserving the relation of inertia to time, also preserve the constancy of rest mass in time.

Conclusion

I have argued here that *proper time* must be regarded as one of Minkowski's enduring contributions to physics. I have examined some confusions that still interfere with an appreciation of this, including a conflation of proper time with the co-ordinate time of the inertial frame of a system at rest, and the related mistaken notion that SR cannot be applied to accelerating systems. This sets the stage for a treatment of the so-called "clock hypothesis" (CH). If my arguments above are sound, then the CH is not needed as an independent postulate in SR. Insofar as it can be regarded as stating the criterion for an ideal clock in SR, it is already implicit in that theory in the invariance of proper time, as defined by Minkowski: in a spacetime whose global metric is Minkowskian, an ideal clock cannot fail to keep proper time, however it is accelerated. The argument that many real clocks will fail to satisfy the hypothesis is just the claim that many processes fail to qualify as ideal clocks; but provided we can account for that discrepancy by means of an account of the ratio of the acceleration undergone by the clock and its restorative forces, no appeal to any hypothesis extrinsic to the theory is needed. The argument that there is a contrast between a "kinematic account of acceleration" and "the dynamics of the real forces acting on the clock" seems to misconstrue the question of whether a given process can approximate an ideal clock with the question of whether an ideal clock can be defined using the theory alone; and also to come close to implying that SR is inadequate to treat real accelerations, the mistaken notion we treated in section 2.

Secondly, the failure of the clock hypothesis in Weyl's unified theory of gravity and electromagnetism does not imply that it is an independent assumption in Einstein's theory. There *is* an assumption in Einstein's GR separate from the assumption of LIFs: this is the (strong) Equivalence Principle, that in every such LIF, at any spacetime point all the non-gravitational laws of physics must take on the forms they have in SR, i.e. must be Lorentz covariant. In a theory such as Weyl's, ideal clocks do not in general keep proper time. This is a result of the non-metric compatibility of Weyl's theory: the covariant derivative of the metric does not vanish everywhere. But by the same token, Weyl's theory does not conform to the Strong Equivalence Principle. Einstein's GR, on the other hand, does conform to the Equivalence Principle, and it follows from this that the clock hypothesis is no more an additional assumption in GR than it is in SR.

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