@ ISSUE

Correspondence, conference threads and debate.

Magnetic monopoles would not conserve energy

Magnetic monopoles are incompatible with the Lorentz-invariant electromagnetic conservation law for 4-momentum. A mechanism fueled by a magnetic monopole charge would provide free energy.

If one takes conservation of energy in an electrostatic field using the electrostatic potential, and then demands Lorentz invariance, the result is a law of conservation of 4-momentum using the electromagnetic 3-potential A. More traditionally, one regains conservation of energy with the transformed electrostatic potential, and then adds the conservation of momentum provided by the vector potential A created by the Lorentz transform.

This is fairly simple compared to much of physics, but self-appointed "authorities" on the Internet admit their ignorance of any such conservation law. They actually prefer to believe in magnetic monopoles, and make the bald unsupported claim that this argument has no merit.

One can also derive this conservation law with variational methods or from the wave equation for a charged particle. But deriving the 4-potential A from Maxwell's equations yields a "gauge freedom" not allowed by other derivations. This gauge freedom can be used with a careless lack of insight to thoroughly obscure the conservation law. Only the Lorentz gauge, which requires no closed surfaces, no bulges in surfaces, and no surfaces at infinity (when using the 1-form representation of the potential) allows the confirmation of the conservation law which is easily seen using the other methods. The gauge freedom in this context is a physically meaningless artifact of solving differential equations by integration.

A very elegant argument exists which shows that, given a 4-potential A, magnetic monopoles must not exist. So, if experts insist on monopoles, they must sacrifice the conservation law discussed above. Even gauge freedom cannot refute this argument. For beautifully abstract generalities, see Misner, Thorne and Wheeler (1973).

The elementary form of the argument is that the density of the magnetic monopoles would be, by definition, the divergence of the curl of the vector potential A, which is identically zero. Therefore, when considering the requirement of energy and momentum conservation, one sees that the "symmetric" form of Maxwell's equations is not their natural form. Indeed, their natural form is as topological theorems which rule out the possibility of magnetic monopoles.

Magnetic monopoles force the 4-potential A to be undefined. To rub the point in, one can verify the following design for a free-energy machine which depends on a magnetic monopole charge. There is an amusing silence on the Internet over this design.

Mount a positive charge on an arm which rotates counter-clockwise on a frame. Allow the frame to wobble on a (vertical) perpendicular axis to the axis of the rotation. Attach a spring to the frame (to the side) off-axis of the wobble and rotation, and directed (horizontally toward the viewer) out of the plane of rotation, so the frame can be adjusted to resonate in its wobble with the rotation of the arm. Mount a negative charge out of the plane of rotation in the average direction of the axis of rotation (using the right hand rule this would be toward the viewer). Now mount a north magnetic monopole at the axis of the arm.

The out-of-plane charge will deflect the charge when it rotates counter-clockwise into a resonant wobble such that the magnetic monopole will propel the charge forward in its rotation. The deflection by the negative charge cancels and reverses the wobble which would be induced by the magnetic force in opposite phase. But the cancellation by the negative charge does not extend to the torque exerted by the magnetic force on the wobble axis. It is advanced by one quarter rotation and equal but opposite in direction to the magnetic torque on the rotating charge. So energy conservation is violated by this mechanism, but angular momentum is somewhat conserved.

The mechanism may still operate when a normal north magnetic pole is substituted for the monopole. But then the 4-potential is well defined. Conservation of energy would require the demagnetization of the ordinary magnet in order to fuel the rotation of the arm.

One adjusts the spring so that the wobble is in resonance with the rotation of the arm. With the axis of wobble directed up from the standpoint of the out-of-plane negative charge, then at the top of the arm's orbit, the axis of rotation is deflected at a maximum to the left of the charge. When the arm is at bottom, the axis is deflected at a maximum to the right.

The charge on its motion down, then, is also wobbling toward the negative charge. This motion toward the negative charge causes a magnetic force from the monopole to impel it faster downward.

When the charge is moving up, and again wobbling towards (not away from) the negative charge, the magnetic force impels it faster upward.

Correctly thinking about this mechanism involves overcoming difficulties in separating the compound motion of the arm into independent components, and making subtle appeals to conservation of angular momentum.

A spherical coordinate system centred on the monopole with a vertical axis serves to separate the motion of the arm into an independent rotation and wobble. The force of the monopole on the charged arm can be seen to always have a component in the direction of rotation (latitudinal).

Conservation of angular momentum by electrostatic forces can be used to show that the attraction of the negative charge does not stall the rotation.

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Experimental test of tired light

Regarding the "Tired Light" hypothesis—*i.e.*, that redshift could be a function of distance traversed, two experiments come to mind.

One experiment would look at a galaxy with an enormous redshift. By present accepted theory, this galaxy should be receding from us at a speed greater than 50% that of light (many such galaxies with this kind of redshift have been discovered).

The apparent magnitude of the galaxy would then be precisely measured. Then, several years latter, it would be re-measured. If the red shift is a Doppler effect, the galaxy should have receded from us with enough distance that the apparent magnitude should be slightly fainter. Failure of the galaxy to "dim" would support the tired-light hypothesis.

The obvious problem with this is that the optical instruments would have to have a sensitivity of 1 part per several billion. I doubt if present day instruments could be calibrated to this kind of sensitivity.

Another experiment involves sound. There are many parallels between light and sound propagation. The velocity of light propagation through the "space medium" is constant, as is the velocity of sound through

the air medium (assuming unchanging pressure and temperature). Both follow the formula velocity = wavelength times frequency, although the numbers are vastly different. The Doppler effect is also operative in both systems (a locomotive moving away from a stationary observer will seem to have a lower pitch—*i.e.*, the observed frequency will be "red shifted").

For the experiment, point A would be a loud noise generator of one specific frequency. Stationary point B would be 10 meters away from point A and contain a detector to measure the precise frequency emanating from point A. Stationary point C would be several kilometers from point A and would also contain a detector to precisely measure the frequency of the sound coming (faintly) from point A. This experiment assumes no wind and a constant temperature. The result of a lower or "redshifted" frequency detected at point C compared to point B, while not proving the tired light hypothesis would certainly lend support and credence to it.

One could envision a possible mechanism to explain this unique result. Again given the formula $v = ln$, energy in many wave propagating systems is thought to be a partial inverse function of l, the wavelength. The greater the energy, the shorter the wavelength. As energy is lost in sound transmission over a large distance (by displacement of air molecules), the wavelength could be envisioned as becoming longer (having less energy). Since v in the above formula is constant, n would have to decrease (become redshifted).

One additional point. I have talked to several acoustical engineers about this type of experiment. No one could recall anything like this having ever been done.

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Autodynamics

I have seen hundreds of new theories trying to explain different aspects of the physical world. One theory has caught my attention because it is complete in its application and offers concrete formulas directly comparable to Newton's and Einstein's.

I am referring to Autodynamics (AD), a short name for "Fundamental Bases for a New Relativistic Mechanics.." The Autodynamics equations (Walz 1984) are written below with the Special Relativity (SR) equations. At first glance, the two sets of equations appear similar, but in fact they are very different.

Special Relativity

$$E = m_o c^2$$

$$m = m_o \left| 1 - b^2 \right|^{-\frac{1}{2}}$$

$$KE = m_o c^2 \left[\left| 1 - b^2 \right|^{-\frac{1}{2}} - 1 \right]$$

$$p = m_o v \left| 1 - b^2 \right|^{-\frac{1}{2}}$$

$$e = e_o \text{ (constant)}$$

Autodynamics

$$E = m_o c^2$$

$$m = m_o \left| 1 - b^2 \right|^{\frac{1}{2}}$$

$$KE = m_o c^2 \left[1 - \left| 1 - b^2 \right|^{\frac{1}{2}} \right]$$

$$p = m_o v \left| 1 - b^2 \right|^{\frac{1}{2}}$$

$$e = e_o \left| 1 - b^2 \right|^{\frac{1}{2}}$$

The fundamental difference between the Special Relativity and Autodynamics equations is creating a revolution in physics.

The most important aspect of AD is that it explains the full experimental spectrum of modem physics. In decay processes like the muon (Carezani 1992) and the pion, *etc.*, AD explains all the experimental results without resorting to the neutrino or other penetrating radiation (Buechner 1946). It explains electron-electron and especially proton-proton annihilation. in nucleus-nucleus collisions, AD explains the smaller Linear Momentum Transfer (LMT) found experimentally (Carezani 1995). AD gives a new and complete equation for the Compton effect (Carezani 1993). "Energy absorption-mass decay" Autodynamics concepts drive AD to create a new gravitational quantum theory that introduces a new discussion of the second law of entropy, cosmologically speaking. This theory explains perfectly the perihelion advance of all the planets. As we know, Special Relativity can explain only Mercury's perihelion advance. Of course Autodynamics gives a clear interpretation of the phenomenon of RaE decay (Carezani 1988) and explains perfectly the experimental value of 0.35 MeV as decay energy. AD simply explains the K electron capture, the smaller experimental decay energy in U^{238}, the anomalous mean pass, *etc.* In a paper on nucleus-nucleus collision the author proposes a chain reaction by mixing common and chip materials to generate energy by decaying nuclei. This idea was suggested by the experimental results which showed that energy after compound nucleus decay is greater than the energy of the bombarding nucleus. Superluminal velocity is not strange to AD and an outline of one experiment conceived by AD to generate energy traveling faster than light speed is given.

The derivation of AD is astonishingly simple. Through a detailed discussion of Galilean systems in relative motion, Autodynamics demonstrates that it is possible to describe physics using only one frame of coordinates. AD demonstrates that setting the phenomenon velocity equal to the velocity of the frame in relative motion to simplify the equations that lead to the

Lorentz coefficient makes no sense, and introduces many contradictions within SR. A physical system is defined by an "observer" and the "observed." This leads to the conclusion that it is possible to simplify Lorentz's equations. Another way to arrive at the same conclusion is to observe a phenomenon from two different locations and make the coordinates x', y', z' and t a function of t. The following equations are found:

$$x' = \frac{vt}{\left| 1 - b^2 \right|^{\frac{1}{2}}} \qquad t' = \frac{t}{\left| 1 - b^2 \right|^{\frac{1}{2}}}$$

Starting from these equations it is very easy to find the equations shown at the beginning.

It is necessary to point out that AD applies to decay phenomena. Immediately a question arises: What happens to a particle accelerated inside an accelerator? The AD interpretation is very simple: the particle absorbs the electromagnetic energy to decay later. To AD a particle inside an accelerator increases its kinetic energy (KE) and its momentum but not its mass. In SR the particle increases its KE, its momentum and its mass. That is the reason SR fails when its equations are applied to decay phenomena. Since the momentum and energy in SR are greater than the experimental result found, there is an apparent lack of momentum and energy conservation and SR explains this difference by introducing an *ad hoc* theory, the neutrino hypothesis. AD does not have this problem because the values calculated with its equations are equal to the experimental values, and consequently the energy and momentum are conserved. In AD the neutrino has no meaning.

The same thing happens in the Compton effect. In SR the photon strikes the electron, flying away with less energy. In AD the photon is absorbed by the electron, forming a new particle, to decay immediately.

If in the AD KE equation we introduce the particle rest mass plus the KE energy of the particle when it is accelerated by external energy, it is possible to demonstrate simply that the AD KE equation evolves into the SR KE equation. The AD KE equation is general. If the particle decays, the AD KE equation is correct. If the particle receives energy from an external medium the equation explains this phenomenon as well.

It is impossible in a short article such as this to develop a complete and thorough explanation of Autodynamics. Its argument and the new and huge extent of the experiments are many however, and they fully support this theory.

If the reader is interested in more information or original papers, please write to the Editor of this magazine.

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Numerical coincidences and the constant of gravity

Two systems of numbers. In a recent article, Arp (*Apeiron* 2(2):41) attempts to classify the objects in the Universe, from elementary particles (electron, muons, nucleons) up to stars, planets and their satellites, as well as galaxies (including their masses and dimensions and separation). He proposes the number 1.228 as a base, raised to a whole or half-integer power.

The author proposes replacing 1.7275 (the Titius number) with 1.228 to determine distances between the planets and the sun. This same number also occurs in fractals and in the study of quasars.

Since 1982, in submissions to the Academy of Sciences, I have been proposing a numerical system that gives masses as $m = 210^n$ (where n is a multiple of 1.5) and dimensions as $m = d^2$ (where d is diameter). None of this work was published until 1991 (*Apeiron* 11:13).

Since the two series proposed by Arp and myself represent the same objects, they must obviously be compatible with one another. Now, it is easy to see that

$$P = A^{26}$$

with $P = 210$ and $A = 1.22833302$. I therefore propose this relation as a definition of the mysterious A. We give two examples of the correspondence between the two numbering systems for two major objects, a star and a galaxy.

Object	Pesteil	Arp
Star	P^{27}	A^{102}
Galaxy	$p^{31.5}$	A^{819}

(masses are expressed in fundamental units: $m_0 = 4.395 \times 10^{-30}$ g.) The correspondence between these two methods of calculation is remarkable.

A few comments on these two numbering systems are in order. 1) Arp divides the universe into fine slices, whereas in my system, the formulae represent the most likely averages for the same types of object. The masses are dispersed from 1 to 300 around the calculated value. Here are a few examples:

Atoms (210^3): this number represents atom No. 24, whereas the atomic numbers extend from 1 to about 250.

Stars (210^{27}): masses extend from 1 to approximately 300.

Galaxies ($210^{31.5}$): masses of galaxies also extend from 1 to 300 around this average.

2) The relation I propose between m and d ($m = d^2$), in the fundamental system of units fits the electron, nucleons, the Sun and our Galaxy remarkably well; but it is contrary to fractal theory, where we have $m = d^{1.23}$). I believe that the definition of diameter for fractals is different from mine. This problem obviously requires further investigation.

3) The base $P = 210$ used to give the maximum probability of masses for objects in the Universe started from the *exact numerization* of nucleons. It makes it possible to calculate the magnetic moment of these particles and yields the so-called fundamental system of units whereby all the important magnitudes of microphysics are expressed as a function of a_e, the fine structure constant. Thus, P has at least some theoretical foundation.

A formula for G. Z. Akil (*Apeiron* 12: 5) proposed

$$G = \left[4p^2 m_p m_u \big/ m_e^2 \right]^{-1} = A,$$

the gravitational constant (where m is mass, m is the muon, p is the proton and e is the electron).

This extremely exact formula is peculiar for three reasons:

a) the appearance the proton, muon and electron masses is unexpected; in any case, it has not been accounted for by theoreticians.

b) G has a dimension, while this ratio of masses does not.

c) the metric system appears to have been designed with G in mind.

However, since the formula appears unimpeachable, I tried to restore a dimension to the second member of the equality ($m^{-1} l^3 t^{-2}$) which I chose a long time ago as the value of G: dc^2/m. In the present case, this function, in c.g.s., is equal to 1, and in S.I, to 10^{-3}.

The problem is obviously to find the object in the Universe that best matches this equation. The best choice is our own Galaxy, since for $c = 2.99793 \times 10^{10}$ cm s^{-1} (after switching to fundamental units and back to c.g.s.), we find $m = 2.808969 \times 10^{44}$ g and $d = 3.1253539 \times 10^{23}$ cm.

For G, I therefore propose the formula

$$G = \left| \frac{4p^2 m_p m_u}{m_e^2} \right|^{-1} \frac{dc^2}{m_{Gal}}$$

which is valid in all systems of units. However, it should be noted that objection (c) still applies.

Continuous creation. Since 1988 (in my submissions to the Academy of Sciences) and in 1991 (in this journal) I have adopted the hypothesis of continuous creation of matter governed by the law $dn/dt = Kn$ (where n is the number of nucleons already created, and K is the rate of production). I also admitted the relation $m = d^2$, connecting the mass and diameter of objects in a "fundamental" system. The creation law can also be written $dn/dt = K/m_{Nu} d^2$, where m_{Nu} is the mass of a nucleon and d is the diameter of the mass created. The number of nucleons created per unit time is thus proportional to the diameter of the sphere occupied by nucleons already produced. What I propose, therefore, is matter creation that is not central or generalized, but originating at a source at the edge of the Universe.

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More meetings by dissident physicists in North America

In the report appearing in the Oct. 1994 issue of *Apeiron* about the June 1994 meeting of dissident physicists in San Francisco, it was announced that the most favorable meeting site for the same group in 1995 would likely be in Norman, Oklahoma, in connection with the annual gathering of the Southwestern and Rocky Mountain (SWARM) Division of the American Association for the Advancement of Science. This forecast was correct: the very genial and tolerant director of the SWARM division, biologist Donald Nash, allowed symposium status for all of our sessions, which took place from 22 to 24 May. This meant advance publicity around the region, resulting in several previously unknown dissidents attending in person, and making valuable contributions to our discussions.

The much greater time for discussion, within the scheduled sessions and also outside of them, marked this meeting as considerably more valuable for the attendees than were our cramped series of short papers—all we were allowed there—in S.F. In Norman we also had a much larger and more comfortable room. Out of 24 authors on the program, 22 showed up in person (compared to 33 and 29, respectively, for S.F.). There were many fewer *in absentia* and co-authored papers from abroad, than in S.F.; but a few who had contributed *in absentia* in S.F attended in person this time: most notably the prestigous "father of radio astronomy," Grote Reber, who journeyed all the way from Tasmania for this and other tasks in the U.S. Reber has of course for

many years been an outspoken opponent of big bang theory.

Prospects now seem favorable for another SWARM meeting in 1996, during ca. 3 to 5 June in Flagstaff, Arizona. Here we hope to allow for even a higher proportion of open discussion than occurred in Norman, by scheduling panel sessions with only short prepared comments.

We also held a much smaller meeting this year with the Pacific Division, in Vancouver, B.C., Canada, on 20-21 June. They allowed us some extra discussion time in the program, but still denied us symposium status. 12 speakers were scheduled, and 9 attended. Grote Reber spoke in person here too; but he was denied a chance to speak in a general noontime lecture slot, which was kept empty instead. He was probably the most distinguished scientist at the entire meeting, but organizers evidently held uppermost in their mind a perceived need to keep our group as obscure as possible, just barely allowing us to be on the schedule at all.

Even further restrictions on our efforts in the Pacific Div. have been announced for their June, 1996 San Jose, CA meeting; so it now seems unlikely we will attempt anything there.

Our group has now been formally organized, and titled "the Natural Philosophy Alliance." In Norman we made plans that have since led to establishing a non-profit corporation. The planned *Proceedings* volume for our S.F. meeting, with a few extra invited papers added, has been much delayed, mainly from lack of funding. It won't appear until 1996, or even later.

We continue the strategy of holding our main meetings along with regular AAAS meetings, which provides extra publicity that reaches both other dissidents and the general public; and we've also held a few small, localized independent meetings. But we have found to our intense displeasure that the same intolerance for ideas like ours—mainly our criticisms of special relativity and big bang theory—that has prevailed at the *national* level for several decades is still being enforced. The sessions we proposed for the Feb. 1996 national AAAS meeting in Baltimore were turned down flat, on the basis of 4 referees' reports that totally dodged our specific arguments, instead ladling out the usual revolting mess of vagueness, irrationality, and contempt, spiced with unjust accusations about our abilities and motives, and even with an uproariously unsupportable claim that physics itself is always open to new ideas [!!!]

The new and younger officials in the AAAS office, whom I talked with in Atlanta during the Feb. 1995 national meeting (where 3 of us presented typically little-noticed poster papers), denied that censorship is being exercised. They gave such rea-

sons as that there didn't seem to be much potential audience for our kind of ideas. Yet I retain my impression that these men are more tolerant than their predecessors, and they do still leave us hints that we might be accepted in some future year. The national meeting will be in Seattle in 1997 and Philadelphia in 1998. As of Sept. 1995 it is uncertain what if anything we will plan for them; but *eventually* being scheduled for a national meeting does remain one of our foremost goals.

I'm sure that as of right now, there would be considerable interest in and attendance at any N.P.A. sessions at a national AAAS meeting; physicists and astronomers themselves are far from the only ones interested in special relativity, big bang theory, or the prospect of a more objective and rational physical science in general. But it would of course help to convince AAAS personnel that this interest exists, if we could generate more publicity for our movement, beyond dissident physics journals like *Apeiron*..

Already such added publicity is beginning to develop. Two journals that deal with all sorts of dissident, progressive science have paid attention to our 1995 meetings: *Frontier Perspectives* (Center for Frontier Sciences, Temple Univ. 003-00, Philadelphia, PA 19122) placed a small notice in their Spring, 1995 issue; and *Explore!* (P.O. Box 1508, Mt. Vernon, WA 98273) sent a reporter, Jeane Manning, to prepare a long article on our Vancouver meeting.

A rather large and influential science journal, *The Scientist* (3600 Market St., Ste. 450, Philadelphia, PA 19104) had a reporter prepare a long article about us for its 15 May 1995 issue. But the article was clearly biased towards mainstream views, and its background tells of still another way in which our ideas can be suppressed: I had spoken to its editor in May, 1994 seeking publicity for our S.F. meeting, when she held a lower post; after she became editor in fall 1994, she asked me to prepare a long article about our movement. But when the publisher saw it, he demanded that I rewrite it and not spend so much time dealing with the intolerance of the establishment. In another version I cut way back on this topic, but he still didn't like the result; and his next ploy was no less than to *change the subtitle of the journal* so that it was said to apply to bioscientists only! But evidently news coverage of our work, of potential interest even to bioscientists (AAAS take note), was still allowed—at least, if it took a

certain slant. We have only recently sent 3 letters to the editor to correct certain false statements in the 15 May article (*e.g.*, that only a few of us are physicists); we aren't yet sure if any of them will be printed.

A few local newspapers have also recently published or promised to consider material about our work, with mixed results. Further information about the Natural Philosophy Alliance can be obtained from me at the address below; I am currently its Provisional Director. Or you may write to our Assistant Prov. Director, Neil Munch, 9400 Five Logs Way, Gaithersburg, MD. 20879. We welcome inquiries, and new members and contributors, from any country.

### Two significant errata relating to the Oct., 1994 article:

(1) Physicist Edward Apgar (on p. 40) joined us in S.F. as a fellow dissident, not as a discussant on behalf of orthodox views. (2) The outline of my theory on p. 41 speaks of "varying net velocity of photons," when actually—a very significant difference indeed—it should say "*unvarying* net velocity..."

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Relativity of simultaneity: Reply to J. Watson

We read with great interest J. Watson's statement that "I think we are in general agreement—certainly over the ROS," and his conclusion that observed simultaneity...certainly is spurious, and to me it is a meaningless concept to consider this apparent simultaneity to have any reality." But we need to add that, as a result, the whole Einstein theory is meaningless, spurious, and loses its place in physics, since it and the relativistic space-time idea are based on this spurious notion of ROS as a conceptual foundation.

To confirm the spuriousness of ROS mentioned by Watson, let us consider the case shown in the figure below, where we suppose that a source of light S_1 is in middle of two clocks C'_1 and C''_1 at rest in an (inertial) frame K_1, and an observer O_2 is at rest in frame K_2 which is moving with respect to K_1 at a speed $\overline{v}$ (to the right), and another observer O_3 at rest in frame K_3 moving at a speed $-\overline{v}$ (to the left).
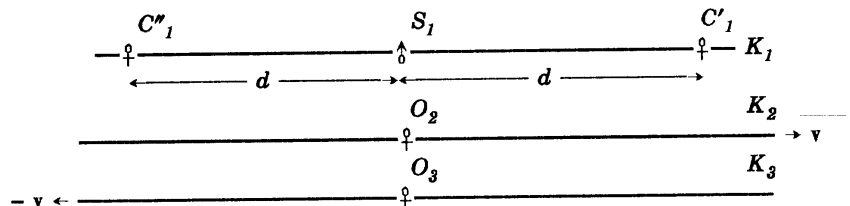
According to Einstein: (a) an observer O_1 at rest in frame K_1 draws the conclusion that two light signals simultaneously emitted from the source S_1 arrive at two clocks C'_1 and C''_1 simultaneously; (b) the observer O_2 in frame K_2 will reach the different conclusion that the same signals arrive earlier at C'_1 than at C''_1; and (c) the third observer O_3 in K_3 will reach the entirely different conclusion different that the same signals arrive later at C'_1 than at C''_1. As they say, it all depends on the point of view!

So, what is wrong with all this? Clearly, it is impossible for either O_2 or O_3 to see the signals unless two mirrors are set beside the clocks; in this case the situation becomes "two-way" one (even if so, Einstein's argument remains invalid, see below), because one can perceive only a light-ray arriving at one's eyes along the line of sight. Thus, this set of "calculations" is based on an absolutely impossible fabrication, so that it falls short of science and is untenable from the start.

By contrast, any physicist who does not indulge in the fantastic stories of Einstein will necessarily come to this conclusion: both observers O_2 and O_3 should have the same judgment as observer O_1 does, since they know clearly that the source and both clocks are all at rest in frame K_1, not on Olympus!

To shed further light on this point, let us make some points clear with regard to the terms $(c - v)$ or $(c + v)$: (a) What do they mean? One possible answer is: a velocity; (b) What velocity is it? It cannot be anything other than a value of the *light velocity*; (c) Which frame is the velocity referred to? Of course, to K_2 or K_3 rather than K_1 in our case, noting that it is meaningless if one speaks of a (relative) speed but does not refer it to a certain frame; (d) What does a velocity mean? Any constant velocity, such as $(c - v)$ or $(c + v)$, *etc.*, is defined as $V = l/t =$ displacement/time, which implies that without a velocity, there would be no displacement.

Unfortunately, Watson states that "the terms $(c - v)$ and $(c + v)$ arise solely from calculation of the effects of changes in location and in themselves do not imply a change in the value of c," a statement which can be found nearly word for word in Einstein's "direct" proof of ROS and/or from the misinterpretation of O. Roemer's (1667) experiment in some works on SRT, *etc.* However, this argument merely demonstrates Einstein's undue imaginative faculty and utter ignorance, because any student knows that displacement and velocity are closely related and cannot be taken separately. Incidentally, this sort of ignorance or school-boy errors involving basic concepts such as inertia, motion, *etc*, can readily be found everywhere in Einstein's works, some of which have been dealt with in our pub-

lished publications and some in papers to be published later.

Accordingly, in the case cited by Watson, $(c - v)$ or $(c + v)$ must be the velocity of light referred to the earth frame; or else, they have no physical meaning at all.

Thus, Einstein's PIVL is killed by Einstein's $(c - v)$ or $(c + v)$!

Concerning the Michelson-Morley (1887) experiment, most physicists regard it as empirical evidence of the PIVL. However, as we have pointed out [*Galilean Electrodynamics*, vol. 3, p.6], the negative result is nothing but a negation of the various "ether" theories (*in vacuo*); once the idea of an ether is abandoned, it naturally becomes such a proof that the light velocity is isotropic and equal to c only with respect to the source frame, not to any other frames. It is worth noting that it has no physical meaning if one speaks of the velocity of light without mentioning the location of its source, because without a source there would be no light emitted from it. Although Einstein claims that the velocity of light has nothing to do with the (speed of) the source, he has never failed to map the position of the source when dealing with specific light problems. For instance, in his argument for the ROS, he prescribed the locations of flashes. The light source must also be dragged down from Einstein's Olympus!

Speaking of the ether, one further point may be added here: is it impossible for light to travel through free space without a medium? No, it need not necessarily have a medium, since light may preserve its initial velocity once it leaves its source, in virtue of the Galilean principle of inertia, which has been distorted and confused by Einstein, and will be dealt with by us later.

What the Michelson-Morley (1887) experiment has verified is: it is futile to try to figure out one's own velocity of uniform motion without the aid of an external message or reference-object, even by means of light signals emitted from the observer frame. This is but another proof of the principle of inertia. The PIVL is as spurious as the ROS, only because they both violate this principle, no more or no less.

Doppler (radial and transverse) experiments are also disproofs of the PIVL, because they refute the equivalence of inertial frames, a foundation on which the PIVL and the entire SRT are based.

In short, the alleged "confirmations" of the PIVL are not true, and, incidentally, they will become reliable disproofs of PIVL instead, when a whole series of major mistakes and confusion in basic physics, astrophysics, and cosmology are clarified or rectified.

Thus, without need of further evidence, PIVL turns out to be the Emperor's New Clothes, in the eyes of any physicist who is aware of the common principle that any

proposition must be rejected without need of quantitative analysis, when it has been disproved in qualitative analysis, and that a theory cannot be valid at second-order approximation, if it has been invalidated at first-order one—except of course those who are willingly misled by the nose by Einstein so as to forget at all the ironclad rule of science that even though a proposition may appear to be ratified by a great number of experiments, it only requires one crucial negative experiment to invalidate it.

We always welcome comments from anyone, for we believe in the Chinese saying that the more truth is debated, the clearer it becomes.

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The "Aarau question" and the de Broglie wave (*Apeiron* 2(2): 33)

The manuscript by Dr. G. Galeczki from the recognized German University of Cologne is rather unusual. It appears to contain no original contributions at all.

Thought experiments are not real but imagined experiments, and therefore prove nothing. Einstein's juvenile question as to what an observer riding with the speed of light on a light wave sees is irrelevant to physics, since it is obvious that such an unphysical concept cannot be realized in a laboratory experiment. Dr. Galeczki takes 5 pages to arrive at the trivial conclusion that this thought experiment is "illusory and untestable."

Then the author turns to an obsolete idea due to de Broglie in connection with the problem of moving clocks. Here, Dr. Galeczki informs us that, by his common sense, "any clock has to be a public clock," being defined as one having a built-in periodic mechanism, a dial, and hands." Apparently, this author has never heard of atomic cesium clocks, which provide our standard of time. Following de Broglie, he defines the frequencies $\boldsymbol{n}_o$ of the "internal periodic mechanism" of particles by means of the equation $h\boldsymbol{n}_o = m_o c^2$. If this relation were physically tenable then the internal frequencies of a macroscopic particle (P) of 1 gr and a hydrogen atom (H) would be in the ratio $\boldsymbol{n}_p/\boldsymbol{n}_H = m_p/m_H \sim 10^{24}$ whence $\boldsymbol{n}_p \sim 10^{46}$ s^{-1}, since $\boldsymbol{n}_H \sim 10^{22}$ s^{-1}.

Frequencies of the order $10^{46}\, s^{-1}$ do not exist, not even for photons in the ultra-gamma-region. How can a competent physicist take such "internal particle frequencies" seriously? It is clearly absurd to associate with a particle a physical frequency which increases proportionally to its mass.

What would be the physical meaning of the "internal frequency" of the planet earth? Hence, it should not be necessary for me to respond to the reviewed extension by M. Magur-Schachter in the paper. Similarly, what is the purpose of the unoriginal rediscussion of the erroneous Weber electrodynamics?

Finally, Dr. Galeczki reviews a "successful particle model" (a torus with surface charge *e* rotating at speed *c*) by D.L. Bergmann and J.P. Wesley which allegedly "implements this "great idea" of de Broglie. These authors show that the magnetic stresses (inwards) balance the electric stresses (outwards) at the torus interface and claim they have found a model for a particle which is contained without artificial, non-electromagnetic stresses. However, they (and Dr. Galeczki) are mistaken since this electron would disintegrate under the influence of the unbalanced centrifugal stresses of the torus rotating at relativistic speeds *c*. Any engineer knows that a rotating wheel must be designed (at infraluminal speeds of rotation!) in such a way that the centrifugal stresses do not surpass the admissible stress of the material. The Bergmann-Wesley electron model, without balanced centrifugal stresses, is science fiction.

After the war, some quite good physical and mathematical papers on particle models were published by H. Honl in Germany. But these publications are not quoted by the author or by Bergmann & Wesley. Einstein, Pauli, Sachs, and de Broglie are recognized physicists, but the other authors quoted are as unknown as the obscure journals they publish in.

There are several banalities in the paper, such as the quotation from P. Beckmann: "The recognizable velocities in electrodynamics are: the velocity of a charge in a magnetic field, ... and the velocity of charges forming a current..." Serious researchers would quote recognized electrodynamics texts such as those of Jackson, Panofsky and Phillips, or Sommerfeld. It is strange that Dr. Galeczki prefers "Einstein Plus Two."

The above paper is probably not publishable in a scientific journal. The ideas discussed in the paper are not original contributions of the author and of little interest today. I do not recall that they had any impact at the time they were first proposed. The paper contains no new physical or experimental results.

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Reply to H.E. Wilhelm

Fundamental physics is in a more difficult situation than at the beginning of this century. Then, new theories were wanted, while today erroneous theories and vested interests have to be given up. No progress is possible if the untenability of "special" relativity is not generally recognized.

The trouble with the "Aarau question" is not that it was (supposedly) asked by a 16 year-old boy, or that it cannot be realized in laboratory. The first problem is that it reveals a peculiar property of *vector fields* obeying Maxwell's equations, namely: If the vectors $\vec{E}(0, f(x-ct),0)$ and $\vec{B}(0,0, f(x-ct))$ constitute a solution of $\vec{E} = 0$ and $\vec{B} = 0$, then the time-independent solutions envisioned by the young Einstein, $\vec{E}(0, f(x^*),0)$ and $\vec{B}(0,0, f(x^*))$ would not satisfy the Maxwell equations. The second problem is that the supposed impossibility of "freezing the light wave" implies a velocity addition law $\oplus$ for which $c \oplus v = c$! Such an addition law is obeyed in one (spatial) dimension by "radar velocities," but these cannot be measured in the microworld. Anyway, whether the "Aarau question" was formulated by a teenager, or by a 26 year-old man, it is not a trivial question and it *is* relevant to physics.

Far from being "an obsolete idea due to de Broglie," the relation $h\mathbf{n}_o = m_o c^2$ which connects the intrinsic mass m_o of a *fundamental particle* and the frequency of an intrinsic periodic phenomenon is a relationship of utmost importance which makes particle *modeling* compulsory. The implication of de de Broglie's formula is that a "simple" particle can be modeled as an imprisoned "energy quantum" $h\mathbf{n}_o$ with an associated inertial mass m_o. Such a particle necessarily has a finite size, in contrast to orthodox quantum mechanics where either the possible structure of the electron is ignored, or it is considered as "point-like." For complex objects, like atoms, molecules, planets, galaxies, *etc.*, no simple relation between mass and some internal frequency exists. This is somehow reminiscent of the absence of macroscopic spontaneous magnetization of materials containing elementary magnetic moments of quantum origin. This is due, apparently, to the non-homogeneous ferromagnetic domain-wall structure, rather than to a vanishing Planck constant—as claimed in "recognized" monographs. The *intrinsic frequency* $\mathbf{n}_o$ of simple particles cannot be directly measured, although it is fixed by m_o and may also be connected with the *external frequency* of the de Broglie wave associated with the particle pc/h (*p* linear momentum)—which *is* measurable. The fact that some frequency is very large is no reason to declare that it cannot exist.

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Instructions to authors

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