Farewell Minkowski Space?

G. Galeczki and P. Marquardt (*Apeiron* 3: 3-4, p.121) invite physicists to bid farewell to Minkowski space in favour of Poincaré's (3+1)D model. They ignore that neither, on purely mathematical grounds, is applicable in physics and that this is the reason why the mathematical untenability of special relativity (SR), independent of its obvious inapplicability in dynamics, has not been seen. In consequence, oblivious of the warning, by many mathematicians including Lakatos and Kline, that modern methods are instruments of mystagoguery, they present their argument in terms of an esoteric mathematical jargon which serves to obscure the simple logic of the case they purport to describe. They thus fail to see, for instance, that the isotropy claimed by J.-P. Vigier can also easily be shown to be a phantom.

Minkowski proclaimed that "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" (I restrict references to essentials). This was a strange claim coming from a mathematician, for in the mathematics of motion, as is to date evident in textbooks at all levels, time, as a so-called parameter, has always been intrinsic to space measurement, in that distances are exclusively quantified in terms of parametric expressions like vt or ct. Poincaré cannot escape blame for the havoc caused by dissolving the union, and Minkowski's noumenal ict renders ct, the resultant of the space vectors, orthogonal to itself.

Einstein's exquisitely simple original description of moving points is preferred by physicists precisely because the conventional parametric rendition is still clearly present if unacknowledged; only uncritical acceptance of the group concept, not applicable to parameters, may have led such a distinguished author as Silberstein to complain that "Einstein's method of reasoning, as given in his original paper may be mathematically interesting, but does not seem the fittest when a clear discussion of the physical aspect of the [case] is aimed at." To my knowledge, only Cullwick (1959) distances himself from "the four-dimensional analysis so attractive to mathematicians" and presents diagrams in accordance with parametric convention. Leaving Minkowski space for the (3+1) mathematics of Poincaré, irrespective of the argument from dynamics, is therefore no solution for physics.

Although this confusion is responsible for the unintelligibility of SR kinematics, and the consequent necessity to take recourse to blind symbol pushing with its acknowledged high risk of grotesquely fallacious deduction, nevertheless, it ought to have been seen that Einstein's derivation of the ratio t'/t is valid for points on the main axis of motion only; for points moving in any other direction he (1905, §3) correctly states as an explicit and essential restriction that $(c-v)t = 0$. As should be easily evident from a simple 3D diagram (here omitted), the correlation for a general point $P(x'_i)$ $(i = 1,2,3)$ is

$$t' = t\left(\sum x_i'^2\right)^{1/2}\left(\sum x_i^2\right)^{-1/2} \qquad (1).$$

I have already indicated (*Apeiron* 3: 3-4, p.126) that mathematically $\gamma = 1$, and that the conventional derivation is fallacious in that it fails to correct v' after the relativistic change of t' which is necessary if we wish to obtain c instead of $c \pm v$. (Correction: The equation $|v'| = |vc/(c+v)$ should read $|v'| = |vc/(c \pm v)|$. *N.B.*: When $|v'| = |vc/(c-v)|$ and $v > c/2$ it follows that $v' > c$; one of the essential premises of SR, *viz*. that c is a limiting speed, is thus seen to be mathematically untenable.) For points moving with speed c in direction of the x-axis only, where $x = ct$, $x' = ct'$, equation (1) reduces to $t' = \left(t \pm vx\right)/c^2 = t\left(1 \pm v/c\right)$.

Although Galeczki and Marquardt dismiss the Lorentz transformation (E-LT) as inapplicable on dynamical grounds, they concede that there exists an inverse transformation T^{-1} such that $T.T^{-1} = 1$. They ignore that this procedure succeeds on condition not only that purely mathematical quantities ("distances") shrink by the factor ($\gamma \neq 1$), but that two coextensive distances L' and L shrink such that $L'/L = L/L' \neq 1$; long acceptance of this absurdity does not make it the least bit more reasonable. For if we put $t' = kt(1 \pm v/c)$ $(k \neq 0,\ v < c)$ the conventional inverse transformation succeeds only if we use the correct equation for v', for only then (depending on the sign of v)

$$\left(1-\frac{v}{c}\right)\left(1+\frac{v'}{c}\right)=1, \quad \text{or} \qquad \left(1+\frac{v}{c}\right)\left(1-\frac{v'}{c}\right)=1 \qquad (2).$$

Mathematicians know that recourse to diagrams is essential if nonsensical arguments are to be avoided; such a humble precaution is surely mandatory for physicists. Had the authors observed it they should have seen at once that reciprocity cannot obtain, that the "unknown constant" μ necessarily equals c^{-2}, and that, moreover, notwithstanding the sophisticated terminology, the equation given for two successive transformations (composition of speeds) is false. The correct equation, like the correct equation for v', is easily obtained by recourse to diagram.

Consider a point P moving with speed w' in S' (Fig. 1: see p. 15); let Q be the wavefront at the time t and t'. We have $OQ = ct$, $O'Q = ct'$, $OO' = vt$, $O'P = w't'$ and $OP = wt$.
Then $w't'/ct' = (w - v)t/(c - v)t$, so that

$$w = v + w'(1 - v/c) \qquad (3).$$

(Mathematics is the art which renders the complicated simple!)

Finally, recourse to diagram might have enabled the authors to refute J.-P. Vigier's claim that the light speed is isotropic in SR. The lack of isotropy, on purely logical as well as physical grounds, is nowhere more beautifully evident than in Einstein's derivation of the E-LT [2] construed when at the height of his analytical power. Here we have points moving with speed c in either direction of the x-axes of S and S'; the origin O' of S' moves with speed v to the right; I use subscripts to distinguish the respective distances and indicate the wavefronts by bracket signs. Fig. 2 (see p. 15) shows S as the rest frame.

Einstein puts $ct'_+ = \lambda\, ct_+$ and $ct'_- = \mu\, ct_-$ $(\lambda \neq \mu)$, and concludes that

$$ct_+ + |ct_-| = 2ct$$
$$ct_+ - |ct_-| = 0 \qquad (4)$$

as well as

$$ct'_+ + |ct'_-| = 2ct'$$
$$ct'_+ - |ct'_-| = 0 \qquad (5)$$

Equations (4) and (5) assert isotropy in S as well as S', but for this to obtain in physical reality there must exist two wavefronts to the left of O and O' (Fig.3: see p. 15).

In general, SR, as conceived by Einstein in his maturity, tacitly presupposes therefore that there are as many wavefronts on the side of O opposite to that of O' as there are inertial frames of reference (IFR). This a logical and physical impossibility which the authors might have included in their objections against the infinity of phantomatic IFRs. (*N.B.*: Einstein here proves that the E-LT is independent of the direction which throws a light on his preference for the incomplete form. Hence those who make the E-LT dependent on direction, and who argue the case of isotropy from considerations of simultaneity, are at variance with Einstein's surrealistic interpretation of his theory.)

References

Cullwick, E.G., 1959. *Electromagnetism and Relativity* (Longmans, Green & Co., London).

Einstein, A., 1962. *Relativity* (Methuen, London), App.I, p.116.

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On Moving Rods and Clocks

Based on the Lorentz transformation, Einstein[1] treated the relationship of two inertial systems K and K' which move at the speed *v* relative to each other. Discussing length measurements in system K' he stated: "... it therefore follows that the length of a rigid metre rod moving in the direction of its length with a velocity *v* is $\left(1 - v^2/c^2\right)^{1/2}$ of a metre." He considers that this is so in the inertial system K'. This assumption is based on an observation from a point K_o in the inertial system K (Fig 1.: see p. 25).

If this assumption were valid for a real effect, then an observer K_o' in K' could find the constant velocity "*c*" of light in the direction of *v* only if the unit of time of his clock were longer by the same proportion as the shortening of his rods. On p. 36 such a dilated unit of time is assumed. It then seems that both rod contraction and time dilatation are true "relativistic effects".

We think that these effects do not really happen in a real system K', which is only hypothetical in the above reasoning. It is a shortcoming of this reasoning that it applies the unaltered results of K_o to events in K'. These results are based on observations outside of the system K'.

Whatever is measured by an observer, who moves relative to the system in which an event takes place, cannot correctly describe that event without altering his data by a valid method of transformation. We have shown[2] that the Lorentz transformation cannot be applied in all directions. Correct transformation methods would require many data, not all of which are available at present. Their use would involve very complex calculations.

Furthermore, if all distances in the direction of *v* were indeed shorter, then K_o—using his metre rod of varying length—would measure all distances to be the same as at rest. But using dilated time units, he will measure time intervals to be shorter than at rest. Therefore he would then find the velocity of light in all directions to be greater than "*c*", the constant value of which is a basic assumption of Einstein's Special Theory of Relativity (STR). While some clocks may slow down due to some outside influence, a dilated unit of time in an inertial system is clearly not a relativistic effect.

It can be shown, without relying on any observation and transformation, that if the relativistic effects discussed were real, they would not be compatible with the principles and rules of the STR. If in a real system K' distances parallel to its movement were shortened, and time units of an obsrver's clock were dilated proportionally, then observer K_o' would find the velocity of light in that direction to be "*c*". But according to the STR, distances at right angles to movement are not shortened. Therefore the light speed there would be found greater than "*c*".

Einstein does not clearly define the velocity *v* which may be relative to K' or relative to K. Its value differs if the two timescales differ. This difficulty is due to an unjustified assumption that two different inertial systems can exist in the same location, one at rest, and the other moving. In fact, real inertial systems in which no outside influences act, do not exist in our universe.

One reason for some experts accepting the "relativistic effects" as proven, is that they are mathematically correct when based on assumptions that are not valid in all of the reasoning involved. As Einstein himself said , "....so different is the meaning of the term scientific truth" according to whether we are dealing with a fact of experience, a mathematical proposition. or a scientific theory". When this is kept in mind, one can still recognise the value of the rather abstract STR in the progress of physical science.

References

1. Einstein A. (1960) *Relativity*, Methuen, London, p.35.
2. Szego L. Ofner P.F.(1996) *Eur.J.Phys.* 17 156.
3. Einstein A. (1935) *Essays in Science*, Philosophical Library, New York, p.11.

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Quantum Physics Revisited

Why was Bohr's physical model of the hydrogen atom [1], H, rejected, despite its remarkable successes, and replaced with the abstract mathematical model, quantum mechanics? Witten[2] explains,

> *Early in this century, physicists realized that a classical electron orbiting an atomic nucleus should emit electromagnetic radiation and spiral into the nucleus in a finite time, driven by the singularity of the r^2 force at small r.*
>
> *From grappling with this contradiction, quantum mechanics was born.*

There were two critical pieces of physical information (evidence) missing from the 1913 Bohr model of H which precluded the formulation of a physically consistent model. Both of the missing pieces to the puzzle involved the law of conservation of angular momentum.

There never was a proper accounting in the Bohr model for the change in angular momentum of the system as the electron jumped from one stationary orbit to another stationary orbit. Had this defect been explicitly recognized, the solution might have become obvious at that time.

In 1927 Ruark and Urey [3] proposed and in 1936 Beth,[4] with the assistance of Einstein, experimentally demonstrated that a photon possesses an angular momentum of $\pm h/2\pi$ (*h* is Planck's constant). This model has stood the test of time.

When a free electron and a free proton combine to form an H atom in its ground state, a photon with half of the potential energy of the electron and proton, due to their proximity, is emitted. This photon carries away with it an angular momentum of $\pm h/2\pi$. The conservation of angular momentum is established by the orbital angular momentum of the H atom in its ground state of $\pm h/2\pi$. This process is reversible when a photon of the required energy dissociates an H atom from its ground state into a free electron and a free proton and is annihilated in the process. It is interesting to note that this physical model (process) differs from the quantum mechanical model where the H atom in its ground state is proposed to have zero angular momentum.

Had it been common knowledge in 1913 that the photon possessed an angular momentum of $\pm h/2\pi$, the Bohr model of H would have been on sound physical grounds and there would have been no need to invent quantum mechanics.

Contrary to the assertion of Witten, above, (and similar contentions by others [5,6]) the emission of electromagnetic radiation (photons) and the spiraling in of the electron from the ground state of the H atom is not physically possible—it would violate the law of conservation of angular momentum. In the Bohr model of H in its ground state the orbital angular momentum is $\pm h/2\pi$. A detailed discussion of various aspects of this specific point, the justification for the stability of the Bohr model of the H atom in its ground state by physical models, can be found in this author's writings [7,8].

References

1. Bohr, N., *Phil. Mag.* 5. 6. 26,1-25 (1913).
2. Witten, E., *Nature* 383, 215 (1996).
3. Ruark, A. E. and Urey, H. C, *Proc. Nat. Acad. Sci U.S.A.* 13, 763-771(1927).
4. Beth, R. A., *Phys. Rev.* 50, 115-125 (1936).
5. French A.P. and Taylor, Edwin, *An Introduction to Quantum Physics* (W.W. Norton, New York City, 1978) pp. 638-639.
6. Haken, H., *Light* (North-Holland, Amsterdam, 1981) Vol. 1, pp. 9-12.
7. Deutsch, D.H., *Physics Essays* 2,113-117 (1989).

8. Deutsch, D.H. *Apeiron* 3, 52-53 (1996) and references cited therein.

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