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Book Review

Progress in Space-Time Physics 1987. Edited by James Paul Wesley. Benjamin Wesley, 7712 Blumberg, Federal Republic of Germany, 1987, 280 pp., $32.00 (paperback). Contributors: J. P. Wesley, S. Marinov, T. G. Barnes, C. A. Zapffe, W. Krause, A. Seifert, W. R. Jones, P. S. and S. Gulati, G. D. Bouw, H. P. Dart, J. Wilczynski, S. J. Prokhovnik, H. Aspden, F. J. Müller, W. Schmidt, J. A. Guala and L. F. Gatto, E. C. Sherry, O. J. Johnson, J. A. Briscoe, D. J. Savage, P. Graneau, and S. D. Tipnis.

The title of this book is revealing only if one pays attention to detail—namely, the hyphen. It is this that distinguishes anti-relativists, who put it in, from relativists, who leave it out. Be notified by the hyphen, then, that we have here the work of a collection of mavericks who, with a single exception, do not idolize Einstein. The exception is S. J. Prokhovnik, whose vigorous vindication of Einstein (in a paper entitled "The Mathematical and Physical Self-Consistence of Special Relativity") concludes that "Special Relativity arises necessarily out of the assumption of a fundamental reference frame having *only* the property that light propagation is constant and isotropic in respect to this frame alone." With friends like that, special relativity could perhaps use a few enemies ... and this book fills such a need to perfection.

The editor, J. P. Wesley, declines to attempt a concise definition of "space-time physics"—but two themes run through the diverse subject matters treated in his collection. One is the physical nature and detectability of the "fundamental reference frame" alluded to by Prokhovnik. The other is the incompleteness of our current grasp of "classical" electrodynamics, particularly as concerned with effects of induction and with questions about limits of validity of the Lorentz force law. If there is any link between these two themes it is provided by that unkillable zombie of physics, the luminiferous *ether*. In every generation there are reborn people who will not subscribe to the twentieth-century party line, *viz.*, that phenomena describable by mathematical vectors, tensors, etc. are to be physically

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identified with those vectors, etc. ... *i.e.*, that beyond the mathematics of the "field continuum" physics goeth not. Instead, in the tradition of Maxwell, they demand some physical model or mechanism behind the mathematics and additional to it. So it comes about, despite majority insight to the contrary, that a "fundamental reference frame" continues to be viewed by a stubborn minority as the rest frame of a physical "ether." This book gives such people a timely opportunity to present their case.

The case leads off with the announcement of new experimental evidence. According to Einstein's first (relativity) postulate there exists no preferred inertial frame of reference for the formulation of the laws of nature; hence there is no experiment that can be carried out in the closed earthly laboratory (considered an inertial system to good-enough approximation) that will reveal the "absolute" translatory motion of that system with respect to any other. This book reports for the first time several independent sets of observations that purport to contradict such a postulate and to identify a preferred (electromagnetic) system.

Let me review: In the beginning there was Newtonian, then Einsteinian, relativity, as just described. Then Conklin[1] reported anisotropy of the 2.7° K cosmic background radiation that could be removed by transferring the observer to an inertial system F_0 moving relatively to our solar system at 300–400 km/sec in the direction of the constellation Leo. Coincidentally, it seemed that F_0 served also as a preferred system in which the description of the galactic red shift simplified. None of this (I opine) constitutes a violation of the relativity principle, because the distribution of background radiation (likewise of Hubble red shifts, if similarly correlated with cosmic initial conditions) surely qualifies as a "factlike" rather than a "lawlike" attribute of our universe. That is, the relativity principle refers solely and explicitly to "laws of nature," not to factual initial conditions—which (for electromagnetic, as for mechanical, initial conditions) vary with inertial system and are trivially entitled to possess a "preferred system" in which they simplify. Nevertheless, social processes being what they are, there was much stirring-about among etherists and anti-relativists, who—though still utterly ignored by the relativity establishment—began to gird-up their loins in anticipation of *Der Tag*.

Several papers by Stefan Marinov and by Wesley in this book remind us that for years Marinov has been claiming via his "coupled mirrors" and "toothed wheels" experiments to have measured the velocity of the closed laboratory relative to a fundamental system that, forsooth, turned out to be none other than F_0. That is, the occult attraction of Leo for the ether, or its repulsion for our solar system, is alleged to be confirmed by closed-laboratory observations. This permits of no evasion: either the relativity

principle is violated or Marinov is deluded. The delusion hypothesis is not to be ruled out, since Marinov has gone on subsequently to refute the equivalence principle and the energy conservation principle. To his credit as a physicist, he has not been content with theoretical refutations, but has pressed-home his wrongheadedness by means of homemade experimental apparatus, unhampered by the largess with which governments importune those paragons of "normal science" who think right, impress the right people, and resist all temptation to ask nature the wrong questions.

Now, as Wesley reports, Ernest W. Silvertooth has done an entirely independent closed-laboratory experiment, the first with no moving apparatus parts, that again confirms F_0 as the fundamental system. Silvertooth, using an interferometer similar (but not identical) to that shown in Fig. 1 and employing an ingenious special photomultiplier detector,[2] has

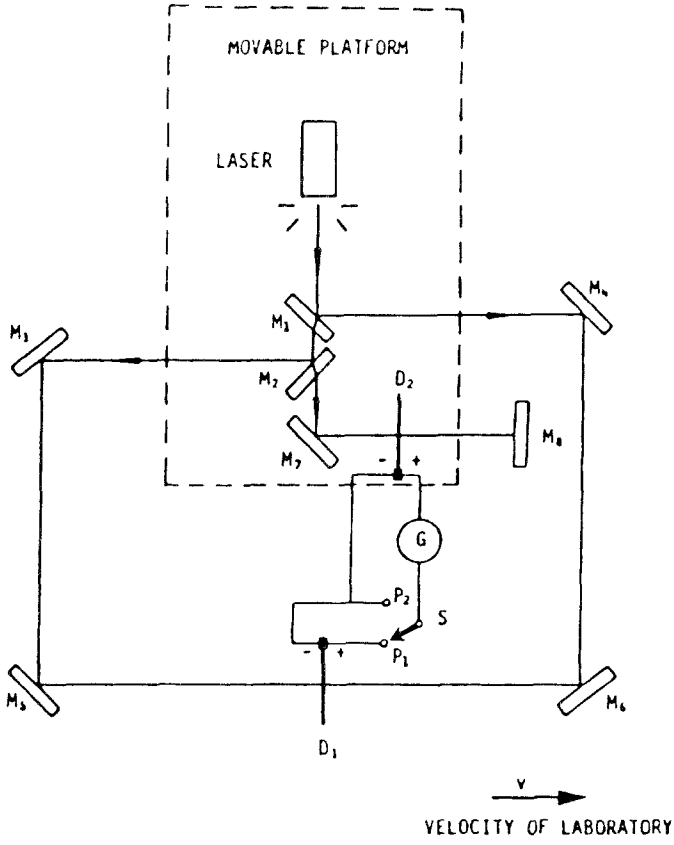

Fig. 1. Marinov's experimental setup to repeat Silvertooth's measurement of the absolute velocity of the solar system. (Fig. 1, page 19, of the book.)

been able to detect and spatially localize an analog of the Wiener[3] fringes (the standing-wave pattern of light first detected in 1890 by photographic plate), produced—as he asserts—not by the traditional two-way mirror (Michelson-Morley) reflection but by the interference of two oppositely-directed one-way traveling laser beams. A shift of the nodal positions of the standing-wave pattern was reportedly observed, varying with sidereal time and correlated once more with the astrological machinations of the constellation Leo. The Silvertooth experiment was "done right" in terms of government (U.S. Air Force) technical support, reportedly had no problems of signal-to-noise, and displayed competent experimental technique. The unwelcome tidings of Marinov are thus resoundingly confirmed.

Another paper by Marinov in this book described his own repetition of a simplified version of the Silvertooth experiment, with the same result: Leo reigns, king of beasts. A footnote to this paper describes a still simpler version of the experiment that could be done with ordinary photodiode detectors. The reported evidence mounts; and suddenly the closed-laboratory measurement of the absolute motion of the solar system appears to become such an easy experiment that it could be done anywhere for a few dollars, perhaps as a high school "science fair" project.

I admit that I accepted all this at face value when I first read of it, and did not know what to think. Was the relativity principle indeed defeated by a first-order experiment? Ours (we are accustomed to think) is ultimately an empirical science, wherein "principles" must bow to "facts." Well, I am happy to report to the physics community that in this instance the "facts" take a beating and the relativity principle seems to survive unscathed ... and sad to report to my fellow relativity critics that *Der Tag* is not yet. Since the subject has some pedagogic interest, let me take a few moments to outline my current view of it. To commence at the philosophical level: Be it noted that there is something anomalous from the start about the alleged ability of any static experiment to cast light upon a science of motion. (This has always been my most fundamental objection to ascribing great significance to the Michelson–Morley experiment).

Let us now examine the specific setup. Silvertooth submitted no material to this volume, so it is necessary to judge his experiment from Wesley's account of it or from Marinov's version, which is sketched in Fig. 1, taken from the book, page 19. Laser light is divided by a beam-splitter M_1 and takes two paths to a detector D_1, a clockwise path via M_1-M_4-M_6 and a counterclockwise path via M_1-M_2-M_3-M_5. The Wiener-type interference fringes are detected at the fixed position of D_1. An ether wind of velocity $\mathbf{v}$ relative to the laboratory is considered to blow parallel to the arm in which D_1 is situated—say, from right to left. On path segments M_1-M_4 and M_5-D_1 there is thus a "headwind" that is

hypothesized to shorten wavelengths of the light by a fractional amount proportional to v/c. Similarly on segments M_2-M_3 and M_6-D_1 there is a "tailwind" that lengthens wavelengths by a similar fractional amount. The experiment consists in displacing (either left or right) by a measurable amount $\triangle$ the "movable platform" with laser and mirrors M_1, M_2, M_7 rigidly attached. With the help of detector D_2 the Michelson-type interferometer involving M_7-M_8 accurately measures, by fringe-counting, the displacement $\triangle$. It is acknowledged (and the fact is exploited) that the latter type of interferometer is unaffected by ether wind because of phase shift cancellation on the folded-back light path.

Supposedly, because the oppositely-directed one-way traveling light beams in the segments M_5-D_1 and M_6-D_1 are differently "windblown," the slightly differing wavelengths in these two segments cause an optical interference phenomenon that may be compared to "beats," such that a value of $\triangle$ can be found that will reverse the phase relationships of the interfering beams. That is, if D_1 and D_2 prior to displacement of the movable platform were each situated at a node of the interference pattern, there exists a least value of $\triangle$ that will produce an antinode at D_1 and a node at D_2. And this value of $\triangle$ is related by a simple formula to the ether wind velocity v, so that measurement of $\triangle$ constitutes measurement of v. That is the idea behind the Silvertooth–Marinov experiment. Now for the catch: It was pointed out to me by Sherwin[4] that in the Marinov geometry shown here any wavelength alterations occurring on the segments M_5-D_1 and M_6-D_1 would be exactly canceled by corresponding alterations (produced by the same ether wind blowing) in segments M_2-M_3 and M_1-M_4. In effect the light path is folded back, and the lateral spreading-apart of the foldings does not alter the basic Michelson–Morley pedigree of the apparatus.

Thus motivated, I made my own analysis according to classical ray-path optics. The problem is formally identical to one in which different indices of refraction (differing from unity by equal plus and minus increments) characterize different segments of the interfering light-beam paths. The optical path length difference determining the interference conditions at D_1 changes when the platform is displaced by amount $\triangle$—and changes in a classically calculable way. It turns out, if we consider only first-order effects, that the resulting calculated phase change at the detector does of course depend on $\triangle$, but not on v (nor on index of refraction in the analog problem). That is, according to my understanding of classical optics, the observable phase shifts produced by changing $\triangle$ cannot vary with v/c. Even if the theoretical claim of Marinov–Silvertooth regarding the effect of ether wind on wavelength is postulated as physical fact, this experiment could not verify such a postulate and remain compatible with the known science of optics.

How, then, did independent observers detect the claimed effects of variation with v? I can only speculate: The reported magnitudes of $\triangle$ amounted to hundreds of optical wavelengths. In principle, what was involved in detecting a hypothetical "beat" was counting, say, 500 fringes on detector D_2 while counting supposedly 499 fringes on D_1. Since the counting in Silvertooth's experiment was "registered electronically" (I gather from Wesley's account) only for the detector D_2 (and observed on a dual-trace oscilloscope for both detectors)—and in Marinov's case was done "manually" for both detectors with an acknowledged counting error of 10 %—it is easy to imagine how track might have been lost.

Some variability of detector outputs would seemingly accompany thermal gradients and time variations, mechanical strains in the platform, etc. Relative phasing of the two detector outputs should be extremely sensitive to such effects—particularly any torquing of the platform by the micrometer that moved it, or other departures from ideally rigid and rectilinear displacement. As for the reported independent observations exhibiting identical correlations with the doings of Leo ... here it is possible that less insight is provided by orthodox history of science than by that remarkable anecdotal history of "pathological science" for which the scientific community is indebted to Langmuir.[5] Let me add that I am personally acquainted with Silvertooth and consider his integrity to be above suspicion.

I refrain from giving my own analysis here because it seems to me that any physicist sufficiently interested to desire an opinion about the Silvertooth–Marinov experiment will wish to earn entitlement to that opinion by studying the experimental description given in Wesley's book and doing for himself the necessary elementary analysis of optical path length differences. I merely remark that the result ought to confirm what was known in the nineteenth century and formalized as "Potier's Principle"[6], to the effect that no first-order influence of ether motion is observable through diffraction, interferometry, or other static (time-independent) optical means. The proof of this principle[7,8] rests on an application of Fermat's principle, and is presumably as firmly founded. The reader must reach his own conclusion; mine is that the Silvertooth-Marinov experiments do not test the hypothesis they were meant to test.

The foregoing observations were premised on a picture of the entire ether as at rest in F_0 and unaffected by the motions of ponderable matter. An alternative view formulated in different ways in this volume by C. A. Zapffe and by T. G. Barnes, expressing an idea that has been around in one form or another since the time of Stokes, is that the ether is dragged along by the earth near its surface (*e.g.*, via the earth's magnetosphere). The positive findings of the Marinov–Silvertooth experiments, if credited, would

disprove all models of this kind. But the above considerations make those reported results appear as incredible on the basis of fixed-ether theory as they are on the basis of Einstein's theory. Therefore possibilities such as those addressed by Zapffe and Barnes appear to remain "in the running."

More recently, it might as well be mentioned for completeness, P. Beckmann[9] has espoused an alternative according to which the "dominant field" (electromagnetic or gravitational) is the reference "object" with respect to which velocities such as that appearing in the Lorentz force law must be referred. (How one gives operational meaning to "state of motion of the field"—as distinct from state of motion of the field detector—escapes me.) Beckmann joins Zapffe in viewing as "crucial" a repetition of the Michelson–Morley experiment in space in a rocket moving around the sun, say, where the relative motion of the dominant field is supposed to create a detectable equivalent of "ether wind." However, all such proposals ignore Potier—thus suggesting that modern technical education, "though plucky and adventury, has scarcely been brought down to the beginning of the century" (to misquote W. S. Gilbert).

Next on the list of goodies in this volume are some modest but significant experiments by F. J. Müller on the "Seat of Unipolar Induction." In Faraday's original experiment a copper disk rotating in a perpendicular magnetic field generated an e.m.f. between center and rim. This simple "homopolar generator" has ramifications of such profundity that Panofsky and Phillips[10] admit to the opinion that only the Powerful Katrika, the Galahad of Physics (whose strength is as the strength of ten because his postulates are pure), the Hammer of Allah—I refer to General Relativity—can crack this nut. As evidence of this profundity, Kennard[11] in 1917 produced a version of Faraday's apparatus involving a rotating cylindrical capacitor in the magnetic field of a solenoid that appeared (in rivalry to the Sagnac effect) suitable to serve as an absolute rotation sensor without moving parts.

Müller, in a set of elegant experiments that would have delighted Faraday, has answered persistent questions as to (1) whether the magnetic field-lines rotate when the magnet that produces them rotates (they do not), (2) whether e.m.f. generation requires relative motion between the rotating conductor and the magnet (it does not), (3) whether Maxwell's flux rule always works (it does not), and (4) whether the e.m.f. is localizable within the external circuit (it is—as Müller shows by an ingeniously simple modification of that circuit). From the results one is free to infer that an absolute rotation sensor could indeed be based on unipolar induction. I should add that this paper, aided in exposition by Wesley's editing, is a pleasure to read. Altogether, a gem of mini-science.

Equally stimulating is the work of Peter Graneau reported here on

"The Pivoted Current Element and Diamagnetism." This investigator's experimentation[12] has done much to reinstate Ampere's original (action-reaction and torque balancing) form of the force law between current elements, as distinguished from the Biot–Savart and Lorentz forms of that law. Here Graneau develops a new model of the metallic current element as an electric dipole, comprising the conduction electron and the lattice ion, pivoted at the lattice site. This dipole, he observes, is "capable of transferring mechanical forces of electromagnetic origin directly to the body of the metal, as required by Ampere's force law."

Employing such a model, Graneau predicts that the Meissner effect should occur in a superconducting ring surrounding a long solenoid (where the vector potential is nonvanishing, though the magnetic field is negligible), and makes a quantitative prediction of the superconducting current to be expected in the ring. Although Graneau's mathematics is clear, I must confess that I did not get an equally clear feeling for the physics behind the proposed model of "absolute diamagnetism": It would appear that a circuit consisting of static electric dipoles (mutually aligned, though not in general aligned with the direction of apparent current flow) can magnetically simulate a current produced by steady transport of charge. Like all significant physical theory, this work leads to experiments that cannot fail to improve our grasp of fundamentals.

Wesley himself contributes several important theoretical papers that appear to clinch the case for Ampere's original force law between metallic current elements. In one of these, 1986 experimental observations of the force on two forms of Ampere's bridge (a bent wire carrying current introduced through mercury contacts) by Moyssides and Pappas[13] are compared with Wesley's exact calculations of what Ampere's law predicts. The "breakthrough" here is Wesley's elimination of infinities that have plagued the traditional calculation (based on the idealization of a zero-diameter filament) by using volume current densities in conductors of finite cross-section. Such an approach to eliminating singularities has been discussed by others,[14] but now for the first time the integrations have been done in closed form for actual experimental configurations.

The results verify Ampere's law, except that the observed forces are consistently 20–30% low compared to theory. It is not hard to guess one source of systematic error in the observations: The surface tension of the mercury in which the bridge floats (seven times that of water) contributes a force of several dynes opposing any motion of the conductor. Thus if F_{obs} is the observed force, theoretically proportional to the square of current I, what should seemingly be compared with theory is not F_{obs}/I^2 but $F_{obs}/(I^2 - I_0^2)$, where I_0 is the (independently measurable) threshold current

for breaking the grip of surface tension on the movable bridge. Such a correction should improve the agreement of theory and experiment.

In the Moyssides–Pappas geometry Wesley shows that the Lorentz (or the Biot–Savart) force law predicts essentially zero force. Hence the contention of Graneau,[12] Wesley, and others that such force laws are observationally disproven for metallic conductors, in favor of the original Ampere law, seems established beyond question. Still, like all good controversies,[14] this one will probably continue. In a companion paper Wesley demonstrated the compatibility of Wilhelm Weber's (velocity-dependent potential) form of electrodynamics with Ampere's original force law. Thus a rival to the Lorentz force law governing actions between charges in vacuum exists and must be taken seriously as physics, because it balances in detail all forms of action-reaction between current elements, whereas the Biot–Savart and Lorentz force laws do not.

One cannot avoid the suspicion that the hitherto unchallenged supremacy of Lorentz's law may be an historical accident largely arising out of its formal property of Lorentz covariance. Since a Hertzian covering theory of Maxwell's electromagnetism has been shown[8] to provide genuine *invariance* (vice covariance) under inertial transformations—so that a truly invariant generalization of Maxwell's theory exists that reproduces all its agreements with observation—it would seem that judgment of the physicality of force laws deserves to be freed entirely from demands for formal covariance. Therefore the contemplation (by physicists, as distinguished from ideologists) of noncovariant force laws, as in papers in this volume by Wesley and by Harold Aspden, seems fully warranted and indeed over-due. Incidentally, in another paper Wesley proposes an entirely different sort of experiment that would be crucial for proving Ampere tension in current-carrying mercury, where Lorentz would deny the existence of such (longitudinal) forces. The ideas of both Wesley and Aspden submit to practical observational tests.

Let me desist at this point from further sampling of the varied contents of this book, beyond mentioning one inclusion and one exclusion. The inclusion is a brief note by a British author, J. A. Briscoe, who calls attention to British Patent Number 884,830, in which he proposed in 1958 a method using radio transmitters (synchronized by an ultrasonic technique) for measuring the "cosmic velocity" of the laboratory. This r.f. version could prove to be one of the easier ways to accomplished the "one-way transmission" objective of the Marinov–Silvertooth experiments. Sherwin[4] has pointed out, however, that a positive result in any such experiments would be hard to reconcile with the claimed 10-meter accuracy of the existing satellite-based global positioning system. One-way transmissions

from satellites at a range of, say, 500 km, if affected in speed at first order by an ether wind of speed $10^{-3}\,c$, would produce variable position errors on the earth's surface of the order of $10^{-3} \times 500$ km $= 500$ meters. And, of course, on the side of theory, Potier's principle remains to be reckoned with at radio wavelengths.

The exclusion, which I find noteworthy, is that none of the dozen or so papers in this collection concerned with the physical nature of an "ether" mentions Dirac's negative-energy electronic form of it. I refer to the "sea" (plenum) of negative-energy electrons logically called into existence by Dirac's highly successful electron theory plus the equally successful Pauli exclusion principle. When theory can only with the greatest ingenuity and deviousness avoid a certain consequence—the existence of a particular space-filling medium having testable properties (vacuum polarization, positron production when $2m_e c^2$ of energy is supplied under conditions permitting momentum conservation, etc.) in complete agreement with observation—I am baffled to find the class of physicists who most earnestly desire that consequence (the existence of a space-filling medium) unanimously dedicated to ignoring the particular medium in question and to postulating innumerable other media *ad hoc* and *ad lib.* The key to this social phenomenon may be that Dirac's plenum appears compatible with the relativity principle. Today's etherists, by contrast, are absolutists practically to a man.

I am still far from reaching the break-over point at which charity would consign to oblivion the residual contents of this book, but space limitation requires drawing the veil of silence at this point. Many of the remaining papers are of the theoretical sort that fail to indicate experiments. One of the exceptions is a paper by D. J. Savage, the title of which must speak for itself: "Measuring Local Time Dilation Using Sandglass Egg Timers."

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