@ ISSUE

Correspondence, conference threads and debate

On the Role of Space and Time in Relativity Theory

My venerable friend Mendel Sachs (*Apeiron*, Vol.5, Nr.1-2, 1998) belongs, together with Henri Bergson and E.G. Cullwick, to the select group of "pure relativists". In contrast to Einstein and his followers (*i.e.* the overwhelming majority of present-day physicists), the "purists" consider "time dilation", "mass increase with velocity", a.s.o., as "appearences", or "illusions", not unlike the convergence of the rails of a railroad track as they recede into the distance. To Mendel Sachs the CERN muon life-time experiment means an apparently (*i.e.* "as viewed from laboratory") slower desintegration rate, rather than an absolute increase of the life-time, as demonstrated by the transport of unstable particles to distances much larger than c.τ. Both tau = γ.τ and m = γ.m° do have dynamical explanations and have nothing to do with "special" relativity! Moreover, radiation requires a unique, very privileged absolute frame of reference. Good professionals, like Ives, Builder, Prokhovnik, Jánossy, Arzeliès, Bell, Vigier and many others, became "neo-Lorentzians", by trying to fit together "special" relativity and the absolute space and time theory of Lorentz and Poincaré.

After 30 years of relativity criticism "from inside", Mendel Sachs utterly failed to persuade the Establishment of the correctness of his interpretation. His "Rubashovian attitude" (Rubashov, the hero of Arthur Koestler´s "Darkness at Noon", is an old, faithful bolshevik, who defends communism even in a communist jail....) only weakens the well-founded (but very hard) fight against the dogmatically defended "special" relativity.

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Reply from M. Sachs

George Galeczki has it wrong about the logical basis of relativity theory *versus* the consensus of opinion today—excluding Einstein himself. Einstein's comments in his "Autobiographical Notes" (referred to in my *Apeiron* letter) as well as references much earlier than this (*e.g.* his lecture in 1923, quoted in A. Einstein, *Sidelights of Relativity* (Dover, 1983), p. 35) make it clear that he did indeed change his mind about his earlier faulty interpretation of the Lorentz transformation as referring to an absolute physical effect in matter—shrinking sticks or retarding the reading of the hands of a clock). He said that this would indeed be logically inconsistent, and as such it is inadmissible as a scientific conclusion.

Without going into the details again, the reason for the logical inconsistency of the consensus interpretation of the Lorentz transformation as a physical effect is the relativity of reference frames. The latter would imply that a meter stick is both longer and shorter than another meter stick, that a twin brother is both older and younger than his brother, *etc*. But the interpretation of the Lorentz transformation as a 'scale change' entails no logical paradox.

Galeczki says that "radiation requires a unique, very privileged absolute reference frame. He seems to be unaware that this "model" of absolute reference frames for all physical phenomena, is inconsistent with the relativity of reference frames, according to Einstein's principle of covariance—no matter what the "good professionals" say!

One may then say: OK, but relativity is wrong. But one cannot have it both ways: keeping the validity of the theory of relativity and the absolute reference frame for radiation. Personally, I believe that it is Einstein's relativity theory based on the principle of covariance that is scientifically true.

On consensus of opinion versus truth, I may remind Dr. Galeczki of advice that was given about 800 years ago:

> *For when something has been demonstrated, the correctness of the matter is not increased, and certainty regarding it is not strengthened by the consensus of all men of knowledge with regard to it. Nor could the correctness be diminished and certainty regarding it be weakened even if all people on Earth disagreed with it.*

Moses Maimonides, *Guide of the Perplexed*, transl. S. Pines (Univ of Chicago Press, 1963), p. 290.

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Selleri's Theorem

Generally, science progresses, not so much by carping iconoclasm, but by positive contributions that help all of us see better some facet of nature. To

understand the powerful message in Selleri's theorem [1], one must asssess who it has helped.

First, if it is to help deflect the conventionals from the strength of their monolithic stand, it is best to use their conventional language, as Selleri did, even though most of the present confusion of both conventionals and dissidents comes from the mix of technical with common word usage. The job of dissidents is to *help* sort out the language and give the conventionals a *cause* and *effect* basis for the experimental data and mathematical formalism of special relativity, which they use daily with great success.

Second, to *help* ourselves, we must correct the present condition "a different theory for every dissident," a weak approach against the conventional monolith. Selleri's theorem *helps* to do this in the following way. When (not if) we succeed in correcting the conventionals' position, we will find, as in the past, that the modifications in the experimental data and mathematical formalism will be very small, whereas the change in viewpoint, language and understanding will be significant. The same principle applies in the dissidents' need to unite under one approach.

For example, one group of dissidents recognizes the experimental validity of the constancy of 2-way light velocity and the slowing of physical clocks in motion relative to the observer; but rejects the contraction of physical rods in perpetuation of the *myth* that "only things directly measurable are real." It should be clear that any measurement is difficult and inaccurate (See [2]). Understanding comes mostly from imagination and intuition *supported* by measurement, where possible; so Selleri's theorem helps that small group of dissidents join the rest of us, who realize that the rod contraction is physically real, with the same cause and effect basis as the clock contraction. Notice that space and time have not yet appeared in this discussion.

One of the biggest problems facing conventionals and dissidents alike is pushing the metaphysical base down below the levels of abstraction where physics is understood. Only by cleaning all metaphysical ideas out of the physics levels can paradox be eliminated. Physics levels are all cause-and-effect understandable; but the metaphysical base is a set of statements that probably never will be understandable. One metaphysical base that has been pushed down out of a successful physical theory (3) allows a universe that has just three metaphysical components:

1. Newton's absolute space
2. Newton's absolute time
3. A massless, frictionless fluid fills space

Little more can be said about *space* and *time* or the *fact* that the fluid ether fills the space. These three conditions are metaphysical, *i.e.* not explainable. Physics takes these three non-physical facts and, by discovering the physical *properties* of the ether, builds from them the world of real objects. Physics shows that particles and waves are solutions of the ether's equations of motion. The particles change shape when moving relative to the main body of ether; and, because rods and clocks are made of these flexible particles, the rods and clocks change; the clocks slowing and the rods contracting. These changes have nothing to do with metaphysical space and metaphysical time; they only affect measured distance and clock time, on an understandable cause and effect basis.

No experiment has ever been performed on "space" or "time", which appear to be homogeneous, to the extent that term can be applied to such metaphysical ideas. Certainly, the ether's physical properties allow large regions where no particles or waves exist to be homogeneous. In using transformation theory, the statement "inertial reference frame" implies a physical laboratory, replete with physical rods and clocks that define time and position inside the laboratory. That is what is being transformed to another physical laboratory's clock and position readings. One carper accused Selleri of using a "physical" rather than a "mathematical" foundation. That pinpointed the essence of the problem at hand. It *is* physical, and mathematics is not the basis of the world's characteristics; it is only a useful tool, like clocks and rods, which when all used together give us the word's physics.

Finally, a small number of dissidents among us find an error in a derivation and beat it into the ground. Many solid physical facts were discovered by erroneous derivations. The thing that made it clear they were facts was their everlasting agreement with observation. Fortunately, there is no error in Selleri's derivation and his theorem also enjoys the blessing of being in agreement with the experimental facts.

Those of us dissidents who use the above metaphysics and physics are appreciative of the help that Selleri's theorem has made available to us to continue the job ahead.

References

[1] Selleri, F. (1997) Remarks on the Transformations of Space and Time, Apeiron 4, No. 4, 100-103.
[2] Munera, H.A. (1998) Michelson-Morely Experiments Revisited, *Apeiron* 5, No. 1-2, 37-58.
[3] Dishington, R. H. (1998) The Conservation Law, *Apeiron* 5, No. 1-2, 1-20.

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No Earth Expansion

Having carefully read L.S.Myers exposition on the "expanding Earth" in the October volume of *Apeiron* (Vol.4, No.4; 1997, pp.118-122) and previously read a large part of Prof. Carey's "Earth, Universe, Cosmos" (1997), I would be pleased if you would permit me to make some comments on the hypothesis of Earth expansion.

Considering the knowledge needed and the work required to develop a new hypothesis it is unfortunate that so little knowledge is required to disprove it if it is disprovable. All that is required is to show that one piece of known data is incompatible with the hypothesis. No great knowledge is necessarily needed. What is required is to recognise the discrepancy if one comes upon it—deliberately or accidentally. As it happens, I accidentally discovered several pieces of data which are inimical to the "expanding Earth" hypothesis while investigating what I thought at the time was an unrelated problem.

While it is relatively simple using geological data to disprove subduction as it is currently argued, it is *not* simple, using geology, to disprove the "expanding Earth" hypothesis. (But see the debate in *The Australian Geologist* (1995-97) numbers 96 to 102, culminating in what apparently is the geologically accepted argument against the idea - No.102 (March 1997, pp.22-25.) However, it is quite easy to disprove the hypothesis using Solar System relationships. As I have written above, there are several incompatible pieces of data which, if not singly, then certainly in plural, show that an expanding Earth as currently argued is impossible. (I personally believe the Earth has expanded slightly and will continue to expand a small amount but in total, expansion has been of very minor importance.)

As Myers states, the Nebula Theory is fallacious and cannot be used to disprove the "expanding Earth" hypothesis; but while knowing how the Solar System did come into being helps with testing, it is not a necessity. *Knowing what to use to test the hypothesis is the important thing.* Of the several tests available I would like to briefly present three, partly because of their simplicity and partly because I like them.

1. Consider the two pairs of planets Uranus (U) and Earth (E), Neptune (N) and Venus (V); and note the relative positions of the two pairs to Jupiter: Uranus-Earth nearest, Neptune-Venus farthest.

Using the generally accepted figures for the planet radii of $R_U = 26150$ km, $R_E = 6378$ km, $R_N = 24750$ km, $R_V = 6050$ km (a few kilometres either way is unimportant.) then the following are radii ratios;

$\quad$ RU/RE = 4.1000 (= 1.198 within +1.96%)
$\quad$ RN/RV = 4.0909 (= 1.198 within +1.73%)

and the two ratios are 0.22% from one another.

$\quad$ RE/RV = 1.0542 (~ 1.23 1/4 within +0.10%)
$\quad$ RU/RN = 1.0566 (~ 1.23 1/4 within +0.33%)

Here we have a symmetry in terms of *size* of the two pairs of planets both in position (both the largest gaseous and terrestrial pair are nearest Jupiter, the smallest pair farthest away.) and in radii relationships. Perhaps as significant, the ratios are powers of the two most common Universe ratios, *viz.* 1.23 and 1.19, suggesting that the relationships are not accidental. Now as I see it, the radii relationships can be explained in one of three ways in terms of expansion:

(a) At this moment of Earth time it just happens that an expanding Earth has such a radius that its ratio to that of Uranus equals the ratio of the radii of Neptune-Venus;

(b) The four planets are all expanding at the same ratio;

(c) The four planets are not expanding.

Surely (a) is most unlikely; and the mechanism for (b) is extremely difficult to envisage—impossible when applying the expansion hypothesis as given by Myers in his article. This leaves only conclusion (c)—that none of the four planets are expanding— and this is not only the simplest case but also the most easily obtained condition.

2. It is an empirical fact that the <u>masses</u> of the planets, excepting Saturn, are related by the formula $m_x = a^n \times M$, where m_x = mass of the planet required, M = mass of Jupiter, a = 1.19 for Neptune and Uranus, a = 1.23 for the terrestrial planets; except that for Mars the figure is only approximate—within –2.65%. This can be explained but is outside the present discussion. The n values (with bracketed percentage difference to the measured value in each case) are:

Neptune, n = –17 (+0.85%); Uranus, –18 (+0.80)
Earth, –28 (+1.57); Venus, –29 (+1.40); Mercury, –42 (+1.46)

In addition, the formula $m_x = a^n \times M_x$, where M_x is the total mass of the planets *and satellites* less Saturn and a = 1.23, gives reasonable values for the masses of the satellites when the following n values are used (percentage difference from the measured value in brackets):

Ganymede, n = 46½ (–2.55%); Titan, –47 (–3.73); Callisto, –48 (+0.42); Io, –49 (–3.02); Moon, –50 (–4.32); Triton –51 (+0.35); Europa –52 (–4.55). The "icy" satellites' average difference is –1.38%, for the "silicate" satellites it is –3.96%.

Now if the masses of the planets and satellites are related by such simple and obviously related formulae as given above, and such a relationship for the planets is not a present day coincidence, then it is impossible for the "expanding Earth" hypothesis as enunciated by Carey or by Myers to account for the

relationship. By far the simplest explanation is that there has been no change of mass, and so no change of volume, of the Earth (again with my earlier opinion).

3. It is an empirical fact that the specific gravities of the satellites and uncompressed terrestrial planets are related by the formula S.G. $= 2\times0.981^n$, where n varies between +36 (S.G. = 1) and –51 (Mercury).

In the case of the "icy" satellites there appears to be a general requirement that the n value be an even, positive integer but there are a few exceptions and some are missing. Suggested explanations for all the missing value bodies can be given but, again, it is not relevant to the subject of this letter.

Excluding Europa (n = –22) the "silicate" satellites and uncompressed terrestrial planets require the n numbers to be negative integers divisible by three. That is, Moon (n = -27), Io (–30), Mars (–33), Earth (–36), and Mercury (–51). Note that the relationship of the specific gravities of the bodies is independent of the "parent" planets. (The preceeding suggests that the n value should be –42 for Venus, giving an uncompressed specific gravity for that body of 4.48.

Again we have the fact of a simple mathematical relationship of a physical property between a number of Solar System bodies, the relationship being reinforced by the tendency of a simple sequence in the number n.

Specific gravity is mass divided by volume. If Earth expansion is taking place as given by Carey and by Myers the bodies mentioned above must be adding mass and volume commensurate with the requirements of the formulae given in 1 to 3. I very much doubt if it can be done; certainly not applying the "expanding Earth" hypothesis as given; again excepting the case of present day coincidences of the relationships.

Your readers may not agree with me but the conclusion I have reached is that the three relationships given above, when taken in conjunction, definitely preclude the possibility of Earth expansion taking place at present and preclude it having taken place in the past. We are considering here expansion as argued by Myers a small amount over a long time can be accommodated by the formulae I have presented.

Myers may take some comfort in noting that the relationships given above also suggest it is very unlikely that the Solar System formed from a Nebula Disc or that the satellites of the gaseous planets formed in a similar way about their "parent" planets. Actually, there is really quite a lot of data which militates against such a development but I must restrain myself from referring to it and end this letter.

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Special Relativity: The purpose of discussion

Dr. I J. Good (*Apeiron*, Vol.5, no.1-2 (January-April 1998, pp.107-121) is to be applauded in his indefatigable labours to establish the supremacy of mathematical reason. Unfortunately he ignores that, in consequence of developments culminating in Weierstrass and Russell, the ways of mathematics (4D) and physics (3D) have long ago separated; his defence of 4D therefore misses the point. In addition, to quote Charles Dickens, [his] enjoyment of embarrassed affairs [is] like a housekeeper's enjoyment in pickling and preserving, or a washerwoman's enjoyment of a heavy wash, or a dustman's enjoyment of an overflowing dust-bin, or any other professional enjoyment of a mess in the way of business.

To join him in his enjoyment is a waste of effort; our resources being limited, they must be directed to find a way out rather than to get stuck in. One item though, needs correction: the u=0 in the Brown & Maya article. u is there the defining velocity in the 4-matrix form of the Lorentz transformation. Although invariance is independent of the numerical value, u=0 involves division by zero. In particular, the proof that v'=v rests on u=0.

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"Establishment Thinking"
A reply to Chappell

Chappell [1] mentions the indisputable fact that "even if 5(IJG) [Good, Ref. 2] had found errors in each of the articles he criticizes… he has not, thereby, proven that special relativity (SR or STR) is invulnerable". Of course, but I never claimed to show that STR is invulnerable in spite of my antidisestablishmentarianism. Apart from my reply in [2] to Phipps, related to group theory, my aim was to refute arguments claiming that the LT (Lorentz transformation) or KSTR (the kinematics of STR) is self-contradictory. Chappell goes on to say of me "He instructs Walton that 'attacks on STR should be based on *empirical* evidence;' but if so, should not *defenses* of it be similarly based?" No,—not when those defenses of STR *refute attacks on STR that are not based on empirical evidence*. Such attacks always claim that KSTR is self-contradictory and of course

are always wrong if KSTR is self-consistent. My confidence in that self-consistency is not based on faith, it is based on careful arguments in sixteen communications [2,3,4,5] in which, among other things, several fallacious arguments are refuted. I will have done a service to dissidents if I can convince them that the LT is self-consistent. For they will then stop wasting their time by trying to prove the impossible and will concentrate on empirical arguments.

My defenses of the self-consistency of KSTR are for the most part mathematical and logical in which no reference to empirical evidence is required. This mathematical work is of physical interest because, of course, if KSTR is found to be self-contradictory, then STR would be refuted.

My remark, intended to be flippant and light-hearted, that I was a "defender of the faith", has misled Chappell. I didn't mean that I should attend a chapel to worship Einstein. The understandable misunderstanding has provoked many of Chappell's critical comments.

A defense of the self-consistency of KSTR does support STR to *some* degree. This degree is small for myself because I have believed in the self-consistency of KSTR for more than ten years, but the degree would be large for those dissidents who currently think KSTR is self-contradictory. If such a dissident becomes convinced, by an argument, that KSTR is self-consistent, then that dissident's personal odds (odds, not probability) of STR, as compared with Newtonian physics, are multiplied by a large factor (even if the odds still remain small). This is known as the *Bayes factor* in favor of the theory provided by the argument.

Fallacies are sometimes interesting and educational, such as some geometrical fallacies [6, pp. 76-84]. So fallacies are not always without value.

In due course I would like to study the other topics and references supplied by Chappell. I wish he had spelt out the Sagnac reference. But having lost my time-machine because a chimp entered it surreptitiously, I cannot accept Chappell's kind invitation to seek enlightenment in Philadelphia in February 1998.

I was not happy with Chappell's rude expressions "gratuitous rudeness", "no fewer than five disrespectful blasts" and "you people". Worse were expressions like "incredibly stupid" (Walton, [7]), and "phenomenal blunder" (Walton, [8]), not referring to me; and, referring to me, "up to his tricks", "ridiculous mistakes" and "lack of basic knowledge of mathematics (S. Xu [9]), and "things are obvious to one only according to his gifts" (Campbell [10], who thought something was obvious when it wasn't even correct). Is this the "spirit of tolerance" on the part of dissidents, to which Chappell referred?

Chappell [1, p. 129ii] says "With gratuitous rudeness, 5(IJG) accuses his opponents of being 'flat-earthers' …". My intention [3, p. 121i] was to draw an *analogy* with a flat-earther because Xu & Xu seemed to be denying the existence of space-time by fiat. I had in mind the flat-earthers of long ago, often intelligent, dissidents of the time. I'm sorry I didn't make that clear enough and might have caused offense.

My interchanges with Phipps and with Szego & Ofner, have been gentlemanly on both sides, so Chappell's expression "no fewer than five" is certainly wrong under any reasonable definition of "disrespectful blasts". Also, to point out an error is not in itself disrespectful.

Chappell takes issue with my response [11a] to Szego & Ofner [12]. Note that [11a] consisted of two parts. Chappell overlooked that just the first part pointed out an error in [12]. The second part, thrown in for good measure, showed that, for their model, the LT leads to a "bingosity", a neat confirmation (corroboration) of the self-consistency of KSTR. In spite of what Chappell says [1, p. 128, col. ii], my argument corroborated (supported) the view that the constant c is common to any two inertial systems and therefore to all inertial observers. (For more on this topic see Section 4 of [13]. This is relevant because Xu & Xu [14] happened to share a model with Szego & Ofner.) The ambiguity of the word "confirmation" has provoked another misunderstanding and another adverse criticism from Chappell. I hope he will now treat my discussions with the tolerance (and respect) that he claims dissidents in general have.

When I referred to Xu & Xu as Xu^2 I was of course being facetious. Note that Xu & Xu [14] wrote $STONES^2$ and $EGGS^2$, not facetiously, but based on the incorrect assumption that x and t can represent stones and eggs.

Chappell [1, p. 129] says Einstein [15]

"gives each of two light beams two different velocities (explicitly: first $c + v$ and $c - v$: then later surreptitiously [it wasn't surreptitious, it was "up front"] but quite certainly, exactly c in each case)."

Chappel is presumably referring to [15, p. 42] and I cannot blame him for failing to understand the formulae

$$t_B - t_A = r_{AB} / (c - v),$$
and $$(1)$$
$$t_{A'} - t_B = r_{AB} / (c + v),$$

for Einstein's explanation of (1) was seriously incomplete. I will now fill the gap. I assume that the reader has [15] to hand and I will explain the first of the equations (1). The second equation has a similar

explanation. Note once and for all that the explanation is *entirely* in terms of the frame of reference in which an inertial observer Ω is stationary, his "natural coordinate system" (NCS).

A rod AB is moving with velocity v along the x axis relative to Ω. The length of the rod (as measured by Ω *of course*) is denoted by r_{AB}. At time t_A on Ω's clock the end A of the rod coincides with Ω who then sends a light pulse along the length of the rod. Because of the *definitions* of t_A and t_B, of course the pulse takes time $t_B - t_A$ to reach B. Meanwhile B advances a distance $v(t_B - t_A)$ so the light beam travels a distance $r_{AB} + v(t_B - t_A)$. Hence

$$c(t_B - t_A) = r_{AB} + v(t_B - t_A) \qquad (2)$$

because the speed of light is c (in the NCS of Ω) by one of Einstein's basic hypotheses for KSTR [15, p. 88]. Therefore

$$t_B - t_A = r_{AB} / (c - v) \qquad (3)$$

as required.

This explanation of (3) is independent of whether KSTR is assumed. It shows that Einstein did not make the "utterly incredible yet virtually unknown blunder" (Chappell's p. 129) of assuming that the speed of light is $c - v$ (or $c + v$) relative to Ω. It is the speed or velocity of the light beam relative to the other observer Ω' as measured in the NCS of Ω.

Einstein ostensibly used (1) to derive the usual LT. It is hardly worth bothering with his difficult pages 44 and 45, and I have not done so, because another method, suggested without proof in a footnote on p. 46 of [15], leads more easily to the ordinary LT. The footnote was due to Arnold Sommerfeld according to Miller [16, p. 391]. See [3, p. 118] for one explanation of the footnote.

Incidentally, I published an article by Dingle in Ref. [17] after much friendly correspondence with him.

References

[1] John E. Chappell, Jr. (1998). *Apeiron* 5, (1-2), 128-130.

[2] I.J. Good (1997), five letters in *Apeiron* 4 (4), within pages 126-132. See also 5 (1-2) (1998), within pp. 107-121.

[3] I.J. Good (1998). *Apeiron* 5 (1-2), within pp. 107-121.

[4] I.J. Good (1995), *Intl. J. Theor. Physics* 34, (5), 779-799.

[5] I.J. Good, *Physics Essays* 4, (4) (1991), 591-595; 7, (4) (1994), 436-441; 10, (2) (1997), 327-333; 10, (3) (1997), 454-465; 11(2) (1998).

[6] W.W. Rouse Ball, *Mathematical Recreations and Essays* (Macmillan, London, 1940).

[7] G. Walton (1996), *Apeiron* 3 (3-4), 126.

[8] G. Walton (1998), *Apeiron* 5 (1-2), 130.

[9] S. Xu (1998). *Apeiron* 5 (1-2), 123-128.

[10] J.O. Campbell (1997), *Apeiron* 4 (4), 132.

[11] I.J. Good, (a) *Apeiron* 4 (4) (1997), 126; (b) 5 (1-2) (1998), 113-116.

[12] L. Szego & P.F. Ofner (1997), *Apeiron* 4 (2-3), 91; 4 (4), 126-127.

[13] I.J. Good, "A reply to Shaozhi Xu", in this issue.

[14] S. Xu & X. Xu, *Apeiron* 4 (4) (1997), 130-131.

[15] A. Einstein (1905/23). *In The Principle of Relativity* (Dover, New York. 1923).

[16] A.I. Miller (1981). *Albert Einstein's Special Theory of Relativity* (Addison-Wesley, Reading, Mass.).

[17] *The Scientist Speculates* (I.J. Good, General Ed.; A.J. Mayne, Assoc. Ed.; J. Maynard Smith, Biol. Ed., 1962; Heinemann, London. There were French and German editions).

I.J. Good

"Refutations"
A reply to Xu Shaozhi

1. Good (1998b) (which Xu had not seen when he wrote his long "brief comment" Xu 1998), contains some responses to Xu & Xu (1997a, b). In particular, I proved there, and in Good (1997b), that Xu & Xu did not show, as they claimed, that the LT is self-contradictory. But further response to Xu (1998) is necessary because it contains eleven insults emanating from his own errors. He cannot be blamed for failing to show that the kinematics of special relativity (KSTR), or the Lorentz transformation LT, is self-contradictory for no one has succeeded in doing so. I have examined enough attempts to make that statement with confidence; for references see my response to Chappell in this issue. I can supply copies if requested.

Xu says incorrectly

Rather than offer a direct refutation of the proof by Szego & Ofner, Good feeds us 'a neat confirmation of the LT' instead ...

Contrariwise, I *did* give a direct refutation of Szego & Ofner (1997a) in Good (1997a) and *in addition* I used their model to give a neat confirmation (support or corroboration, not proof) of the self-consistency of the LT. So Xu's "rather than" and "instead" are incorrect. In the corroboration I made use of Pythagoras's theorem. See Good (1998b, p. 120, col. i) for one proof that, in the context, Pythagoras's theorem can be used correctly in spite of Xu (1998, p. 124). An inertial system consists of a Euclidean space plus a time axis. The distance between a pair of events that are *simultaneous* in an inertial system is simply the Euclidean distance because the temporal term in the familiar relativistic interval drops out. I have assumed here that the concept of simultaneity *within* an inertial frame of reference is accepted although it is by no means free

of controversy. See, for example, Salmon (1977) for a survey.

Note that the equations

$$(\gamma^2 v^2 + c^2)^{1/2} t / (\gamma t) = (v^2 + c^2 \gamma^{-2})^{1/2} = c, \quad (1)$$

which led to "bingosity" (neat corroboration) were very badly printed in Good (1997a). This must have discouraged readers. There were obvious misprints also in Good (1998b, pp. 116 and 117).

See also Szego & Ofner (1997b) and Good (1998a). My responses to Szego & Ofner in Good (1997a and 1998a) also refute Xu's Lemma 1.1 where the usual LT is misused because his formulae (A2) and (A3) refer to two distinct photons. This invalidates Xu's position at its roots. The point is discussed in Good (1998b).

3. On page 124 of Xu (1998) he says incorrectly that I lack basic knowledge of mathematics. For an indexed list of many of my mathematical publications see Good (1983, pp. 251-313). There is a more up-to-date unpublished list.

In Good (1997a) I mentioned that the triple (where $z = 0$ is ignored)

$$(x, y, t) = (0, ct, t), \quad (2)$$

which, for variable t, represents a photon traveling up the y axis, transforms, by the usual LT, to

$$(x', y', t') = (-\gamma ct, ct, \gamma t). \quad (3)$$

Xu (1998, p. 124) says "substituting $y' = ct'$, this yields $(-\gamma vt, \gamma ct, \gamma t)$" instead, thus seeming to him to contradict both (3) and the self-consistency of the LT. His mistake is the same as in his Lemma 1.1: his equation $y' = ct'$ refers to a photon travelling up the y' axis, not up the y axis. The equation $y' = y$ in the usual LT doesn't mean that the y and y' axes coincide (except when t and t' are zero)! I pointed this out in Good (1997a). Xu's response (1998, p. 124, col. ii, the 2295 (iii) para. starting "Excuse me") is unintelligible apart from three insults. Xu's error becomes even clearer if we give t a specific value, say 7 seconds, so as to refer to a definite event instead of to the whole path of a photon. When Xu (1998, p. 125) says that I "should now nobly admit" (boomer-anging an expression of mine which is *not* an insult) that I am mistaken, and should withdraw my "Bingo" refutation, "at least before he [Good] can refute the above". I have now presented the required refutation so Xu should do the noble thing.

5. Now consider Lemma 2.1 in Section III of Xu (1998). This is somewhat subtle. On a first reading I thought I might have to say "Ouch". He considers a spherical wave-front as in Einstein (1905/23, p. 46). Xu points out that, by the usual LT, we have

$$(ct')^2 = x'^2 + y'^2 + z'^2 \quad (4)$$

$$= \gamma^2 (x - vt)^2 + y^2 + z^2 \quad (5)$$

and he says in effect that this *contains* variables y and z unlike the formula

$$(ct')^2 = \gamma^2 (ct - vx / c)^2, \quad (6)$$

also from the LT. (There is a misprinting of (6) in Xu, 1998.) (5) and (6) are of course both correct. Xu thinks he has obtained a contradiction. But he has overlooked that for this model x, y, and z are *not independent* because, on the wave-front, we have

$$x^2 + y^2 + z^2 = c^2 t^2. \quad (7)$$

Therefore the right side of (5) is seen, by using the definition of γ, to be equal to

$$\gamma^2 \left[(x - vt)^2 + (1 - v^2 / c^2)(c^2 t^2 - x^2) \right] \quad (8)$$

which reduces to the right side of (6). Not "Ouch" but Bingo!

6. The first half of Section IV of Xu (1998) makes use of his Lemma 2.1 and is therefore also mistaken. It might be helpful to mention that it is adequate to use the (1+1)D form of the LT when only two inertial observers or frames are under consideration. This because they can choose their x axes parallel to the velocity v, and their y and z axes in the standard manner to force $y' = y$, $z' = z$. The "event" for the LT is not constrained to the x and x' axes in spite of Xu. Then the 4D invariant "interval" reduces to a 2D invariant.

7. Xu (1998, p. 126) states that "The LT is a set of intrinsic 0/0 type [of] equations". But mathematicians interpret indeterminate forms as limiting forms. Let us do so. I ignore Xu's constants C and C', because I am using the LT in its simplest form.

The coordinate pair (vt, t) in a system S represents an object (a sequence of events) moving with velocity v relative to S. It is therefore at rest in a system S' that is also moving with that velocity relative to S. The object is at rest at the spatial origin of S'. It can be regarded as a sequence of events for which, for each t, the equation $x' = \gamma (x - vt)$ of the LT be-comes

$$0 = x' = \gamma (x - vt) = \gamma \times 0. \quad (9)$$

If $v < c$ this puzzles no one, but if $v = c$ we get the indeterminate form $\infty \times 0$. Of course this form is to be interpreted as the limit as $v \to c$, and the correct interpretation of the indeterminate form is simply

$$x' = 0 \quad x = ct. \quad (10)$$

There is no inconsistency.

8. Conclusion. Xu has not shown that the LT or KSTR is self-contradictory.

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I.J. Good

Walton vs. Good, concluded?

Ms. Walton has been kind enough to send me a prepublished continuation of our discussion [1, 2, 3, 4]. This enables me to reply in the same number of @ *issue*.

Note first that my pun "Nobelity" (with an e after the b) was misprinted as "Nobility". My point was that the first *correct* and *lucid* proof that the kinematics of STR is self-contradictory, without reference to experimental results (and without making a Newtonian assumption for granted, such as absolute simultaneity) would deserve a Nobel prize. I am not joking. As far as I know, all attempts so far are fallacious or lack lucidity.

In [1] Walton has made the bold claim that the reciprocity formula $v' = -v$ (she typed $v' = v$ which is O.K. if she meant *speeds*) is based on an "incredibly stupid" error. In view of the potential importance of this claim for physics, and for her everlasting fame, she ought to rewrite her argument with *extreme lucidity* and with self-critical iron logic, including a very careful statement of her assumptions. But I think it is an impossible dream.

Certainly, as Walton [3] implies, one should distinguish between (i) the composition of velocities in different systems and (ii) the addition of velocities in a single system. She thinks I confused the two, but the confusion was seemingly hers in [1, p. 126, col. ii] because her equations related t' to t without the factor γ. Her exposition wasn't clear enough for me to be entirely certain. That is why I said in [4, Sec. 2] that the Stockholm committee could ignore [2].

The present letter might conclude our interchange because she says in [5] "our resources being limited they must be directed to find a way out rather than to get stuck in". So she has decided to "change the subject". But she has a point.

In [1] she says incorrectly that Brown & Maia [6] have used the proposition that c = 0. If light indeed had zero speed, then we wouldn't receive radiation from the sun, and dogs would not exist. But dogs exist, therefore c > 0. This is an example of the canine principle. The canine principle, and the feline principle, and the anthropic principle, are all special cases of the biotic principle.

Returning now to Walton's first paragraph, she says that my defense of 4D misses the point, namely that physics deals with 3D and only mathematics deals with 4D. But see [7, p. 59] where Einstein points out the obvious fact that "Classical mechanics, too, is based on the four-dimensional continuum of space and time."

I don't want to discourage anyone from trying to advance physics, whether Newtonian, relativistic or neither. I just wish that people would admit error explicitly and lucidly. This would remove some confusions in the literature.

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A Continuation On Good's "Refutations"

This letter continues on I.J. Good's "Refutations" [1] about the Lorentz transformation (LT):

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

Its differential forms are

$$dx' = \gamma(dx - vdt); \; dt' = \gamma(dt - vdx/c^2) \qquad (2a)$$
$$dy' = dy; dz' = dz. \qquad (2b)$$

I Concerning $t' = t$ From the LT Itself

Good denies the argument that $y' = y$ must lead to $t' = t$, by saying that his opponents "seem to be inferring from $y' = y$ that the y′ axis is the same as the y axis". But, none except the fools contrived by Good would assume so.

It is clear: $y' = y$ gives $y'/C = y/C$; replacing C by c yields $y'/c = y/c$ and then $t' = t$, where C is an arbitrary non-zero constant; c light speed. If Good refutes it, he should prove y'/c not to be equal to t'; y/c not to t. And, gives the answer: what does it mean by y'/c or y/c?

Good's "refutations" demonstrate that he seems to know little of coordinate representation. What does it mean by, say, y' or y? The coordinate (value) y' is *uniquely* defined to be the *projective* component on y'-axis, O- $P_{y'}$, of the path O- P_r for a space point P_r which need *not* be on y'- or y-axis, where $P_{y'}$ is the projection of P_r on y'-axis (*cf.* Fig.2, in Ref.4d).

That is, $y' =$ O- $P_{y'}$, *no* matter how to "view" or "measure". Any different understanding must lead to an absurdity. To convince Good, I dissect his photon-"confirmation" [1a].

The LT, (1a,b), is *assumed* to apply to *any* event in space, say, P_r (*cf.*, Fig.2 in Ref.4d), not only to any event on y-axis. When Good cites, by one party only, the *specific* case of an event (point) on y-axis for his "neat confirmation", he has already made an inexcusable mistake.

For the P_r in general, can Good get his "Bingo" result? The answer must be in the negative.

PROOF: According to Good's approach for two (spatial) dimensions (2-D), the "distance of the event [P_r] from the origin, in the primed system, is (by Pythagoras's theorem)"

$$(x'^2 + y'^2)^{1/2} = [\gamma^2(x - vt)^2 + c^2t^2]^{\frac{1}{2}}, \qquad (3a)$$

"so the speed of the photon in the primed system", denoted by C'_P, is

$$C'_p = \gamma(x^2 - 2vxt + c^2t^2)^{1/2}/t' \qquad (3b)$$

even despite his other errors.

A. If Good remains to take $t' = \gamma t$, (3b) yields

$$C'' = (x^2t^{-2} - 2vxt^{-1} + c^2)^{1/2}. \qquad (4a)$$

If $C'_P \equiv c$ as Good assumed, (4a) gives

$$x^2 - 2vxt \equiv 0, viz., x(x - 2vt) \equiv 0. \qquad (4b)$$

That is, noting that $t \neq 0$ as an assumption has been specified:

$$x \equiv 0 \text{ or/and } v \equiv 0, \qquad (4c)$$

which is in conflict with the assumption of $v \neq 0$!

B. If Good taking $t' = \gamma(t - vx/c^2)$, then from (3b) he gets

$$C'_P = \frac{c\sqrt{x^2 - 2vxt + c^2t^2}}{ct - vx/c}; \qquad (5a)$$

viz.,

$$\frac{\sqrt{x^2 - 2vxt + c^2t^2}}{ct - vx/c} = 1, \qquad (5b)$$

unless he no longer wants $C'_P \equiv c$. From (5b) Good has

$$x^2(1 - v^2/c^2) = 0, i.e., x = 0 \text{ or } v = c, \qquad (5c)$$

which is in conflict with the assumption of $v \neq c$ or/and $v \neq 0$!

In either case, Good's photon has a *variable* speed with x or/and v, as shown in (4a) or (5a), and Good cannot get a "neat confirmation" for $C'_P \equiv c$, unless $v \equiv 0$ or $v \equiv c$. Thus, Good's "Bingo" result fades away into mere shadows. QED

In short, under condition of $1 > v/c > 0$, the assumption of PIVL (principle of the velocity of light in vacuum) Good spares no effort to defend *cannot* hold!

Good speaks of very a lot about coordinate, geometry, and even "economization in notation" etc., but he wins merely against the fools contrived by himself as Einstein was used to do, perhaps due to his economic faculty.

The tragedy I.J.Good played on lies in: (a) He even does not understand "coordinate", a simple but important concept, let lone the LT, false or not; (b) Therefore, he has failed to distinguish between (*x, y, z, t_r*) and (0, *y*, 0, t_y) or (*x*,0,0, t_x). So, at last, although Good speaks of 4-D "universe" a lot as if he were a 4D-vers**er** just coming on his 4-D feet from there, his faculty proves himself to match nothing but a (spatial) one-D line-*man* on y-axis only in the real 3+1-D world; (c) He cannot see, borrowing his own words: *not* "to assume that x and t, etc., have exactly the same meanings [*within* SRT] is logically on a par with a beginning student of coordinate geometry".

II Regarding the 0/0 Type Forms

Since the LT itself is inconsistent, it can of course be invalidated in many different ways.

As is proven [2], the two in (1a) turns out to be a set of 0/0 type equations, as

$$dx' = (dx - vdt)/\sqrt{1 - v^2/c^2}$$
$$= \frac{dx - dxdt/dt}{\sqrt{1 - (dx/dt)^2/(dx/dt)^2}} = 0/0 \qquad (6a)$$

$$dt' = (dt - vdx/c^2)/\sqrt{1 - v^2/c^2} = 0/0 \qquad (6b)$$

which are inevitable result decided by these indisputable relations

$$c = x/t = dx/dt \text{ and } c = x'/t' = dx'/dt'; \qquad (7a)$$
$$v = dx/dt = x/t + C \text{ and } v' = dx'/dt' = x'/t' + C. \qquad (7b)$$

Both (7a,b) hold within the SRT because the LT rests on them: (7a) represents the PIVL; (7b) the relative motion of two frames and thus of *all* counterpart-points in them.

Yet, basic linear algebra firmly identifies that (7a) is in conflict with (7b), unless

$$v \equiv 0 \text{ or } v \equiv c. \qquad (7c)$$

This incompatibility implies that the PIVL violates the long-tested principle of inertia (or, of relativity).

This 0/0 type Eq.(6) is the root cause of why the SRT has produced many paradoxes unsolved yet,

why inconsistent explanations for it appear in different textbooks, and why the LT creates contradicting results so that Good can hunt for his "neat confirmations" to defend it.

Good's one-sided mind leads him to disregard the irrefutable relations in (7b), which are *specified* to be assumptions of the LT, though he said "it is important to hold in mind what the LT means". Good is free to have his $u' = dx'/dt'$; but he has *no* reason to refute the tenable relations $v = dx/dt$ (and $-v = v' = dx'/dt'$). So, it is not that we "have misled" ourselves, but that Good fails to keep it in his mind that the $v = dx/dt$ is the *specified* (relative) speed along the x-axis!

Now, conversely, we prove that the LT must lead itself to the result in (7c).

PROOF 1: A substitution of, say, $x'/t' = a/b$ into (1a,b) yields

$$x'/t' = (x - vt)/(t - vx / c^2) = a/b,$$

where a and b may be *specified* to be non-zero constants, that is,

$$v = x/t = (a + bv)/(b + av / c^2) ,$$

which, in view of $v' = x'/t' = a / b$, amounts to

$$v = (v' + v)/(1 + v'v / c^2) \quad (\neq v)$$

from which one obtains

$$v'(1 - v^2 / c^2) = 0 ,$$

that is, $v' = -v \equiv 0$ or $v \equiv c$. QED.

PROOF 2: Dividing (2a) by (2b) yields

$$v' = dx'/dt' = (dx - vdt)/(dt - vdx / c^2),$$

$$viz., dx - vdt = v'dt - v'vdx / c^2 ,$$

and then,

$$v(dt - v'dx / c^2) = dx - v'dt, viz.,$$

$$v = (dx - v'dt)/(dt - v'dx / c^2)$$

$$= (v - v')/ (1 - vv'/c^2) \quad (\neq v) ,$$

which gives

$$v'(1 - v^2 / c^2) = 0.$$

That is, $v = -v' \equiv 0$ or $v \equiv c$. QED.

By a comparison, suffice it to say that Good's "correct inference" on his Eq.(5) is nothing but a ridiculous fiction misled by himself, though hailed as "an interesting example…", "Bingo".

III The E-L Group Is A Null-Set

Despite pages over pages of matrices and their deductions, parroted from the SRT textbooks, Good proves he does not understand what a group means, as seen below.

As is proven [3], the Einstein-Lorentz (E-L) group is a null-set. I make a brief repetition.

PROOF: The LT is assumed to stem from the alleged 4-D invariant form

$$x'^2 + y'^2 + z'^2 - c^2 t'^2 = x^2 + y^2 + z^2 - c^2 t^2 (= F). \quad (8a)$$

Yet, the LT cannot be derived from (8a) *alone*, because it contains parameter v, but Eq.(8a) *not*.

Undoubtedly, (7b) is indispensable for the LT, and then both (8a) and (7b) *en bloc* form, explicitly or implicitly, a set of simultaneous equations so that the LT can be derived.

Yet, (7b) says that both x and t (and, x' and t') cannot be arguments (independent variables), and *only one* of the two is. Thus, ultimately, (8a) is a *spurious* 4-D but genuine 3-D quadratic. And, the E-L group does *not* meet the definition of a *4*-D group and hence is a null-set. QED.

It seems clear that Good does not understand Eq.(8a) and is unaware of its origin. It should be known that (8a) was first presented by Minkowski, by modelling on the (Euclidean) 3-D form

$$dx'^2 + dy'^2 + dz'^2 = dx^2 + dy^2 + dz^2 = dR , \quad (9)$$

via augmenting the usual 3-D vectors with their respective fourth component.

Yet, this modelling lacks theoretical ground and empirical evidence and hence is in question.

Moreover, this modelling rests on the failure to understand that identity (9) can hold because it bases on the unquestionable assumption of length invariance, a firm premise Euclidean geometry rests on, namely that an ideal-rigid rod preserve its length R in any case without external influence such as temperature change or force, *etc.*, at least *in theory*. But the LT scorns such invariance and hence is incompatible with Euclidean geometry, and it is unjustifiable to use, or model on, (9).

What is the ground for Eq.(8a) when $F \neq 0$? *No tenable* answer can be given by Minkowski or any others, including Good. Whereas it is clear that the LT predicates on0

$$x^2 + y^2 + z^2 - c^2 t^2 = 0 \text{ and } x'^2 + y'^2 + z'^2 - c^2 t'^2 = 0 ,$$
(8b)

a set of alleged light-wave equations that *en bloc* form the PIVL.

Thus, at the best, (8a) can hold only when $F = 0$ and the PIVL can be proven valid.

If $F = 0$ and the PIVL were valid, from (8b) one should obtain

$$x'^2 + y'^2 + z'^2 - c^2 t'^2 = k(x^2 + y^2 + z^2 - c^2 t^2)$$

rather than (8a), where k need not be unity. Any "proof" about $k = 1$, such as given by A. Fock or Einstein himself, can be proven wrong (a proof is omitted here), let alone Good's "reason".

Worse, as I have shown [2], (8a) has been mistaken for being equivalent to

$$x'^2 - c^2 t'^2 = x^2 - c^2 t^2 (= f) \quad (10)$$

in general case of when observed events not on the x-axis, due to the failure to see that time variable t' (or t)

here differs from that in (8a): $t' = x'/c$ here; and $t' = r'/c$ ($\neq x'/c$) there, a trivial but crucial error which has escaped the attention of generations of highly trained scientists!

It is well known that in fact *only the two* of Eq.(1a) are derived from (10). Eq.(1b) is simply irrelevant to (10) and thus (1a), and has as a purely extra-fiction been stuck to (1a).

In other words, at the best, (10) or (1a) can apply to only $(x,0,0, t_x)$ rather than (x, y, z, t_r), and must correspond to

$$y' \equiv y \equiv z' \equiv z \equiv 0,$$

in conflict with (1b)! Thus the LT is not qualified as a coordinate transformation, because the x-axis alone cannot form a (spatial) 3-D frame. Good cannot understand this and still "ignores the equation 1b" in his "refutations" over and over, even after reading our writings!

Now I show that the LT creates, say, contradicting time-rates. Setting $dx = 0$ leads the second equation in (2a) to give

$$dt'/dt = \gamma \qquad (11a)$$

In view of $v = dx / dt,$ the second in (2a) directly yields

$$dt'/dt = \gamma[1 - vdx/(c^2 dx)] = \gamma(1 - v^2 / c^2) = 1/\gamma \qquad (11b)$$

Taking account of (2b), one has

$$dt'/dt = (cdt')/(cdt) = d(ct')/d(ct) = dy'/dy = 1, \qquad (11c)$$

unless $dy'/dy = (dy'/c)/(dy/c) = dt'/dt$ resting on the PIVL can be refuted.

A substitution of (7a) into the second of (2a) yields

$$dt'/dt = \gamma[1 - vdx/(c^2 dt)] = \gamma(1 - \beta); \beta = v/c . \qquad (11d)$$

5. Within the SRT framework the following holds:

$$dt'/dt = (dxdt')/(dtdx) = vdt'/dx = v(dt'/dt)/(dx/dt),$$

and putting $dt'/dt = \gamma(1 - \beta)$ from (11d) into the above yields

$$dt'/dt = v(dt'/dt)/c = \gamma\beta(1 - \beta). \qquad (11e)$$

IV Postscript

With a great pile of empty verbiage copied from textbooks, Good's "refutations" are riddled with mistakes and confusion. They each is untenable and false as the LT.

Good admitted that he does "not know why" we used t instead of his t_1 and t_2 (Good's notation "is definitely wrong"). Indeed he does not know others more, such as: (a) His Eq. (13) is not equivalent to (1a), because (1a) is not (genuine) linear and cannot arrive at (13), though both are false; (b) The Einstein model cannot provide the said free choice, a necessary prerequisite for any correct coordinate transformation in terms of *basic quantities, x, t, etc.*, and cannot tally with observational condition in practice; (c) If our matrix U, despite being false as the LT,

"does correspond to the LT", Good is saying he accepts 1 = 3 for truth, since the U predicates on the same assumption of speed $v = c\beta$, not $v = cB e^{-B}$; (d) Although Good does not understand the "relativity of simultaneity", false or not (it is surely false, see Refs.4a,b), he use it to attack Campbell's argument that those SRT tenets are fictitious. If his "inference" were valid, then, at least, all his "difference forms" such as his Eq.(13) would be defeated by himself ... and so forth.

The rest, such as "stone-egg", etc., is much far beyond Good's faculty, so I stop here.

Good's one-sided mind and dual-standards find everywhere in his articles. By virtue of such logic full of errors, Good would force his opponents to "nobly admit" their "mistakes"!

It perhaps is a waste of time to persuade Good who is free to hold his belief with "Bingo". Yet, if insists his opponents to admit their "mistakes", he "should now nobly say clearly, and without beating about the bush", that his refutation of our "section 1 was mistaken". Or, he withdraws, say, the "Bingo" result refuted by me in section I here. I trust he "not simply to evade the issue".

Anyway, the LT is a set of 0/0 type equations and hence the SRT is nonsense.

No one, including I. J. Good, can rescue the LT from collapsing.

Any challenges from anyone are always welcome. I believe in that the more truth is debated, the clearer it becomes, so that science can advance in the next millennium.

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The Doppler Effect

This is a response to O.J. Campbell's note (October, 1997) entitled "Enigma of light". In his note, Campbell raised two questions: (1) Is light wave propagation affected by an ether or space-time continuum; (2) Is light energy speed a constant? Campbell concludes that, if there were a medium, M, through which light propagates, then the Doppler

effect should be a function of two speeds, the speed of the source with respect to the medium, and the speed of the source with respect to the observer. This is true, but Campbell insists that the Doppler effect depends only on the relative speed between source and observer.

I believe Campbell is wrong about that. The best evidence of a medium, in my opinion, is the discovery of the cosmic background radiation, which serves as a basis for measuring the absolute motion of our planet Earth with respect to the background radiation. This was first measured by Conklin in 1969.

The proper general formula for the Doppler effect in terms of frequency is set forth in my paper on that subject published in *The Toth-Maatian Review* (1996). The Doppler effect is a function of the speed of the source and the speed of the receiver (observer) relative to the background radiation, and the angles between the paths of the source and observer and the path of the light. Time dilation for both the source and the observer must also be taken into account.

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Length Contraction

In *Apeiron* of January-April '98, Prof. Good (p. 111) gives his surrebuttal to my arguments regarding Length Contraction and Time Dilation. Certainly, nothing I can say will persuade him or any member of the Physics Establishment so long as they fail to draw a correct distinction between Mathematics and Physics. The former is merely the "language" of the latter. Perhaps this is why they erroneously equate Special Relativity Theory (physics) with the Lorentz Coordinate Transforms (mathematics). As Dr. Phipps has pointed out in *Heretical Verities*, all the Physics lies in the interpretation of the Mathematics.

In the same issue of *Apeiron*, Mrs. Gertrude Walton addressed this subject in two letters, "Fancifid math?" and "The Twin Paradox does not exist" (p.130), with a profundity that is as admirable as the rhetoric she employs. Please, Prof. Good, ponder well her remarks, for she clearly understands the *essential* differences between mathematical and physical entities and that is the issue here.

When confronted with Common Sense and Right Reason, Establishment physicists (relativists) invariably launch those weapons of mass destruction known as Simultaneity and Inertial Reference Frames. They ignore Essences (definitions) of things like Space,

Motion and Time and invent a mystical ether (the Space-Time Continuum), which since they do not defined it in terms of physical entities like charge, mass and energy, they can assign it whatever properties they please. They ignore the fundamental truth that things like number, space, motion and time are purely mathematical entities. Clocks and rods for measuring time and space (distance) are, of course, physical entities, but failure to draw the distinction between the mathematical and physical, Prof. Good, produces the fallacious.

This error on their part is philosophical as it concerns essences (definitions); so it is not surprising that its genesis lies in the works of a philosopher, Immanuel Kant (1724-1804) who argued that space and time are notions generated in our minds, that our mental faculties impose space and time on things perceived. The resulting conclusion is that man can never know things as they really are; he cannot grasp objective or absolute truth; all truth is subjective and relative. This "relativist" mindset renders debate impossible and leaves ones notions of all of physics, mathematics and everything else "up for grabs".

In spite of the above, it would be a great mistake to entertain any thoughts about the stupidity of the Establishment, because inept, they are not. Prof. Good has demonstrated his brilliance; his *knowledge* of mathematics and physics is par excellence; the problem is with his *understanding* of these subjects. But what I like about Prof. Good is his guileless manner of debate, which exposes a refreshing naivety. Therefore, I can only assume that he has a mind open to Truth and will condescend to rethink his position, bearing in mind that those who reject Establishment physics do so solely because they are compelled to follow the dictates of Common Sense and Right Reason.

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Zero Proper Motion in the Outer Parts of Our Galaxy

For the past 80 years, astronomers have measured the altered frequencies of light as emitted or absorbed by distant objects such as stars, galaxies or quasars; and have interpreted those data in terms of Doppler shifts due to motion.

Some experienced astronomers such as Hoyle, Narlikar, van Flandern and Arp have questioned that interpretation (1-4). They believe that such altered frequencies of light may be due to: (i) loss of energy by light as it travels for huge distances through space;

or (ii) variation in the mass-energy of particles over broad ranges of space or time.

Still, the vast majority of astronomers adhere to a Doppler interpretation of altered light frequencies. When viewed in that way, the experimental data entail further belief in: (a) an ancient Big Bang; (b) very distant quasars; and (c) vast quantities of imaginary dark matter which, by means of its high gravity, keeps fast-moving stars on the outer edge of the Milky Way from flying off into space.

Despite the consensus opinion of these many astronomers, those of us with an inquiring mind might still wish to ask: does the proper motion of distant stars, galaxies or quasars really confirm their apparent motion as deduced from light frequencies? One cannot easily measure proper motion for galaxies or quasars, because of the huge distances involved. But one should be able to measure proper motion for stars in the Milky Way, as a tiny but detectable effect.

In fact, the slight proper motion of stars along the outer edge of the Milky Way has recently been measured accurately, by means of a satellite in space (5). According to the previous Doppler interpretation, stars in that outer part should be spinning about the center with a high speed near 200 km/second, due to the gravitation of imaginary dark matter. However, these new data seem to show little or no proper motion at all, for stars along the outer edge of the Milky Way when moving along their tilted path or "warp". By simple geometrical analysis, I now show that zero proper motion for stars along the outer warp of the Milky Way, means that those stars are essentially stationary, with respect to rotation about the galactic center.

For the purposes of this analysis, let us model the Milky Way in terms of two circles drawn on a sheet of paper. Let the inner circle, which contains the Sun and Earth, lie at a radius of 8 kiloparsecs (kpc) from the center. Next let the outer circle, which contains the outer stars, lie at a radius of 11 kpc from the center. Finally, let us tilt the outer circle by 4 degrees in an up-down sense, so that it lies slightly above or below the paper on either end. That slight tilt represents a non-linear "warp", as seen for stars at either end of our galaxy: see Figure 2 of ref. 5.

Next let us consider the motions of stars along those two circles, in a rotational sense about the center. As a preliminary model, let us suppose that stars might be moving along both circles with an equal frequency of orbit. In that case, no relative motion in the horizontal plane should be detectable, for any star in the outer circle as viewed from Earth. Stars along the outer circle will move with a velocity that is 11/8 larger than along the inner, so as to pro-

vide for an equal frequency of orbit despite a larger radius of spin.

For example, if we assign a velocity to the inner circle as $v(8) = 220$ km/second, then the velocity along the outer circle must be $v(11) = 220 \times 11 / 8 = 300$ km/second, for an orbit of equal frequency. Those values of 220 and 300 km/second agree roughly with the apparent rotational speeds of stars in the Milky Way, when deduced from a Doppler interpretation of light frequencies.

Finally we can ask: do such high apparent velocities of rotation as $v(8) = 220$ km/second for the Earth, and $v(11) = 300$ km/second for the warp, agree with proper motions of stars in the warp as seen from Earth? Because the outer circle is tilted by 4 degrees relative to the inner, stars along the outer circle should show a large up-down component of proper motion as seen from Earth, equal to $v(z) = +13$ km/second for $v(8)$ and $v(11)$ as given above. Yet our predicted value of $v(z) = +13$ disagrees strongly with the observed proper motion of stars in the warp, which lies close to $v(z) = 0$ on average: see Figures 3a and 3b of ref. 5. Indeed, the authors say: "We find that although the spatial distribution of stars agrees with previous studies, the velocity distribution has the opposite sign to that expected. Finding a plausible explanation of this result may be the key to solving the long-standing puzzle posed by galactic warps."

How can we fix our model for the Milky Way, to make it agree with new data on proper motion? The only plausible solution is to assign $v(11) = 0$ or thereabouts as a velocity for the outer circle, so that stars in the outer warp will hardly be rotating in an absolute sense, about the center of our galaxy. In other words, if stars in the outer circle show no up-down motion $v(z)$ relative to Earth, that can only be because the vectors for absolute motion $v(11)$ have gone to zero. Thus, any absolute motion $v(11)$ along the outer circle would create a non-zero relative motion $v(z)$ as seen from Earth, since the outer circle is tilted by 4 degrees, while Earth lies in the galactic plane.

Still, stars in that outer circle should show a horizontal component $v(xy)$ of motion relative to Earth, since the Earth may be rotating forward with some velocity $v(8)$. Hence, an observer on Earth will see those outer stars moving "backwards" in a relative sense $v(xy)$ within the plane, although not up or down as $v(z)$ out of the plane. Precise data on proper motion of stars within the galactic plane have not yet been published, so we cannot assign the value of $v(8)$. If the model from ref. 5 is correct on a relative scale, one can deduce that $v(xy) = -70$ km/second, and so $v(8) = +70$ km/second as an approximate result.

Proper motions for stars within the inner part of the Milky Way are not known with as much accuracy (6). They seem to show an increase when going in towards the center, as if those inner parts might be spinning more rapidly than Sun and Earth.

In summary, these new data on proper motion of stars in the outer part of the Milky Way, would seem to argue against the interpretation of altered light frequencies as Doppler shifts, in the case of galaxy spin. The blue-red gradient of light frequencies as seen across the widths of many galaxies, would seem therefore to represent some gradient of mass or time, rather than any Doppler shift (7,8).

Finally, the origin and structure of galaxies may be clarified through the analysis given above. Let us suppose that most galaxies were formed originally, through the large-scale ejection of matter from a central energetic source, as proposed by Arp (2). In that case, matter may be ejected in two opposing directions, so as to create two spiral arms as observed. Also, such matter will eventually lose its initial velocity and become stationary in the outer parts, as it is pulled to the center by some weak gravity, which may be reduced or "screened" over wide regions of space (4).

Now in order to explain the "warp", let us suppose that the plane of ejection may have changed slightly, say by 4 degrees while the Milky Way was being formed. That would then explain why the Milky Way and many other spiral galaxies appear "warped", and hence show a slightly different planes for their outer parts versus their inner. In other words, we seem to see in the nearly-stationary outer parts of modern galaxies, a historical record of how they were formed billions of years ago. If those outer parts were really rotating with high speeds of $v(11) = 200$ km/second, instead of $v(11) = 0$ as found here, such a historical record could never have persisted to the present time.

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Electromagnetic Retardation and Theory of Relativity

I just saw the Internet version of the January-April, 1998, issue of *Apeiron* (I have been reading APEIRON on the Internet for some time now). Thank you very much for publishing Hillion's review of my book. I am very pleased indeed with the review. If possible, please indicate in the next issue of *Apeiron* that the actual title of the book is "Electromagnetic Retardation and Theory of Relativity," that the price of the book is $40.00 US, and that the publisher's address if P.O. Box 4132, Star City, WV 26504-4132, USA.

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Relativity and a Large, Dense Sphere of Electrons

Einstein [1905] defined our present concept of relativity when he wrote, "… the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good." He named this "The Principle of Relativity."

For Einstein's concept of relativity to be correct, the laws of electrostatics and electromagnetics must not lead to irreconcilable results when viewed from two unaccelerated inertial coordinate systems moving linearly toward or away from each other.

It appears, though, that at least one irreconcilable pair of results exists.

At the origin of a 'moving' coordinate system, an observer produces a large, dense sphere of electrons. The observer watches the sphere expand equally in all directions due to electrostatic repulsion. No magnetic field is apparent.

The 'moving' coordinate system is approaching rapidly (almost the speed of light) a nearby 'stationary' coordinate system. The 'stationary' observer at the origin of the 'stationary' system first sees the rapidly approaching sphere of electrons within a strong, self-generated magnetic field. The field squeezes the sphere into a negatively-charged rod,

approaching the 'stationary' observer lengthwise. The near end of the rod is moving more rapidly toward the 'stationary' observer than is the far end of the rod.

The 'stationary' observer is not worried because a simple mathematical exercise reveals that the electrons are really in the form of a quickly expanding sphere in the 'moving' system.

The near end of the rod reaches him and, unfortunately, the concentrated beta rays burn and punch a large hole through the 'stationary' observer's chest. This leaves him dead, and leaves the 'moving' observer wondering how an electron or two from his greatly expanded sphere of electrons could possibly produce that much damage.

The views of the 'stationary' and 'moving' observers are not compatible. The electrons cannot be greatly expanded and tightly concentrated at the same time.

This brings me to several questions. First, for the relativists: 1. Can these two disparate views be reconciled from the viewpoint of relativity? If yes, please show us how. 2. If no, what does this tell us about our understanding of our universe? 3. If no, what does this tell us about our present concept of relativity, and 4. what does this say about any theory based on our present concept of relativity?

Second, for the nuclear physics theoreticians: 5. This squeeze effect on rapidly-moving 'like-charges' also occurs for the charged quarks of the same electric sign that are most numerous within each charged nucleus. Quarks with charges of sign opposite to the foregoing 'like-charges' are magnetically repelled from the 'squeezed' quarks despite their own weaker 'squeeze' effect. These effects imply a change in state from when the nucleus is at rest which, in turn, suggests that the decay time of the nucleus might be changed from when at rest. What does this tell us about any possible speed-dependent mean decay time for mesons? 6. Can a mathematical expression for any possible speed-dependent mean decay time of the meson be developed? If yes, please show us the development.

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Again on the Marinov Motor

We report some preliminary experimental research about the (siberian coliu) Marinov motor which (except in the sign of rotation) are in rough accordance with Wesley theory. Moreover, we were able to make, by connecting in series 50 loops, the above machine additive. Continuous rotation of the rotor was achieved resorting to a well-known design of conmutation. We hope to describe thoroughly our motor in a couple of months.

Introduction

Some months ago Paul Wesley wrote us in connection with the Marinov motor, clearly described by him in this journal[1] inviting us to perform a straightforward experiment in which a mercury ring would be the rotor. We have attempted to perform the above but failed to observe motion. Promptly we recognise that inertia and surface tension of mercury are indeed too high as to mask the electromagnetic force. During October-November '97 Tom Phipps (with whom we searched on VACE in a related matter[2]) put in our knowledge[3] that Tom Ligon succeeded with the reciprocal arrangement: the ring at rest and the toroidal magnet free to rotate about the vertical axis.

The last two months we repeated some of the experiments performed by Phipps crudely verifying his outputs. Now we are searching about the additivity of torque and for the counter-electromotive force (back emf).

Adding Rings

Figure 1 sketchs the two first loops of our first additive siberian coliu. The (50) rings located as in Wesley (fig1) configuration. We employed a (Alnico5, 3cm wide, 8cm height, 0.8 cm thickness) one piece, quasi perfect toroid enclosing some 6,000 gauss, with dispersive flux amounting no more than 30 gauss, as measured in the ecuatorial plane, at the place were we located the coil. With the aid of the above device we have verified most of the Phipps measurements4 . We don't report here numerical figures but we wish to emphasize that $T50 \approx 50$ T1 being T1 the torque acting upon a isolated loop. The toroid was suspended with the aid of a 250 μ diameter stainless steel wire. Our observations agree with that due to Phipps[4] as far as torque and sense of rotation concerns, but disagree with Wesley calculations at last point. Up to now we cannot advance any physically tenable explanation about this extrange fact (we were unable to find flaws in the Wesley derivation). Another point yet advanced by Phipps[4] , we confirmed in our lab is the failure of (ferromagnetic core) electromagnets for torque production.
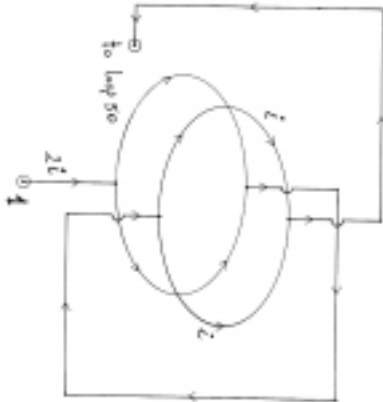
Toroid fixed, coil free to rotate

When the (50 loops) rotating coil was suspended (with the aid of the above steel wire), remaining the armature (toroid) anchored to the lab, a vigorous rotation of the coil was observed after passing some 10 DCA. We feed the device with the aid of two circular troughs of mercury in which the copper threads 1-50 were immersed. When the coil was epoxied to the armature, both suspended as above, rotation doesn't take place (we reached up to ca. 100 A DC in very brief pulses). From the above follows angular momentum conservation, as expected.

After our deceptive experiments with the (as suggested by Wesley) mercury ring, we succeeded when replacing the above by a highly saline water solution, at currents of some 20 DCA. For experiments consuming a few seconds electrolysis was not a hard problem.

The puzzle of back emf

The next step to be reported in this preliminary letter concerns with the unavoidable question related to back emf. Previous correspondence with Costa de Beauregard[5] showed the importance to search the siberian-coliu behaviour as generator. For this purpose we anchored the (50 loops) coil to the lab and rotate the armature up to 1.200 rpm. Besides the random noise we were unable to detect any relevant voltage. Our oscilloscope sensitivity was better than 0,1 mV. A crucial question arises: What is the braking force in the Marinov motor? If you start the motor with 10 DC Volt, after reaching (say) 10.000 rpm , the same 10 DC Volt will drive current into the coil. At first sight, only (mechanical) frictional forces are the candidate to brake the armature. Going into the theoretical realm, the observation deserves further investigation since we know that, for a time varying A, we get $Eind = -\partial A/\partial t$. In our arrangement rotation is the responsible for a (motional) time-varying potential vector in the bulk of the wires. We

only measure $\oint Eind.dl = 0$, but we don't know about Eind at an arbitrary point.

Final considerations

We feel the physics community is indebted to S. Marinov, P. Wesley and T. Phipps,Jr. for his bizarre attitude as far as the search of truth concerns.

Acknowledgement: To Cambio F.Vaccaro S.A (Baires).

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Address to the *'98 ISCFPNS* participants and global scientific community

Taking this opportunity offered by the **'98 I**nternational Scientific Congress on *Fundamental Problems of Natural Science* (St.-Petersburg, June 22-27, 1998), we solemnly make it known to the respectable participants all here and global scientific community that:

Mathematical occurrences demonstrate that the well-known Lorentz transformation is a set of intrinsically inconsistent and illegitimate equations, misled by the "leading scientists", such as H. Poincare, A. Einstein, and H. Minkowski, due to their, at least, failure to *distinguish* between (x, y, z, t_r) and $(x, 0, 0, t)$ as seen in the attached material.

No one can rescue the Lorentz transformation from collapsing.

It thus is simply ironic to speak of experimental "confirmations" for the Relativity, which is doomed to vanish, along with all its derivatives, such as length-contraction and time-dilation, relativistic speed law, relativistic Doppler formulae and relativistic space-time theory… up to the big bang.

Now more and more scientists who read our critical papers come to give up the improper belief that the problem with the Relativity theory is not in

mathematics but in physical explanation of experiments only.

Where is the way out for modern science? The answer may be found from the References [1], [2] and [4] in the attached material.

All in all, Contemporary science, especially basic physics, astrophysics and cosmology, needs fundamental mathematical and physical revisions; or it is impossible to advance modern science in the next millennium.

No One Can Save the LT From Collapsing

With usual meanings for symbols below the Lorentz transformation (LT)

$$t' = \gamma(t - vx/c^2); \ x' = \gamma(x - vt) \tag{1a}$$

$$y' = y; \ z' = z \tag{1b}$$

$$\gamma = 1/\sqrt{1 - v^2/c^2}$$

seems to be well accepted but can be proven invalid, as below.

I. The LT is *assumed* to apply to any event in 4-D space, including, say, an event (point) P_r with coordinates $y \neq 0$ and $z \neq 0$. Yet, Eq.(1a) *cannot* describe the P_r but any event on the x-axis *only*.

PROOF The point P_r with $y \neq 0$ and $z \neq 0$ have primed coordinates

$$(x', y', z', t'),$$

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

Substituting (1) into the above yields

$$t' = \ '_r = \sqrt{\gamma^2(x - vt)^2 + y^2 + z^2}/c,$$

which is sure to *contain* $y \neq 0$ and $z \neq 0$. But, the first equation of (1a)

$$t' = \gamma(t - vx/c^2)$$

has *no y* and *z* terms, and hence cannot describe the P_r ...QED

In other words, Eq.(1a) requires

$$y' \equiv y \equiv z' \equiv z \equiv 0,$$

which is in conflict with (1b)!

II. All equations in (1a,b) *en bloc* are *assumed* to derive from

$$x'^2 + y'^2 + z'^2 - c^2 t'^2 \equiv x^2 + y^2 + z^2 - c^2 t^2, \tag{2a}$$

or $\qquad r' - ct' = 0 \ \text{ and } \ r - ct = 0 \tag{2b}$

where

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

$$t = \sqrt{x^2 + y^2 + z^2}/c = r/c = \ _r.$$

Yet, *in fact only* (1a) stems from

$$x'^2 - c^2 t'^2 \equiv x^2 - c^2 t^2$$

which is not equivalent to (2) due to different time variables: here $t' = x'/c = \ '_x$ and $t = x/c = \ _x$; but in (2), $t' \neq r'/c = \ '_r$ and $t \neq r/c = \ _r$.

Thus the LT is good for nothing except as evidence that Einstein and Poincare (et al) have, at least, *failed* to *distinguish* between $(x', 0, 0, \ '_x)$ and $(x', y', z', \ '_r)$, a trivial but glaring error which has escaped the attention of generations of scientists.

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[4] Xu Shaozhi & Xu Xiangqun: *Physics Essays*, 9(3) 1996, p.380.

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Question: Negative Group Delays and Action-at-a-Distance

In the recent issue of *American Journal of Physics*, M. W. Mitchell and R. Y. Chiao[1] discuss the causality and negative group delays in the series of the experiments on the superluminal propagation of electromagnetic waves (see references therein).

We have a question. On p. 14 the authors write:

> *"…it is not the group velocity, but rather the front velocity that must be no greater than* c *by Einstein causality;"*

And on the other hand, on p. 17 the authors write:

> *"...the front... reaches the input and the output of each amplifier at the same time."*

We do not understand how one should reconcile these two expressions. Even if one assumes that the time of flight for light to cross from the input to the output is completely negligible on the scale of the typical time scales for the pulses and "the front reaches the input and the output of each amplifier at the [*almost*] same time," it is also not comprehensible for us (in the framework of the Einsteinian claim about the *c* as the limiting velocity) the following expression [p.18, Eq. (12)]:

> *"...the output depends only on the present and past values of the input (and not on future values)."*

Theoretically we agree (we are theorists). But, in our opinion, this statement (in comparison with the previous statements) also can cause misunderstandings: if the output depends on the *present* values of the input (see the term $V_{in}(t)$ in Eq. (12), *t* is the same time, not retarded!), does the above statement signify

that *there is* information in the output which comes with "*infinite* velocity," thus confirming action at a distance[2], does not it?

In this context we wonder if the formalism of the causal Green function, $V_{out}(t) = V_{in}(t) + \{$retarded terms$\}$, Eq. (12)[1], contradicts what Einstein and his successors told us? We wonder, if it is possible to construct some device in order to check experimentally, whether the front comes at the *same time* or at the *almost same* time.

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[1] M. W. Mitchell and R. Y. Chiao, "Causality and negative group delay in a simple bandpass amplifier," *Am. J. Phys.* **66** (1), 14-19 (1998).

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Author's query: Large Numbers

For an updated survey article on the LNH, I would like references, unpublished articles, *etc*. on attempts to explain the large number coincidences. Please send replies to Martin Kokus at: HCR64, BOX32 Harrisonville, PA. 17228, USA; kokus@mail.cvn.net.

ERRATA, Vol 5, No. 1-2

Page 116:
Left col. 2nd para., line 1: "Nobility" should read "Nobelity"
Right col., five lines below (1b): The meaningless expression should be (x',t').

Page 117:
Left col.: The misprinted formula should read:
$$dx - v dt = v' dt - v v' dx / c^2$$

Page 119:
Left col., lines 7 & 8: Quote signs look like primes

Page 120:
Line 11: "is" should read "in"

Page 124:
Line 18, left column: Fig.1 should read Fig.2
Line 12, right column: x'<not equal to> x should read x'= γr(x - vt)

Page 125:
Line 11, left column: in Eq.(A4): R(x')= R(x) read R(x') <not equal to> R(x)
The same column, in Eq.(A6): R(y')0 R(y) should read R(y') <not equal to> R(y)

Page 126:
Line 11, left column: Fig.1 in should read Fig.2 in
The same column, in Eq.(C): t'=γr(t - vx)/c² read t'=γr(t - vx/c²)
Line 14, right column: t'= x/c should read t'= x'/c
The same column, the line above Theorem 2.2: theorem 3 read theorem 2
The next line: Theorem 2.2 should read Theorem 2

Page 127:
Last line from bottom, left column: acorn read scorn
Line 3, right column: IV should read V
Line 16, the same column: theorem 2 should read theorem 3
Line 18, the same column: Theorem 2 should read Theorem 3
Line 8 from bottom, the same column: physicist should read defender of the LT.