Electron Centenary

George Thomson in an interesting account of the story of the electron assigns 1897 as its birth year, though the word "electron" as a unit of electric charge was first used by an Irish physicist Johnstone Stoney in 1891[1]. Will 1997 herald a rebirth of the electron? To put the discussion in perspective, let me quote from Stoney: "In looking back at itone is impressed by the extent to which a theory long held can blind even first-rate minds to new ideas and by how easy it is to explain almost anything in terms of a favour theory." I have termed such theories extended to explain anything as complex extended derived theories, and suggested that once a deep thought occurs to someone, the complexities disappear, and there originates a simple and beautiful theory[2]. In this letter I propose to make a case for an alternative theory of the electron.

Quantum electrodynamics (QED) is the most successful theory of the electron; however, it has inherent weaknesses. The problem of divergencies has remained unresolved, the meaning of electric charge, mass and spin of electron is not clear, and the lepton mass spectrum is unexplained. The grand unified theories, for that matter even the so-called theory of everything, *i.e.*, the superstring theory, are no better than the QED.

Technological advances have led to a number of new experimental results in condensed matter physics which are not clearly understood, fome of them are listed in the following. The theory of high temperature superconductivity is still in a fluid state, the situation has not changed much since the analysis presented by Anderson in 1992[3]. Role of Magnus force on a moving vortex in a superconductor has found renewed interest, but remains controversial[4]. Electron transport in mesoscopic systems has given rise to novel results, examples include ballistic transport, quantized conductance in multiples of $2e^2/h$ and Hall conductance in multiples of e^2/h in low dimensional semiconductor devices (where e is electron charge, and h is the Planck constant[5]), and phase measurements of electrons during resonant tunelling in quantum dots[6]. It is possible that these results are hints for a new physics where single electron and single photon dynamics, and a concrete physical description of the structure of these objects are important.

The nature of the electron was a fundamental problem for Lorentz, and Dirac proposed quite a few new versions of electron theories (see [2] for a discussion). My own interest in this problem has led to some new and unorthodox ideas (see a review in [7]). It has been argued that physical concepts like mass, charge and electromagnetic fields derived from macroscopic observations may not be valid for elementary entities like the electron and photon. Geometrical structure for electron, neutrino(s) and photon has been envisaged in terms of what I have termed spatio-temporal bounded fields. Electron and neutrino are 2 space + 1 time dimensional extended objects, while the photon is a composite structure consisting of neutrinos. The electron has two lengths, *i.e.*, the Compton wavelength and electron charge radius, which determine the dimensions of the circular disk and torus in 2 space dimensions. The neutrino is represented as a circular disk. Angular momentum (or spin) and internal oscillation frequency are basic physical properties. Circulating field in a torus or a "hole" in a disk is responsible for what is called electronic charge. Positive charge (or positron) is a time-reversed state of negative charge (or electron). Free electron moves with the velocity of light in this model. I have attempted to develop a tentative theory based on this model. One approach which gives interesting results is based on a new action function in Weyl space[8]. This theory allows decoupling of electron and magnetic fields, and a solution can be found such that the electron moves with the velocity of light[9]. One might raise the objection that Weyl geometry is unphysical or that the solution is unphysical. I believe this idea needs to be tested experimentally, and should not be dismissed without serious consideration.

I admit that much work remains to be done to develop an alternative theory, and the ideas belong to the realm of speculation. In 1987 I proposed an experimental scheme to test the idea of massless electron. The proposal is that the electron motion with the velocity of light is responsible for the undiminished current flow in a superconductor. A frequent objection against this proposal is: how does electron motion with the velocity of light imply infinite conductivity? In this connection, a recent discussion of Landauer's idea that "conduction is transmission"[5] could prove useful. It is surprising that mass of the cooper pair in the BCS theory of superconductivity is arbitrary, and it has remained inaccessible to experimental observation. We propose to use time of flight technique to measure the velocity of electrons in superconductor. This method has been successfully used in semiconductors to determine velocity-electric field characteristic of carriers. The width of the external circuit current pulse is a measure of the transit time of electrons across an active region of the device. In the case of superconductors, a long specimen could be used; however, the problem of contacts was noted in [9]. Recently, it has come to my notice that InAs semiconductor and Nb superconductor possess a transparent interface, *i.e.*, there is no Schottky barrier[10]. Thus it seems technologically feasible to make direct observation of electron velocity in a superconductor.

To conclude, I quote from J.J. Thomson's 1897 paper[11]: "… as to the nature of the cathode rays. The most diverse opinions are held as to these rays; according to the almost unanimous opinion of German physicists they are due to some processes in the aether to which… no phenomenon hitherto observed is analogous: another view of these rays is that, so far from being wholly aetherial, they are in fact wholly material…" Thomson went on to establish the second point of view, i.e., the electrified particle theory. The massless spatio-temporal model of the electron is in a sense akin to the aetherial view; perhaps the reborn electron will be of this kind.

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S.C. Tiwari
1 Kusum Kutir, Mahamanapuri
Varanasi 221005, India

The mathematics of relativity

Now that we possess a journal dedicated to the clarification of mathematical aspects of special relativity (SR), the Special Relativity Letter, a reply to the relevant debate in *Apeiron* would be wasting space urgently needed for other topics. But there is the danger that, for a wider readership, misconceptions go unchallenged. Critical articles and correspondence leave the impression that SR is Einstein's creation and that it is open to unqualified objection. To see things in perspective, and to reclaim mathematics for the description of intuitively fully intelligible models of hypothetical physical reality, some distinctions need to be made. Broadly, there are valid, faulty and invalid mathematical arguments, and valid, faulty and invalid applications. Needless to say my sk*etc*h is tentative and crude.

As to the mathematics, first, geometrical models based on Minkowski's interpretation of the non-zero SR space-time interval as an instance of his theory of quadratic forms are perfectly valid (but inapplicable in physics). Before I can discuss the rest I must explain the difference between faulty and invalid arguments. An argument may be valid according to the rules but marred by errors; such an argument is faulty. An argument may be wholly unacceptable; even if it hits upon the right answer it is invalid. Our second case is therefore that of numerous 'valid' derivations of the Lorentz transformation (LT) by mathematicians; though valid, they are marred by a number of more or less serious errors. Third, we have wholly invalid arguments; significantly these include Einstein's own.

As to application, first, upon certain conditions and if amended, faulty forms of the LT are applicable. They specify so-called ideal clocks which render the light speed invariant in all inertial systems; they do not imply that these clocks set themselves to go at the specified rate, but specify the rate to which users have to set them in order to obtain the desired result; after all, ordinary clocks do not calibrate themselves, either. However, the LT prescribes recourse to two different clocks for signals in opposite directions; though theoretically applicable this is useless. Second, most importantly, contrary to assumption, the LT does not imply contraction and mass increase since these are merely the result of a mistake. Third, the bulk of SR formalisms of applied mathematics is invalid. They include tensor models which ignore that kinematics is not 4D but 3D, as well as constraints involving concepts of group theory, covariance or contravariance; none of these are capable of interpretation in terms of the kinematic models of physics. They include, further, those kinematic arguments of physicists, including Einstein, which arrive at the plethora of equations believed to be amenable to verification (time dilation, length contraction, *etc*.).

Assertions that SR formalisms are altogether false, and a degree of Einstein fixation among critics, are therefore misleading. What is important to see is that, even though some formalisms are perfectly valid, they are either not applicable in physics or useless.

G. Walton
18 St. Swithun Street
Winchester SO23 9JP (U.K.)

Refutation of a refutation of the Lorentz transformation

Consider the Lorentz transformation in its familiar form in 2 + 1 dimensions, between the coordinates of an event in an inertial system to those in another such system,

$$x' = \gamma(x - vt),\ y' = y,\quad t' = \gamma(t - vx/c^2) \qquad (1)$$

where $\gamma^{-2} = 1 - v^2/c^2$. Here the coordinates (x, y, t) and (x', y', t') refer to the same "point event". The "primed system" is moving to the right with uniform speed v, relative to the other system, along the x axis. (Or one could say that the x and x' axes are chosen to be in the direction of the relative velocity.) We ignore the third spatial dimension because it is irrelevant to the context.

Szego & Ofner (1997), when considering "Einstein's errors", offer an alleged proof that $t' = t$ which of course would refute the Lorentz transformation if their argument were correct. A defender of the faith is confident in advance that their argument must be fallacious and my purpose is to expose the fallacy.

They consider a photon travelling up the y axis, starting at the origin so, for any point in its path we have $y = ct$. They assume similarly that $y' = ct'$ and they argue that, because $y' = y$, it follows that $t' = t$. They seem to be inferring from $y' = y$ that the y' axis is the same as the y axis, but actually the y' axis is moving with speed v away from the y axis while remaining parallel to it. The equation $y' = ct'$ does represent the path of a photon moving up the y' axis but this is not the same photon as the one moving up the y axis. An event on the y' axis is not an event on the y axis except when $t = 0$. So Szego & Ofner are mistaken and should now nobly admit it.

But let us take the argument further to get a neat confirmation of the Lorentz transformation and also to make the argument more convincing. We again consider a photon travelling up the y axis, of course with speed c. A typical event on its world line has coordinates $(0, ct, t)$. By the Lorentz transformation, equations (1), in the primed system the coordinates of the same event are $(-\gamma vt, ct, \gamma t)$ because $x = 0$.

Note first that $x' \neq 0$, so the "event" is not on the y' axis although $y' = y$. (We have already noticed that.) The distance of the event from the origin, in the primed system, is (by Pythagoras's theorem)

$$(x'^2 + y'^2)^{1/2} = (\gamma^2 v^2 t^2 + c^2 t^2)^{1/2},$$

so the speed of the photon in the primed system is

$$(\gamma^2 v^2 + c^2)^{1/2}\, t/(\gamma t)$$
$$= (v^2 + c^2\gamma^{-2})^{1/2} = c. \text{ (Bingo!)}$$

Thus the photon travels with speed c in both the unprimed and the primed systems, and the self-consistency of the Lorentz transformation, and the constancy of the speed of light in inertial systems, are once again confirmed. Everything fits together like a jigsaw puzzle.

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I.J. Good
Department of Statistics
Virginia Polytechnic Institute
& State University
Blacksburg, VA 24061-0439

Reply by Szego and Ofner

I.J. Good[1] has discussed our letter "Einstein's Errors"[2] and found our reasoning in error. We think that his objection is due to different meanings attached to the symbols we used. When using mathematics for examining a statement in physics, it is essential for every symbol used to have a definite meaning. Einstein used the Cartesian system of coordinates in which a symbol y means the abscissa, the distance of a point from the (x,z) plane. Perhaps we should have said so. However, neither Good, nor Einstein thought this essential. Sometimes the meaning of a symbol is self-evident, and in other cases it may not be.

Good thinks that we refuted the Lorentz transformation falsely. In fact, we only pointed out its limitations. Our relevant reasoning was described in our article in *Eur. J. Phys.*[3], referred to in our letter[2]. This reference was apparently not considered by Good.

Good writes about events in a moving inertial "primed" system as observed from a different inertial system overlapping this one. We think that such an observation cannot reliably be made and interpreted using Einstein's Special Theory of Relativity (STR). That theory says that a rigid rod pointing in the direction of its rapid movement contracts, while the same rod, when pointing in the direction normal to its travel retains its full length.

Actual measurement of this contraction is not feasible in the usual way, because any measuring instrument would also contract in the same manner. The STR states that the velocity of light *in vacuo* is a universal constant, identical in all directions within any inertial system. The time taken by light to travel the length of a rod should therefore indicate the rod's true length.

Performing such a measurement is beyond the limits of present technology. But theoretically it is possible. This difficulty can be overcome by so-called thought experiments, which disregard purely practical problems. But the validity of this particular experiment does also depend on the existence of inertial systems. These do not exist anywhere in our universe, as far as we know. Even less possible is the existence of two such systems, the domains of which overlap.

When examining our statements about y, it is neither necessary, nor helpful, to change the Cartesian representation of the STR. In this system y or any other symbol y denotes the distance of a point from a plane containing the Cartesian axes x and z. If y were understood to mean the distance from the "origin", then its relevant coordinates would be polar ones. From the "origin" an observer would indeed find the length of y and y to be different, as Good suggests. The second Lorentz equation $y' = y$ would then not apply.

A constant speed of light *in all directions* in all inertial systems is stipulated by the STR. Observers in both systems may therefore find $c = y'/t'$ and $c = y/t$ respectively. If c is a universal constant, *this means that* $y'/t' = y/t$, as we stated. Our other statement, namely that $y' = y$ follows from the two inertial systems in Einstein's case moving parallel to the x axis, and hence y does not change when the system moves. $y' = y$ remains true for all speeds. Whether both of these equal zero or not is not important. If c is indeed a universal constant, then $t' = t$ is obviously correct. Good's argument is correct when considering *apparent* distances from the Cartesian zero point. However, finding numbers for y and y in this case, the values would differ, as Good states. The second Lorentz equation would then not apply!

What we see, or find by other means of observation, is not the objective truth. We treated this question in some detail in our article "Apparent and assumed real changes of moving objects"[4]. We may mention as a matter of interest only that about 2500 years ago, Protagoras[5] taught that "sense perceptions are all that exists. Reality may be different from one person to another." A distinction similar to ours, but a different conclusion. It may be worth mentioning also that Einstein[6] himself referred to difficulties arising when domains of validity of two inertial systems overlap. "Without committing a fundamental error, we can disregard the fact that in reality these frameworks would continually interfere with each other, owing to the impenetrability of solid bodies." He dismissed this as not significant—an error we did not mention.

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Laszlo Szego
Peter F. Ofner

Clock rates in special relativity

Phipps (1997) asks why do clock rates (in the special theory of relativity, STR) lack group properties? One could reply with the question why should they? But velocities form a group in 1 + 1 dimensions as Einstein stated in 1905. An easy way to see this is by noting that "rapidities" are additive, where the rapidity corresponding to a velocity v is defined as $\tanh^{-1}(v/c)$ (see, for example, Eddington, 1923/30, p. 22). It is less clear whether velocities form a group in more dimensions, but I will leave that matter aside because Phipps argues that group properties fail even when there is only one spatial dimension. I claim that his argument is incorrect, although his conclusion is correct, as I show in a separate communication submitted together with this one (Good 1997). Nevertheless Phipps is an imaginative physicist with a lively style. Einstein also made mistakes!

Phipps considers three clocks in inertial systems 1, 2, and 3. The spirit of his argument can be stated thus: clock 2 runs slower (with relative rate less than 1) than clock 1 as measured in system 1, and clock 3 runs slower than clock 2 as measured in system 2 (also with relative rate less than 1). He then argues that the relative clock rate in system 3 as measured in system 1 ought to be the product of the two relative rates just mentioned, and he quickly reaches a contradiction. But this step in the argument is incorrect. For, by the same logic we could argue as follows, by identifying system 3 with system 1: Clock 2 runs slower than clock 1 as measured in system 1, and clock 1 runs slower than clock 2 as measured in system 2. "Therefore" clock 1 runs at the product of these two relative rates as measured in system 1 and this is clearly absurd. Indeed this would be roughly the essence of the fallacies of Herbert Dingle (1972) against the self-consistency of STR. Dingle's argument was worse because it was, at least sometimes, based on omitting the qualifications "as measured in system 1" and "as measured in system 2" thus denying STR a fair trial. Four of Dingle's closely related arguments were carefully and briefly refuted in Good (1991). I considered them separately for the sake of clarity.

Now Phipps says "clock rates might be a reliable aspect of physical 'reality'." Indeed, I have direct and close observation of my clock so its tick-tockery is clearly physically real if I am not hallucinating. But my clock measures only one of the coordinates in my spacetime system. If an observer Jane uses a different coordinate system her temporal coordinate can disagree with mine as measured in *any* inertial system. The ordinary intuition of us humans is based on the fact that we don't travel very fast, just as the flat-earthers didn't travel very *far*. Arguments based on this ordinary intuition are unreliable. (As in statistical practice, data can appear to be consistent with a "null hypothesis" even when that hypothesis is not *exactly* true. In the present situation the "null hypothesis" is that space and time are not intertwined which is a good assumption in *ordinary* life.) My clock keeps good time and all "inertial observers" will know that it gives my "proper" time as defined below. This was the meaning of Minkowski (1908/23), p. 85, when he considered the integral

$$\frac{1}{c}\int (c^2 dt^2 - dx^2 - dy^2 - dz^2)^{1/2} \quad , \qquad (1)$$

computed in the coordinate system of an "inertial observer", along *my* world-line from any fixed starting-point P_0 to a variable end-point P. This integral is the definition of the proper time of the point P. The idea of this definition is that the integral, over *my* world-line, has the same value (invariant) as measured by every inertial observer interested in *my* clock. Moreover, in my coordinate system (in which I am at rest by definition even if my system is *not* inertial), the integral again measures the time on my clock (because in my system $dx = dy = dz = 0$). Although the English text of Minkowski's lecture is by no means literally clear, there can be no serious doubt of his meaning. The clock rate of my clock, if reinterpreted as "proper time", does have an invariant ("absolute") meaning in STR. So Phipps's requirement that "clock rates ought to be a reliable aspect of physical 'reality'" is satisfied in the sense of proper time.

Phipps, with beautiful irony, implies that a relativist is liable to shoot himself in the foot, and this is true if he perpetrates a somewhat Dinglian fallacy, as in Section 2 of Phipps's paper. Suppose now that the observer Jane is in an inertial system and is moving relative to me. If the relativist says that my clock appears to Jane to be running at the same rate as her clock, then he would be putting his foot in his mouth. Therefore, if he also shoots himself in the foot he is in deep trouble. As the saying goes, two Wongs don't make a White.

When Jane evaluates Minkowski's integral (1) over my world line, using *her* coordinate system, she discovers the elapse of time recorded on *my* clock, not on *hers*. Indeed the integral is obviously smaller than the elapsed time on her clock because $dx^2 + dy^2 + dz^2$ is positive.

Note that the Minkowski integral (1) leads to the time dilation formula

$$(t - t_0)\, c \div \int_{P_0}^{P} \begin{pmatrix} c^2 dt^2 - dx^2 - \\ -dy^2 - dz^2 \end{pmatrix}^{1/2} \qquad (2)$$

and this is more general than the very familiar formula

$$(1 - v^2/c^2)^{-1/2} \qquad (3)$$

which applies when *both* systems are inertial with relative speed v. If my travels bring me from Jane back to Jane, in which case I cannot have been in an inertial system throughout, then my clock will lag behind hers. This is the familiar clock paradox treated by Minkowski's method. The Minkowski integral is a convenient tool for the resolution of a more general clock paradox. (See, for example, Good 1994, Sec. 6).

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I.J. Good

Clock rates (2)

Thomas Phipps' (*Apeiron*, Apr.-July 1997) unusually naïve question has a trivially simple answer: *because clock rates to not depend on uniform transport velocity* relative to inertial frames of reference! Phipps' scenario is that of Einstein (1905): a) clock mechanisms and/or principles of functioning are unspecified; b) clock rate is "kinematic" and it could depend on uniform velocity, rather than on acceleration of dynamic origin; c) "pure kinematics" means neglect of gravity (but not of electromagnetic fields, *etc*.); d) relative motions are one-dimensional; e) the velocity composition law is hyperbolic and is unrealistic to the same degree.

In his search for a "reliable aspect of physical reality", Phipps missed the one-dimensional (longitudinal) Doppler effect:

$$\frac{l_2}{l_1} = \left(\frac{1+v_{12}/c}{1-v_{12}/c}\right)^{1/2} \equiv T_{12}$$

which does show group properties, like $T_{13} = T_{12}T_{23}$, with

$$v_{13} = \frac{v_{12}+v_{23}}{1+v_{12}v_{23}/c^2} .$$

However, the arithmetic mean

$$\frac{T_{12}+T_{21}}{2} = \left(1-\frac{v_{12}^2}{c^2}\right)^{-1/2} \equiv g_{12} ,$$

falsely identified with the "time dilation" factor, does not display group properties. *One of the most blatant errors in the special relativistic literature is the identification of this transversal Doppler factor with time dilation .* One cannot overemphasize the difference between the illusory "time dilation"—pertaining, supposedly, to clocks' *internal dynamics*—and the external changes in the wave characteristics $\vec{k}$ and w .

George Galeczki
Society for the Advancement of Physics
Flittarder Hauptstr. 22
D-51061 Cologne, Germany

Phipps replies

Yes, my critics [1,2] are right … my discussion [3] of the apparent failure of clock rates to display group properties was vulnerable to objection from both dissident and orthodox sides. My main mistake (pointed out also by M. H. Brill and by D. P. Allen, Jr. in private correspondence) was mixing into a meant-to-be relativistic account an element of absolutism, in the form of an assumption that clock rates have an absolute physical meaning. The resulting apparent contradictions do not impugn the self-consistency of special relativity theory (SRT) but only of my own logical processes. It turns out that the main mistake just mentioned is very easily corrected by a simple notational reform, using two-index symbology for clock rates instead of one-index. So, let us clear that up first.

Previously [3], to review, I assumed (for clocks in collinear motion and subject to time dilatation) a relationship $R[i] = R[j]\sqrt{1-v_{ij}^2/c^2}$ between "absolute" clock rates $R[i]$ and $R[j]$, where *i* and *j* label inertial systems and v_{ij} is the velocity of system *i* as measured by instruments at rest in *j*. (This reflected my interpretation of time dilatation as expressing a physically factual asymmetry of clock rates, as suggested by evidence [4] of muon "clock" motion relative to the CERN laboratory. That is, I sought to save the phenomenon rather than the theory—an approach guaranteed to save neither.) It immediately got us into logical trouble via $R[2] = R[1]\sqrt{1-a^2}$, $R[3] = R[2]\sqrt{1-b^2}$, $R[3] = R[1]\sqrt{1-d^2}$, the last two expressions for $R[3]$ being mutually contradictory, in view of the definitions $a = v_{21}/c$, $b = v_{32}/c$, $d = v_{31}/c$, even without invoking a velocity composition law such as that of Einstein, *viz.*, $d = (a+b)/(1+ab)$.

Clock rates treated as relative

Now, instead—using a two-index notation to describe an assumed "relativity of clock rates"—we may express accepted time dilatation by $R[i,j] = R[j,j]\sqrt{1-v_{ij}^2/c^2}$, where $R[i,j]$ is the rate of clocks at rest in system *i* as measured by clocks at rest in *j*, and $R[j,j]$ symbolizes the intrinsic or self-measured rate of clocks at rest in system *j*. (If "rate" connotes a frequency, $R[j,j]$ might be proportional to a transition frequency of the stationary cesium atom.) We have at once, $R[2,1] = R[1,1]\sqrt{1-a^2}$, $R[3,2] = R[2,2]\sqrt{1-b^2}$, $R[3,1] = R[1,1]\sqrt{1-d^2}$. These relations are not mutually contradictory, but leave us with three as yet undetermined quantities, $R[1,1]$, $R[2,2]$, $R[3,3]$.

Theorem on intrinsic clock rates

The three quantities just mentioned will next be shown to be all numerically equal, provided we assert the clock rate symmetry $R[i,j] = R[j,i]$. Such an assertion appears to be a valid expression of the relativity principle, in harmony with SRT's velocity reciprocity. If so, we easily verify the

Theorem. In SRT all inertial clocks run at the same intrinsic rate; i.e., $R[1,1] = R[2,2] = R[3,3]$.

Proof: Taking the ratio of $R[3,3]\sqrt{1-b^2}$ to $R[1,3] = R[3,3]\sqrt{1-d^2}$, and remembering that in SRT $v_{ij} = -v_{ji}$, we obtain by applying rate reciprocity, $R[i,j] = R[j,i]$,

$$\frac{R[2,3]}{R[1,3]} = \frac{\sqrt{1-b^2}}{\sqrt{1-d^2}} = \frac{R[3,2]}{R[3,1]} = \frac{R[2,2]\sqrt{1-b^2}}{R[1,1]\sqrt{1-d^2}} \to R[2,2] = R[1,1] ,$$

etc.

This is new as a proven result, as far as I know, although it is often tacitly assumed.

Interpretation of theorem

One can only speculate on what the theorem means physically. It could mean that cesium atoms have the same frequency in all inertial systems. Or, it could mean that a standard reference clock agrees with any and all permanently inertial clocks with which it may co-move. Or, it might have no meaning at all. The attempt to assign it physical meaning, be it noted, encounters immediately a fundamental difficulty. Since SRT pays no attention to *calibration* of spatial coordinate axes or clocks, how could the assertion of equality of clock running rates or of meter sticks be physically meaningful? Calibration would have to be done separately for each axis, in lack of a technical device that can serve as a unified reference standard for "spacetime" mensuration: No instrument, no measurement, no calibration. Separate axis calibration seemingly requires accelerative transfers of time and space metric standards. But acceleration is forbidden, or at least it plays no overt role in SRT. In the absence of calibration, how can "measurements" yield numbers? Or, how can the equality of "measured numbers" asserted by our theorem mean anything *physically* at all?

Bergmann [5] (p. 33) deals with this (or rather, fiddles with it) by observing that the Galilean transformation equations "can be replaced by new equations which are not based on the assumptions of a universal time and the invariant length of scales, but which assume at the outset the invariant character of the speed of light." He also says (p. 36) that "neither the unit length nor the unit time is directly comparable in S and S*" [two inertial systems]. What this amounts to is that we do not need to calibrate either space or time axes individually or separately, but can get away with just assuming "at the outset" universal constancy of the *ratio of their measures*.

Talk about living dangerously! Put baldly, in that way, it is quite a philosophic pill to swallow, is it not? Minkowski would gild it by saying that separate space and time calibrations are *metaphysical nonsense*, because the physical distinctions between these entities have "faded away." The pure-blooded operationalist, on the contrary, would say that anything for which no measuring device can be conceived is *metaphysical nonsense*. Interesting, is it not, that Einstein's "rods and clocks" earned him credit for operationalism with Bridgman (operationalism's pioneer), who held Einstein's coat and cheered him on? Physicists tend to "fade away" from the vicinity of metaphysical disputes [6], but this does not mean that they are smarter than philosophers; rather, that they are no less dumb than mathematicians—in that all they really take seriously about SRT is the equations of the Lorentz transformation. *As a logical construct relevant to physics* that transformation is no better grounded than is our present theorem (above). The upshot is that the theorem is a nice bit of formalism, but that it cannot be unambiguously pinned-down to having a verifiable physical meaning independent of the theory that engenders it. It is in fact no better nor worse than, but is much of a piece with, the rest of SRT.

What say the CERN clocks ?

Let us suppose (as is generally done, although this has been challenged, *e.g.*, by Wesley [7]) that a cloud of comoving mesons can serve as a *clock* that will agree as to average running rate with an idealized "genuine" clock (cesium clock , biological clock, *etc.*) sharing the same state of motion. Consider the CERN muon cloud [4] in high-speed motion ($g = \left(1 - v^2/c^2\right)^{-1/2} \approx 29$) in its circular orbit, and suppose that initially another similar cloud sits at rest in the laboratory inertial system S at some point adjacent to the orbit. The half-life of the latter is so many microseconds of laboratory time, and that of the high-speed cloud is observed [4] to be some 29 times longer than that. So, the clock rates represented by the two clouds are in objective fact asymmetrical by a factor of 29.

Now, as the moving cloud departs from the stationary one, the former may be considered instantaneously at rest in some comoving *rigorously inertial* system S'. The proper-time clock of the orbiting cloud presumably *agrees in rate* with the permanently resident clocks at rest in S'. (See my previous discussion [3] of clock rate as a state function.) To make things symmetrical, we shall suppose that there is another cloud of muons at rest in S'. Let us proceed by formal application of the Lorentz transformation, to be sure of making no mistakes. Consider motion along coincident x,x'-axes of inertial systems S, S'. System S' moves with constant velocity v in the direction of positively increasing x. Let all three muon clouds mentioned above be each initially identically populated with $N^{(1)}$ muons. All observers in whatever state of motion agree on this nose count. Event 1 is the triple coincidence of these three clouds, the circling-muon cloud, the permanently S'-stationary cloud, located at $x'=0$, $t'=0$ in S', and the cloud stationary at the laboratory observer's origin, $x = 0$, $t = 0$.

Event 2 occurs at position $(x,t) = (0,T)$ in S at the next circling-back to the laboratory origin of the non-inertial "clock" (the orbiting muon cloud whose motion, once each period of lab-duration T, is momentarily tangential to the x-axis at the S-origin). For the time being we do not seek to treat further the motion of the third (non-inertial) cloud, but keep its presence in mind for later remark. Applying the Lorentz transformation [5],

$$x' = \frac{x - vt}{\sqrt{1 - v^2/c^2}}\ ,\ t' = \frac{t - vx/c^2}{\sqrt{1 - v^2/c^2}}\ ,$$

to the motion of S' relative to S, we find that $(x',t') = (-gvT, gT)$. At Event 2 both the lab-observer and the S' observer (located in S' at position $x' = -gvT$) count $N_S^{(2)}$ survivors of the S-stationary cloud and $N_{S'}^{(2)}$ survivors of the S'-stationary cloud. Let us note the conditions of compatibility of these counts. We have seen that the elapsed time between Events 1 and 2 is $t = T$ in S and $t' = gT$ in S'. (It would be better to show these as Δt-increments.) We might as well write our relation between Events 1 and 2 (both of which occur at $x = 0$) more generally as

$$t' = gt\ . \qquad (1)$$

If t is the mean decay time of a muon cloud at rest (a constant of nature), then our hypothesized lab-stationary muon cloud, as viewed in S, obeys

$$N_S^{(2)} = N^{(1)} e^{-t/\mathrm{t}}\ , \qquad (2)$$

by the ordinary rule of radioactive decay. This same S-stationary cloud, when viewed in S', obeys by substitution for t from Eq. (1)

$$N_S^{(2)} = N^{(1)} e^{-t'/g\mathrm{t}}\ . \qquad (3)$$

The "forward" Lorentz transformation just employed is appropriate for describing the rate of a clock at rest in S, as viewed from S'. To describe the muon cloud at rest in S', as viewed by observers at rest in S, it is simplest (though not necessary) to apply instead the "inverse" Lorentz transformation

$$x = \frac{x' + vt'}{\sqrt{1 - v^2/c^2}}\ ,\ t = \frac{t' + vx'/c^2}{\sqrt{1 - v^2/c^2}}\ ,$$

expressing unprimed coordinates as functions of primed—see Bergmann [5], p. 38. The result is simply a role reversal in Eq. (1); namely,

$$t = gt'\ .\ \text{(inverse)} \qquad (4)$$

This yields expressions that are duals of those just derived. It is convenient to gather all these expressions together for comparison:

Laboratory-stationary cloud:

$$N_S^{(2)} = N^{(1)} e^{-t/\mathrm{t}}\ \text{ viewed in } S \qquad (5a)$$

$$N_S^{(2)} = N^{(1)} e^{-t'/g\mathrm{t}}\ \text{ viewed in } S' \qquad (5b)$$

S'-stationary cloud:

$$N_{S'}^{(2)} = N^{(1)} e^{-t'/\mathrm{t}}\ \text{ viewed in } S' \qquad (5c)$$

$$N_{S'}^{(2)} = N^{(1)} e^{-t/g\mathrm{t}}\ \text{ viewed in } S. \qquad (5d)$$

Now let us return to our "third cloud" of muons, the one in circular uniformly-accelerated motion of period T in S, identifiable with the CERN laboratory. This non-inertial cloud maintains a constant orbital speed v in S. By the symmetry of circular motion (and space isotropy) in the laboratory, the rate of muon decay (clock running), judged in S, cannot depend on azimuthal angle of the motion. Thus at all azimuths the instantaneous "running rate" of these muon clocks is the same and constant. We can sample this running rate at any azimuth and be assured it is the same there as throughout the orbit. In particular, where the orbit is tangential to the x-axis at the origin of system S, if the rate is sampled there, that instantaneously-measured rate can be assumed constant throughout the duration of the circular motion. When the muon cloud permanently at rest in S' briefly co-moves with the orbiting third cloud, their momentarily identical states of motion assure their equal instantaneous running rates as timekeepers. (State function assumption!) And, since the running rate of each of these two clouds is constant in time (the S'-stationary clocks because they are inertial, the third-cloud clocks by the argument from azimuthal symmetry just given), it follows as a fact of empiricism that these two clouds (as clocks) run at equal rates and are *statistically equivalent timekeepers*. This deducing of equality of average rates of the S'-stationary and circling muon clouds should not be surprising, since the circling clocks and the translating clocks both have speed magnitude $|v|$ relative to the lab inertial system, and clock rate depends only on g , thus on v^2, which is the same for both muon clouds.

Strictly, the above argument should be stated with the qualification "as seen in S," but since all observers must agree on the *number of survivors* of the circling cloud at each of the return-to-S-origin events, it follows that, even in inertial systems in which the azimuthal symmetry argument fails, the *average running rate* of the orbiting clocks, averaged over an integral number of orbits, must be an absolute invariant, agreed upon by all observers, including those of general relativity. And that absolute invariant must be the same as for the "as seen in S" case. To repeat: The circling muon clocks run *on the average* at the same rate as clocks permanently at rest in S' … and this sameness has to be an absolute fact in the view of all observers. It represents an identity of "intrinsic" rates.

So, the average decay-rate of the non-inertial "third-cloud" orbiting clocks must agree with that of the inertial S'-stationary clocks. This, by Eq. (5d), should correspond to a mean decay time of gt as viewed in S. That seems to imply a low decay rate of the circling muons, which is commonly taken to agree with the "staying young" of space travelers. But the latter is an intrinsic (or proper) effect; whereas the (5d) effect is an artifact of phase settings of synchronized clocks all running intrinsically at the same rate. This is ominous, to say the least. There is clearly no problem with Eq. (5a) or (5c), which are trivial. There appears to be a problem with Eq. (5b), but this claimed "staying young" of the lab-stationary muons is again an artifact of clock phase changes, due to changing x'-position of the *S*-clock in S', as dictated by the Lorentz transformation and the synchronism convention in S'. Artifacts of convention can be ignored when they prove inconvenient [Eq. (5b)] and accepted as physics when needed [Eq. (5d)]. Such, such are the joys of theory beautified by pure logic.

The problem

As I see it, there remains one fatally serious difficulty: We have shown that the non-inertial clocks of the actual CERN muon cloud and the inertial S'-clocks run continually at the same average "intrinsic" rate. That has to be viewed as tantamount to an empirical fact. The orbiting CERN clocks are *observed*, therefore the S'-clocks are inferred, to run 29 times slower than the lab-stationary *S*-clocks. That, too, is essentially an empiricism. Yet, we have exhibited a "theorem," seemingly in perfect harmony with the relativity principle and with SRT, to the effect that the intrinsic running rates of *S*-clocks and S'-clocks are *the same*. This symmetry issue is not a logical paradox but apparently marks a flat contradiction between theory and observation.

That the CERN evidence contradicts SRT has hitherto been hidden by the reluctance of theorists to accord any meaning to "intrinsic" clock rates (although proper time is a recognized kinematic invariant). The accepted SRT canon has rested (whether this was recognized or not) on the Bergmann dictum of incomparability of unit lengths and times in *S* and S' (*i.e.*, there are things in heaven and earth about which we are meant to know nothing). But, as we have seen, the relativity principle asserts such a strong symmetry among inertial systems that the incomparability doctrine cannot be consistently maintained. And even if it could be, that would leave SRT an incomplete system of kinematic thought (specifically, incomplete in respect to axis calibration). To term an immaculate conception incomplete is to sully it as surely as to term it wrong.

Finally, the problem, being endemic to flat space, is not to be solved through appeal to general relativity theory—(a) because the latter reduces to SRT in the flat-space limit, and (b) because acceleration plays no overt role (*i.e.*, the "problem" concerns strictly inertial systems, S, S'). But of course there are logical links that may be faulty, such as my assumption that clock rates are a state (of motion) function, that muons are clocks, *etc.* I should value further criticism or enlightenment.

References

[1] I. J. Good, *Apeiron* **4**: 127

[2] G. Galeczki, *Apeiron* **4**:127

[3] T. E. Phipps, Jr., *Apeiron* **4**, 81 (1997).

[4] J. Bailey *et al.*, *Nature* **268**, 301 (1977).

[5] P. G. Bergmann, *Introduction to the Theory of Relativity* (Prentice-Hall, N.Y., 1946).

[6] However, it may be noted that an international committee composed (I presume largely) of recognized physicists has seen fit to define "*c*" as a pure numeric—so, the Minkowskian metaphysic has been swallowed hook, line, and sinker by the priests of the measurements temple.

[7] J. P. Wesley, *Causal Quantum Theory* (Benjamin Wesley, Blumberg, 1983), p. 172.

Thomas E. Phipps, Jr.
908 South Busey Avenue
Urbana, Illinois 61801

Fairwell Xu^2

Shaozhi & Xiangqun (1997) produce three arguments to support their expression "Fairwell Relativity". I will explain here why their first argument is not entirely fair, and why their other two arguments are fallacious. In addition, they express approval of an argument by G. Walton which is also fallacious, as I show in a separate letter.

In Part (1) they refer to the invariance of the relativistic (squared) interval $x^2 - c^2t^2$ from the origin of coordinates to an event (x, t). ($y = z = 0$ is assumed.) This invariance can be called pseudo-orthogonality because it is formally the same as orthogonality except that *ict* is imaginary. Of course there are an infinity of pseudo-orthogonal transformations but Xu^2 say that the Lorentz transformation is assumed (by somebody) to be the sole [linear] coordinate transformation that would lead to the invariance of the interval. They don't cite "chapter and verse" for this assumption but perhaps they have in mind the footnote on page 46 of Einstein (1905/23). Taken literally that footnote does say what Xu^2 cite. But there must be some constant velocity v of one system relative to the other one since they are both inertial, and perhaps Einstein had that in mind but he wasn't explicit about it. This is sufficient to deduce the Lorentz transformation in 1 + 1 dimensions as I now prove.

The most general 2×2 pseudo-orthogonal transformation from $(\Delta x, \Delta\tau)$ to $(\Delta x', \Delta\tau')$, where $\tau = ict$ and $\tau' = ict'$, is of the form of an "imaginary rotation" (λ being real)

$$\Delta x' = \Delta x \cos(i\lambda) + \Delta\tau \sin(i\lambda)$$
$$\Delta\tau' = -\Delta x \sin(i\lambda) + \Delta\tau \cos(i\lambda)$$

which can be written "really" as

$$\Delta x' = \Delta x \cosh\lambda - \Delta(ct).\sinh\lambda$$
$$\Delta(ct') = -\Delta x \sinh\lambda + \Delta(ct) \cosh\lambda$$

for some λ. (The form suggested by Xu^2 is less elegant than this familiar form.) The points fixed in the "primed" system occur where $\Delta x' = 0$ and such points should have velocity v in the unprimed system. Hence $v/c = \tanh\lambda$. Thus λ is the "rapidity" $\tanh^{-1}(v/c)$, and the transformation is unique. The transformation used by Xu^2 can be seen to reduce to the Lorentz transformation if their symbol B satisfies $Be^{-B} = v/c$. They must have overlooked this.

In Part (2) Xu^2 object to the choice $(x_0, t_0) = (0, 0)$ for a "stationary" system and implicitly to that of $(x'_0, t'_0) = (0, 0)$ for the "moving" system; that is, to the choices of the origins of coordinates in the two systems. They think these choices force the relative velocity of the two systems to be zero but that is a mistake. It is of course quite possible for two observers to share an origin of space-time coordinates though they are in relative motion. They could set their clocks to zero when they are coincident in space. There is no problem at all when there is only one spatial dimension, as here.

In Part (3), which is more subtle, Xu^2 consider two *distinct* particles (I emphasize that they are distinct) having world-lines

$$y' = Ut' \text{ and } y = Ut$$

in the two systems, where U denotes a velocity along these axes. Xu^2 then quote the identity $y' = y$, from the usual representation of the Lorentz transformation, to show seemingly that $t = t'$. Their mistake this time is in thinking that $y' = y$ implies that the y and y' axes are the same! The Lorentz transformation is a transformation between two coordinate systems and applies to each point event. It cannot be applied to two distinct events. Fairwell Xu^2, Hello Relativity!

References

Einstein, A. (1905/23), English translation in the anthology *The Principle of Relativity* (Dover Publications Inc.), 37-65.

Shaozhi, Xu & Ziangqu, Xu (1997) *Apeiron* 4, p. 86 second letter on the page.

I.J. Good

Reply to Good: Spurious Math

We offer with pleasure this reply to the challenge from I.J Good.

I. It is not true when Good says that the current LT

$$x' = \gamma(x - vt)\,,\ t' = \gamma(t - vx/c^2)\,; \qquad (1a)$$

$$y = y,\ z = z, \qquad (1b)$$

where $\gamma = \sqrt{1-\beta^2}$, $\beta = v/c$, or its differential form

$$dx' = \gamma(dx - v dt),\ dt' = \gamma(dt - c dx/c^2)\,; \qquad (1a)'$$

$$dy' = dy \quad dz' = dz\,, \qquad (1b)'$$

"is unique," even if no problem "λ is … $\tanh^{-1}(v/c)$." There exist other forms comparable to (1), such as (y', z' are omitted)

$$x' = \gamma(x - vt)\,;\ t' = \gamma(vx/c^2 - t)\,.$$

In addition, dividing the first equation by the second in (1a)' yields $dx'/dt' = v' = (dx - v dt)/(dt - v dx/c^2)$, *viz.*, $dx - v dt = v' dt - v' v dx/c^2$; then $v(dt - v' dx/c^2) = dx - v' dt$, *viz.*, $v = (v - v')/(1 - v'v/c^2) \neq v$ (or $-v'$), unless $v = v' = 0$.

Thus the LT itself simply cannot preserve the specified speed v constant.

II. It is clear that Eq. (A) does not contain the symbol v, and hence the PIVL is a purely human extra-convention imposed on Eq. (A) and on the relativistic 4-D space-time represented by Eq. (A).

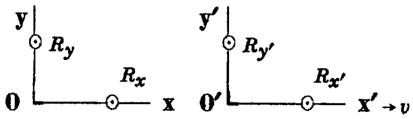

Fig. 1

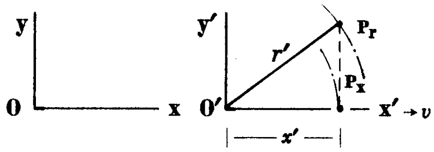

Fig. 2

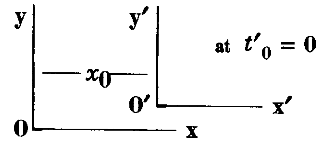

Fig. 3

Also, the LT cannot, false or not, be derived from Eq.(A) *alone,* because the parameter v in it does not appear in Eq. (A).

How can the v enter into (la)? In the final analysis, the LT originates from the set of prerequisites, in the simplest case:

$$x - ct \equiv 0 \quad (\text{and } x' - ct' \equiv 0) \qquad (2a)$$

$$x - vt = 0 \quad (\text{and } x' + vt' = 0) \qquad (2b)$$

(2a) implies the PIVL, and (2b) relative motion of two (inertial) frames. Then, can (2a,b) as prerequisites coexist? The answer must be in negative because (2a) and (2b) are incompatible unless $v = c$ or $v = 0$.

Another way to argue, (2a',b) may be rewritten in differential form as

$$\mathrm{d}x - c\mathrm{d}t \equiv 0 \text{ viz., } c = \mathrm{d}x/\mathrm{d}t, \qquad (2a)'$$

$$\mathrm{d}x - v\mathrm{d}t = 0 \text{ viz., } v = \mathrm{d}x/\mathrm{d}t, \qquad (2b)'$$

to which we ask: $\mathrm{d}x/\mathrm{d}t$ refer to which one, v or c?

If (2a,b) are assumed to represent motions of two independent events, respectively, they should, to avoid possible confusion, be expressed as

$$x_1 - ct \equiv 0 \quad (\text{and } x_1' - ct' \equiv 0)$$

$$x_2 - vt = 0 \quad (\text{and } x_2' + vt' = 0 .$$

Then neither LT-L nor LT-E, which refer to the LT interpreted by Lorentz and Einstein, respectively, can be arrived.

III. If you set, say, 1 = 3, one may find a relation to make two unequal numbers you arbitrarily give, say 8, 2000, exactly equal:

1 × 996 – 988 = 3 × 996 – 988 (note 1 = 3) yields 8 = 2000. *q.e.d*. Another example, for 6 = 24, and 11029 = 243:

24 × 599.22 – 3352.33 = 6 × 599.22 – 3352.33 (note 6 = 24) yields 11029 = 243. q.c.d.

No wonder: if unequaled N and M are set to be equal, one can find

(A1) $Y = (N-M)^P (Y-X) / (N-M)^P + X$

(A2) $X = (M-M)^P (Y-X) / (N-M)^P + X \equiv Y$

(note $N=M$)

whatever X, Y may be, where P is an arbitrary constant including 1/2.

Resorting to such 0/0 *type* backdoor (note: $N - M = 0$ if set $N = M$), one may *cook up* inadmissible transformations of every hue such as the LT to meet any condition at his (her) desire. This is just why almost every top fashionable theory of today puts all its eggs in 0-point math basket!

To set tanh $^1(v/c)$ is to put the forms listed by Good into such basket, because (2a,b)' implies:

$$1 - (v/c)^2 = 1 - (\mathrm{d}x/\mathrm{d}t)^2 / (\mathrm{d}x/\mathrm{d}t)^2 = 0$$

Briefly, the LT is spurious math! The proof of pudding is in eating.

1) By use of the LT, (1a), one can easily show [3a] that:

STONE$'^2$ EGG$'^2$ = STONE^2 c^2EGG^2, which proves the LT itself non-math, the reason is: both stone and egg are not numeral [we are of course using them *per se,* here] and cannot form or meet any quantitative relation; yet, replacing (x,t) by them, the LT still meet a quantitative relation, Eq. (A) or any else, so that proves (x,t) just as (egg, stone) not variables in quantity sense. Note: it is wrong to regard the (x,t)-(egg, stone) as one-rose-two-names.

The belief that a specific equation must have meaning because some quantitative attribute(s) is (seemingly) assigned to it is naive.

2) Eq. (1a) says that two clocks on x'- and x-axes (see Fig. 1, where z-axis is omitted) should have different time-rates:

$$R_{x'} \neq R_x ; \qquad (3a)$$

while (1b) says clocks on y'- and y-axes have an identical time-rate:

$$R_{y'} \equiv R_y . \qquad (3b)$$

On the other hand, all clocks in the same frame can be synchronized and hence should have identical time-rate, that is:

$$R_{x'} \equiv R_{y'} ; \quad R_x \equiv R_y .$$

Substituting the above two into (3a) yields

$$R_{y'} \neq R_y ,$$

in conflict with (3b)! That is, (1a) is simply incompatible with (1b).

The above, which is just our issue on $y' = Ut'$ despite different words, should convince Good that the LT cannot apply to even *a single thing,* the "time-dilation," irrespective of "distinct events." If not, it seems necessary for Good to make it clear: What does it mean by $y' = y$ y in (ib)?

Good's improper comment seems to tell us that the evil trend of dogma stirred by Relativity has terribly eroded the science field. The LT is assumed to apply to any event r in 4-D world (Fig. 2, where z-axis is omitted) except the alleged space-like region:

$$P_r(x', y', z', t_r') ; \quad t_r' = \frac{r'}{c} = \frac{\sqrt{x'^2 + y'^2 + z'^2}}{c} .$$

Yet, what (1a) represents is any of such events only:

$$P_x(x', 0, 0, t_x') ; \quad t_x' = x'/c \neq r'/c$$

because: i) in (1a), t' is a function of x and t only and *irrelevant* to variables y, z (or y', z'); *viz*., (la) can apply to any point along x-axis only, P_x; not to P_r not along the x-axis; ii) the interval t_r' a light signal takes to travel $O' = P_r$ (or $\frac{1}{2}[O' - P_r - O']$, as in Ref. 2) differs from t_x' the light signal takes to travel from O' to P_r, the projection of P_r on x'-axis, unless the event happens along x-axis.

In other words, (la) makes (1b) impossible unless (1b) turns into $y' \equiv y \equiv z' \equiv z \equiv 0$, regardless of any assumption. The LT is simply disqualified as a coordinate transformtion, because x-axis alone cannot form a spatial 3-D frame.

III. The LT derivation is flooded with errors and flaws. Here we only deal with two related to the last disproof above, as listed below.

Step Equation(s) Used

(B1) $x'^2 + y'^2 + z'^2 - c^2 t'^2 \equiv 0;$ $x^2 + y^2 + z^2 - c^2 t^2 \equiv 0.$;

(B2) $x'^2 + y'^2 + z'^2 - c^2 t'^2 \equiv x^2 + y^2 + z^2 - c^2 t^2$

where $t' = t_r' = \sqrt{x'^2 + y'^2 + z'^2} / c;$ $t = t_r = \sqrt{x^2 + y^2 + z^2} / c$

(B3) $x'^2 - c^2 t'^2 \equiv x^2 - c^2 t^2$

where $t' = t_x' = x'/c;$ $t = t_x = x/c$

Comment: An error is introduced on the sly in that: Eq. (B3) differs from (B2): in (B2) the time variable is t_r' or t_r ; in (B3), however, it is t_x' or t_x .

This error is introduced by adopting ambiguous symbol t for both t_r' and t_x' (and t for t_r , t_x); it is somewhat related to the error below. Taking account of $0 = l\,0$, (B1) should give:

$$x'^2 + y'^2 + z'^2 - c^2 t'^2 \equiv l \left(x^2 + y^2 + z^2 - c^2 t^2\right)$$

There is no reason to take $l = 1$, though an argument for it is offered by Einstein, *et al*. (but is invalid, see *e.g*., Ref.4). Be careful, scientists!

IV. What we object to is the inadmissible model that allows of no free choice for (x_o, t_o); not the choice of (x_o, t_o) = (0, 0) as Good said.

It is well known that a free choice of initial condition, (x_o, t_o), is the *sine qua non,* uncompromisable, for any coordinate transformation.

Yet, if set $x_o \neq 0$ at $t_o' = 0$, *as* shown in Fig. 3, then: i) no light signal can be sent off from both origins O and O', simultaneously, as the Einstein model requires; ii) one should have $t_o = -x_o/c$ (the minus sign implies t_o prior to t_o'); yet, (la) gives $t_o = vx_o/c^2 \neq -x_o/c$ ..

It can be seen that both incompatibility of (2a, b) and lack of free choice for (x_o, t_o) imply, and stem from, physical impossibility of PIVL. Faced with the false LT it is ironic to speak of "evidence" for the PIVL.

In conclusion, the false LT is good for nothing except as evidence that mathematics, or linear algebra in particular, is not fully understood.

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Xu Shaozhi and Xu Xiangqun
Beijing Control Device Research Institute
PO Box 3913, Beijing 100854
P.R. of China

Untrained intuition and the special theory of relativity

Walton (1996) says "The assumption that $v' = -v$ ... is responsible for the paradox that, if we put $ct' = ct(1 - v/c)$ and $ct = ct'(1 + v/c)$; ct seems to have become contracted." Here v is the velocity of an inertial system relative to another one, and v' is that of the first with respect to the second, while t and t' are the times at which a photon reaches some point on the x and x' axes (which are chosen in the direction of the relative velocity of the two systems).

If we rewrite the equation $ct' = ct(1 - v/c)$ as $ct' = (c - v)t$, we see that Walton is assuming that the composition of two velocities, say v_1 and v_2, along a straight line is the sum or difference of the two velocities. This of course is equivalent to an immediate denial of STR where the formula is $(v_1 + v_2)/(1 + v_1v_2/c^2)$ which, when $v_1 = c$, reduces to c, not to $c + v_2$. You can't disprove the self-consistency of STR by simple denial; nor by the untrained intuition that we have because we don't travel fast enough. It is precisely because the kinematics of STR contradicted untrained intuition that it was regarded as a revolutionary theory. It also explains why incorrect attacks on STR are produced again and again. I am confident that the kinematics of STR is self-consistent. Attacks on STR should be based on empirical evidence, not on untrained intuition. But the "flat-earthers" seldom give up.

Reference

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I.J. Good

Walton replies

In his reply to my letter (*Apeiron* 3, 1996, p.126), Dr. Good ignores that, for 'events' confined to the x-axis, the Lorentz transformation (LT) reduces to $x' = ct' = (c - v)t\left(1 - v^2/c^2\right)^{-1/2}$ [1], and that, by his own reckoning, the LT itself would be equivelent to an immediate denial of STR. The reason for this is, of course, that he falsely applies the formula for the composition of speeds in different systems to speeds in the same system; for lack of space I cannot here discuss in detail this very important distinction.

Mathematicians have come to recognize that only systematic recourse to diagrams and graphs can prevent apparently cogent steps the gross invalidity of which is obscured on reliance on symbolic manipulation; this applies no less to kinematics. Diagram shows at once that $v' \neq v$; it is true that the 4D model is unhelpful. The error is of a purely mathematical nature; empirical evidence and intuition do not enter.

Reference

[1] Einstein, A. (1960). *Relativity*, Methuen, p. 34.

G. Walton

Yet another obscure attack on special relativity

Campbell (1996) considers a rod L (or segment) on a long straight line (O'', O'), and two observers at O'' and O' moving relative to the rod with equal velocities v in the direction from O'' to O'. The observer at O' is ahead of the rod, and the observer at O'' is behind it. Light is transmitted in all directions simultaneously (in the inertial frame of reference in which the rod is at rest) from the ends (say A and B) of the rod, green from one end and orange from the other end. Campbell regards it as "evident" that "The observer at O' will perceive segment L to have expanded, and the observer at O'' will think the segment has contracted." This is not true in Newtonian physics (in which there is neither contraction nor expansion), nor according to STR (in which the length of the rod appears to change in the same way for both travellers; see below) so what makes it evident to Campbell? It must also be at least as evident to Shaozhi & Xsiangqun (1997) for they say emphatically that Campbell's letter is "ingenious, direct and clear, succinct and effective". Perhaps these three authors could enlighten me or *nobly admit error*. Are they by any chance assuming that the composition of velocities c and v differs from c? If so, they are denying the STR composition law $(u + v)/(1 + uv/c^2)$ which reduces to c when $u = c$ (cf. Einstein, 1905/23, p. 52). But I am only trying to guess Campbell's unstated explanation, or to preempt an incorrect one. Of course he might have *other* criticisms of STR but to introduce them into his reply would cloud the issue and I'm sure he wouldn't wish to do that.

Disestablishmentarians might think that the Lorentz transformation is self-contradictory. So let us now spell out, in terms of the transformation, why the rod appears to change in length in the same way for both travellers although one of them is approaching the rod and the other one is receding from it. This is obvious to a relativist because the travellers are at rest relative to each other, but the honorable members of the Opposition might need convincing.

Let (x_1, t_1) and (x_2, t_2) denote the space and time coordinates of A and B in the "stationary" system (frame of reference) at the moment when the light signals are transmitted. These two events are simultaneous in that system so $t_1 = t_2$. Let us use primed, and doubly primed, symbols for the coordinates in the systems of the travellers at O' and at O'' respectively. Then, from the Lorentz transformation, we have

$$x_2' - x_1' = \gamma[x_2 - x_1 - v(t_2 - t_1)] \qquad (1)$$

where $\gamma = (1 - v^2/c^2)^{-1/2}$. So

$$x_2' - x_1' = \gamma(x_2 - x_1) \qquad (2)$$

because $t_1 = t_2$. Similiarly, even if one is tempted to replace v by $-v$, we have

$$x_2'' - x_1'' = \gamma(x_2 - x_1) \qquad (3)$$

(Whether this represents a contraction or an expansion is only a semantic matter.) This verifies that the two travellers are in agreement about the length of the rod. Fortunately, we can't prove that $x_2 - x_1 = \gamma(x_2' - x_1')$ because the two events are not simultaneous in the travellers' systems.

References

Campbell, J.O. (1996). *Apeiron* **3**, 125-126.

Shaozhi, Xu & Xiangqun, Xu (1997). *Apeiron* **4**, p. 86, first of their two letters on the page.

I.J. Good

Campbell replies

This addresses Prof. Good's diatribe entitled "Yet another obscure attack on the special theory of relativity," which was directed toward my remarks: "Length Contraction and Time Dilation?" Since he only mentioned "length contraction," I must assume he was perfectly satisfied with what was said about "time dilation".

First, Prof. Good asserts "This is not true", without pointing out an error; and a few sentences later he asks to be "enlightened", because he can only "guess Campbell's unstated explanation". Therefore, I shall try to explain what was apparently obvious to other readers of *Apeiron*, as things are obvious to one only according to his gifts.

Let the *green* photon emitter be at the end of segment L nearest O' and the *orange* emitter at the end nearest O", and consider the moving situation in Figure 1(b):

When the *green* photon arrives at O', the *orange* photon will be a distance L behind, but will have to travel a longer distance (L + δ) before reaching O' (because O' is receding from it); clearly then, the observer at O' will measure L' > L. Likewise, when the *orange* photon reaches O", the *green* photon will be a distance L behind, but will have to travel a shorter distance (L – δ) before reaching O" (since O" is approaching this photon); thus, it is evident that the observer at O" will measure L" < L; and this will be true for all velocities ($0 < \beta < 1$).

The above explanation is based upon two propositions:

1. The speed of propagation for photons (*c*) is the same for all observers regardless of their motions.
2. Observers in the same reference frame (at rest with respect to one another) are in the same time domain.

Prof. Good must accept both of these propositions; otherwise, he will not understand the obvious; however, if he can be persuaded of their validity, then he can, in his own words, "nobly admit error."

He rashly insinuates I think "the Lorentz transformation is self-contradictory"; however, the contrary is true. I am on record for demonstrating the validity of the Lorentz transformations using two laws of kinematics; *viz.,* (1) the reciprocity of relative motion and (2) Doppler's principle. (*cf.* Campbell, J.O., "Number, Space, Motion & Time, *Galilean Electrodynamics*, July/August 1997, pp. 63-70.) This gives the Lorentz transforms a solid mathematical foundation, which they never had before; they are, after all, space transforms and have the same relationship to physics as Laplace transforms or any other mathematical entity. So now, when Prof. Good uses them, he can be absolutely certain they are correct. (Physics is dependent upon the correctness of the mathematics employed.)

J.O. Campbell

Enigma of light

This note addresses 2 questions:

1. Is light wave propagation affected by an ether or space-time continuum?
2. Is light energy speed a constant?

Consider a light source S and an observer O, each of which is moving with respect to a medium M in which they are immersed. Let M be an "ether" or a "space-time continuum" so that the light waves are somehow coupled from S into M and then decoupled from M to O. (This *must* be the case if light wave propagation is affected by either an ether or a space-time continuum as most physicists claim.) This scenario would certainly result in a Doppler frequency shift that is a function of two motions: (a) that of S with respect to M and (b) that of O with respect to M. However, all observations of the Doppler effect for light verify that it is a function solely of the relative motion between S and O. Therefore, light wave propagation is not affected by an ether or a space-time continuum.

This dependency of the Doppler effect solely on motion between S and O is a consequence of the fact that light energy propagates at a constant "*c*", so those who argue for a variable speed of light energy propagation are confronted with the fact that all observations of the Doppler frequency shift argue otherwise.

J.O. Campbell

No evidence for photon rest mass

Vigier (*Apeiron* 4, 71 (1997)) ignores the most important relevant facts and literature and makes a number of false assertions to try to claim that photons with a small rest mass might account for Miller's reported small positive Michelson-Morley result.

1) Miller's results have never been properly duplicated by independent observers. It would seem that they are probably a product of experimental error plus wishful thinking.

2) Contrary to Vigier's claim, the Michelson-Morley null result *does not* contradict the classical law of addition of velocities." The Michelson-Morley null result was predicted by Voigt [1] before the experiment was performed as a Doppler effect for light in absolute space-time that permits the classical addition of velocities. Vigier conveniently ignores Voigt.

3) Voigt's equations for the Doppler effect for light, the so-called "Lorentz transformation," being unfortunately written in terms of space and time variables instead of properly in terms of the propagation constant and the frequency [2], led to the foolish idea (*i.e.*, "special relativity") that lengths and time could somehow actually change in a moving system.

4) Vigier's assumption that light velocity is always (almost) equal to *c* in all directions; so the ether drift of about 350 km/s cannot be observed is false. Vigier conveniently ignores the experimental and observational evidence and the literature revealing the fact that the observed oneway velocity of light $\mathbf{c}^*$ depends upon the absolute velocity of the observer **v** such that

$$\mathbf{c}^* = \mathbf{c} \quad \mathbf{v}, \qquad (1)$$

where **c** is the oneway velocity of light relative to absolute space, as verified by the observations of the oneway velocity of light made by Roemer [3], Bradley [4] , Sagnac [5], Michelson-Gale [6], Conklin [7] and others observing the anisotropy of the 2.7 K oneway cosmic background radiation, Marinov [8,9] with his brilliant coupled mirrors experiment and his toothed wheels experiment measuring the difference in the oneway velocity of light in opposite directions in the closed laboratory, and the Müller-Means [10] recent observations using oneway radio time signals from geostationary satellites to measure the absolute velocity of the solar system.

5) From mass-energy equivalence and the photon energy *hv* the photon mass is necessarily

$$hv/c^2; \qquad (2)$$

and the momentum is *hv/c*. It may be readily shown that the Voigt-Doppler effect for light, and thus the *null* Michelson-Morley result, is *caused* by the mechanical recoil of a massive source or detector upon radiating or absorbing a photon [ii], using neomechanics, where the momentum of a body of mass *m* and absolute velocity **v** is

$$\mathbf{p} = \frac{m\mathbf{v}}{\sqrt{1 - v^2/c^2}}. \qquad (3)$$

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[1] W. Voigt, *Göttinger Nachrichten*, Math.-Phys. KI. 10, 41 (1887).

[2] J. P. Wesley, *Found. Phys.* **16**, 817 (1986).

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[4] J. Bradley, *Phil. Trans.* **35**, 637 (1728).

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[6] A. A. Michelson and H. G. Gale, *Astrophys. J.* **61**, 137 (1925).

[7] F. K. Conklin, *Nature* **222**, 971 (1969).

[8] S. Marinov, *Checkosl. J. Phys.* **B24**, 965 (1974); *Gen. Rel. Grav.* **12**, 57 (1980).

[9] S. Marinov, *Spec. Sci. Tech.* **3**, 57 (1980).

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[11] J. P. Wesley, *Causal Quantum Theory* (Benjamin Wesley, 78176 Blumberg, Germany, 1983) pp. 161-163.

J. P. Wesley
Weiherdammstrasse 24
78176 Blumberg, Germany

$B^{(3)}$ field not proven

Evans (*Apeiron* **4**, 48 (1997)) claims to have discovered "longitudinal solutions of Maxwell's equations" involving a "longitudinal magnetic flux density $\mathbf{B}^{(3)}$". But he fails to indicate the direction that defines his "longitudinal". Since Maxwell theory is based upon the Biot-Savart law and the magnetic field d**B** a distance **r** from a source current element *i*d**s** is *defined* as *transverse* to the source current, such that

$$d\mathbf{B} \equiv i d\mathbf{s} \times \frac{\mathbf{r}}{cr^3}; \qquad (1)$$

since the observed **B** field producing a force **F** on a moving charge q of velocity **v** is *transverse* to **v** and to **F**, according to the Lorentz force law

$$\mathbf{F} = q\mathbf{v} \times \frac{\mathbf{B}}{c}; \qquad (2)$$

and since, according to the Poynting theorem, the magnetic flux **B** is necessarily *transverse* to the direction of field energy flow, given by

$$\mathbf{S} = c\mathbf{E} \times \frac{\mathbf{B}}{4p}; \qquad (3)$$

it does not appear that Evans' $B^{(3)}$ field can lie along any "longitudinal" direction.

Evans bases his electrodynamic theory on arbitrarily chosen "fundamental underlying symmetries", which either conflict with empirically valid electrodynamics [1] or else are irrelevant, such as indicated by: "symmetry of special relativity", "Maxwell equations", "sub-symmetry of general relativity, group generators", "rotation generators", "translation generators", "boost generators", "Lie algebra", "Poincaré group generator eigenvalues", "cyclic relations", "field helicity , topological invariants", "Pauli-Lyuban'ski operator", "group identity", spin field", "Euclidean group , "Lorentz group", "Einstein group "spinors", "Lorentz transformation", "commutation relations", "Lorentz covariance , renormalization , non-Abelian quantum electrodynamics "gauge theory", "non-Abelian gauge theory", "photomagneton", "Abelian electrodynamics", and "four vector". Since Evans does not define nor discuss precisely what he might mean by all of these symmetries, merely giving a few references to the literature, his paper makes little sense. For example, the "Lorentz group" is a group in one space dimension only; whereas electrodynamics requires three space dimensions. And quantum mechanical "operators" must fail in general [2].

Evans' claim of having discovered something new in electrodynamics without even considering the recent experimental evidence (as reviewed, for example, by Wesley [1]), that does not support the Maxwell theory, cannot be taken seriously.

References

[1] J. P. Wesley, *Found. Phys. Lett. 10,* #4 (1997); 3, 395, 443, 471 (1990); *Classical Quantum Theory* (Benjamin Wesley, 78176 Blumberg, Germany 1996) Ch. 11, pp.284-321; *Advanced Fundamental Physics* (Benjamin Wesley, 78176 Blumberg, Germany 1991) Ch. 6, pp. 212-272.

[2] J. P. Wesley, *Classical Qauntum Theory* (Benjamin Wesley, 78176 Blumberg, Germany 1996) pp. 264-267; *Causal Qauntum Theory* (Benjamin Wesley, 78176 Blumberg, Germany 1983) pp: 93-94, 260-266, 364, 366.

J. P. Wesley
Weiherdammstrasse 24
78176 Blumberg, Germany
G. Galeczki and P. Marquardt
Society for the Advancement of Physics
Flittarder Hauptstrasse 22
51061 Köln, Germany

Evans replies to Wesley *et alia*

Wesley *et al*. assert arbitrarily and unscientifically that the $B^{(3)}$ field has not been proven. In actuality, the $B^{(3)}$ field is an observable of *non-linear* optics in the inverse Faraday effect. This was first shown as far back as 1992[1]. The relevant theorem, the **B** cyclic theorem, is Lorentz covariant and CPT conserving in the quantized field theory. Thus $\mathbf{B}^{(3)}$ satisfies all known criteria for a magnetic field in contemporary field theory. The direction of the $\mathbf{B}^{(3)}$ field is along the direction of the propagation axis of the light beam.

Wesley *et al*. are still working within the context of linear electrodynamics and list a number of terms with which they are unfamiliar. These are to be found in a textbook on quantum field theory such as ref. [2], and are well known to senior undergraduates. The terminology in my paper is therefore precisely defined in textbooks. The Lorentz group is not used in my paper, I use the Poincaré group with ten generators: three rotation generators, three boost generators, four spacetime translation generators[2]. There are four of the latter because space has three dimensions, not one, as asserted by Wesley *et alia*.

I am prepared to understand that my paper makes little sense to these authors, who appear to know nothing about contemporary field theory. They do not refer to any of the six volumes and over fifty papers (by many authors) now available on $B^{(3)}$. Are we to take *them* seriously?

The electrodynamics of Maxwell are not based on the Biot-Savart Law, but on Maxwell's proposal of his displacement current *in vacuo*. The law of Biot and Savart, later developed greatly by Ampère, was proposed many years before Maxwell's displacement current, and is a law of magneto-statics. In 1965, following the development of the laser at Harvard, it was demonstrated empirically[3] that circularly polarized light magnetizes material matter in the *inverse Faraday effect*. This is a phenomenon of *non*-linear optics which cannot be described by Maxwell's *linear* electrodynamics. It depends for its existence on the conjugate product $\mathbf{B}^{(1)} \times \mathbf{B}^{(2)}$, which is[1] $i\mathbf{B}^{(0)}\mathbf{B}^{(3)*}$. Thus $\mathbf{B}^{(3)}$ is an empirical observable and is therefore proven experimentally.

I suggest that these biased polemicists actually begin to read the literature on nonlinear optics and modern field theory. Having done this, and perhaps having taken an undergraduate course or two, they should repeat the inverse Faraday effect with radio frequency irradiation in order to observe the $\mathbf{B}^{(3)}$ field acting at first order.

I see no reason to refer to their work unless it is of relevance to $\mathbf{B}^{(3)}$ theory, and judging by this letter, no reason to refer to it at all.

References

[1] M. W. Evans, *Physica B*, 182, 227 (1992).

[2] L. H. Ryder, *Quantum Field Theory* (Cambridge, 1987, paperback).

[3] See for example 150 papers on magneto-optics reviewed by R. Zawodny in M. W. Evans and S. Kielich (eds.), *Modern Nonlinear Optics* (Wiley, New York, 1997, paperback), in three volumes; M. W. Evans, J.P. Vigier *et al*., *The Enigmatic Photon* (Kluwer, Dordrecht, 1994 to present) in four volumes to date.

$B^{(3)}$ in press

I would like to record the fact that there have been twenty five formal comments and replies on the $B^{(3)}$ field, the subject of the Spring Special Issue of *Apeiron*. Comments have ranged from "very, very, very important" to "not to be taken seriously" and various forms of abuse which I, in turn, dismiss out of hand. Naturally, serious debate in *Apeiron* would be a good thing. The formal comments and replies to date are listed below. So far these involve fifteen participants. There are now six volumes and over fifty papers available on the idea. Broad agreement has been obtained on the fact that the B Cyclic Theorem is Lorentz covariant, gauge invariant, and CPT conserving. The $B^{(3)}$ field therefore meets all the contemporary criteria for a magnetic field, and is empirically observable wherever the conjugate product is observable. Comments are labeled (a), replies are labeled (b) and are grouped according to subject matter.

1a) L. D. Barron, *Physica B*, 190, 307 (1993).
2b) M. W. Evans, *Physica B*, 190, 310 (1993).

3a) A. Lakhtakia, *Found. Phys. Lett*., 8, 183 (1995).
4b) M. W. Evans, *Found. Phys. Lett*., 8, 187 (1995).

5a) A. Lakhtakia, *Physica B*, 191, 362 (1993).
6a) D. M. Grimes, *Physica B*, 191, 367 (1993).
7a) A. D. Buckingham and L. Parlett, *Science*, 264, 1748 (1994).
8a) A. D. Buckingham, *Science*, 266, 665 (1994).
9a) M. W. Evans, *Found. Phys. Lett*., 8, 563 (1995).

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14a) E. Comay, *Chem. Phys. Lett*., 261, 601 (1996).
15b) M. W. Evans and S. Jeffers, *Found. Phys. Lett*., 9, 587 (1996).

17a) E. Comay, *Physica B*, 222, 150 (1996).
18b) M. W. Evans, *Found. Phys. Lett*., August, 1997.

19a) E. Comay, *Found. Phys. Lett*., July 1997.
20b) M. W. Evans, *Found. Phys. Lett*., 10, 255 (1997).
21b) V. V. Dvoeglazov, *Found. Phys. Lett*., Aug., 1997.

22a) M. Y. A. Raja, W. N. Sisk, M. Youssaf and D. Allen, *Appl. Phys. Lett*., 67, 2123 (1995).
23a) M. Y. A. Raja, W. N. Sisk and D. Allen, *Appl. Phys. B*, 64, 79 (1997).
24b) M. W. Evans, *Apeiron*, 4, 80 (1997).
25b) M. W. Evans, submitted for publication.

The above records the fact that on several occasions, the right of reply was not forthcoming. Without the open mindedness of Prof. A. van der Merwe, censorship of the $B^{(3)}$ idea would have occurred, and a grossly one sided view of the subject matter presented to posterity. It is by no means clear that electrodynamics is a "finished" subject: on the contrary, it has provided one of the liveliest formal debates seen this century in physics, whether one likes the idea of $B^{(3)}$ or not. I cannot recall anything like this with the possible exception of the great Wimbledon match between Pancho Gonzales and Lew Hoad, which went to about sixty games in the final set, before Pancho finally won out in almost total darkness. Let us hope that some light is shed on natural philosophy as a result of all this.

M.W. Evans