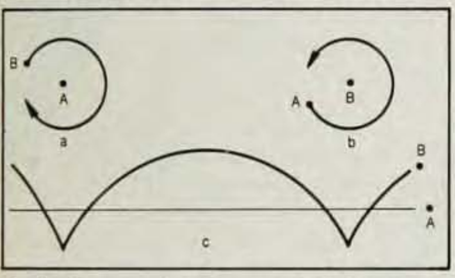
relativity theories are unacceptable to many people is clear from their own papers which have appeared in *Foundations of Physics*, *Spectroscopy Letters*, and *Speculations in Science and Technology*. Basically, the problem is that frame-independent speed is just not very intuitive. Consider a traffic cop speeding down the road at 0.99c in pursuit of a thermal photon wanted for arson. How, one may ask, is it possible for the situation to remain unchanged *vis-à-vis* the chase when the policeman breaks off his pursuit and pulls over to the side of the road? What I should like to do here is show that this question does have an *intuitive* answer within the context of special relativity. The first step is to show that motion in n dimensions does not necessarily exist in $n-1$ dimensions.

Negation of motion through dimensional collapse. Consider the two point-particles A and B in the figures.



In the frame of A [figure a], A is a locus being orbited in a perfect circle by B. In the frame of B [figure b], B is a locus being orbited in a perfect circle by A. One can also define a frame in which both A and B are in motion [figure c]. But can one define a frame in which both A and B are at rest? The first impression one has is that such a frame can not be defined. However, this is not the case. Consider an observer $O(x)$ for which there exists exactly one spatial dimension x. Insofar as $O(x)$ is concerned, the plane needed for the motion between A and B does not exist. Thus A and B merely remain a fixed distance apart, and so are at rest with respect to one another. This illustrates intuitively how motion can be negated by eliminating a spatial dimension.

Now consider an observer $O(y,z)$ who somehow manages to climb aboard a lightbeam propagating along the x-axis. In any theory admitting to Lorentz contraction, $O(y,z)$ will find that the x-axis has collapsed and that the cosmos is all contained in the (y,z) plane. For this reason he will be able to transverse any interval on the x-axis in zero time. (It is not the case, as one sometimes hears, that $O(y,z)$ will observe his own clocks to stop. At least this is not the case in Einstein's theory.) Furthermore, for the reason explained above, $O(y,z)$ will find that the inertial bodies in the cosmos are all at rest with one another in the x direction. Finally, let us extend this to an observer $O(o)$ who manages to propagate in the x, y, and z directions at speed c. For $O(o)$, the cosmos has collapsed to a point—as if at the start of the big bang—and inertial objects are generally all at rest with one another. Since in the frame of $O(o)$ the elements of the set $M = \{\text{inertial bodies}\}$ are all at rest with one another, it should not seem so surprising that $O(o)$ has the same speed with respect to each element of the set M.

Need for more experimentation. Although I have tried to illustrate that modern relativity is not as peculiar as it may appear, I would not want to suggest that one should uncritically accept this or any other theory. We need to keep testing relativity theory experimentally to avoid unscientific overconfidence in our theories. Only in this way does science remain self-correcting, to borrow a phrase from Carl Sagan. In particular, we have no hard evidence at all to support the symmetric aspects of relativity theory. This is because, speaking relativistically, no experiment has ever been conducted in which an observer (or detector) we can communicate with has been outside the rest frame of the Earth. Thus, as I have shown in detail elsewhere,[4] the invariance $c' = c$ might be just an approximation for a strongly nonlinear transformation. As soon as possible, someone should measure the speed of light in freespace from *inside* a spacecraft moving at a relativistic speed with respect to the earth.

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Diehard CP-conservationist

The acceptance of CP violation as a fact has been marked by the Nobel award to Fitch and Cronin, for $K_L{}^0 \to \pi^+\pi^-$, noted in December (page 17), along with the "even simpler" direct preponderance of $K_L{}^0 \to e^+\pi^-\nu$ over $e^-\pi^+\bar{\nu}$, of the Steinberger and Schwartz experiments. Furthermore, we have all recently heard of speculative links of CP violation in K^0 physics, to the apparent excess of baryons over antibaryons—and of e^- over e^+—in the universe, and also to the cosmological photon/baryon

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population ratio; CP violation is proving useful. And yet furthermore, a theoretician who favors qualitative symmetry between electricity and magnetism should feel comfortable with CP violation as the magneto-electric counterpart of MP violation, which is the ordinary Lee-Yang nonconservation of parity, where M reverses magnetic monopoles. Hence to defend CP *conservation* at this point in history may be silly.

Let me nevertheless favor CP conservation, and worry about the experimental evidence. Maybe the K^0 somehow remembers the gross CP asymmetry of the laboratory in which it is manufactured, not in the subtle sense of a CP bias of the vacuum, but in the crude sense that the K^0 comes from a proton-baryon collision, with antibaryons absent. *How* the K^0 may store such a memory is completely unclear to me, but perhaps it may.

The point I wish to make is that this last stand for CP can be refuted by repeating a CP-violation experiment using K^0s either from antiproton–proton collisions or from e^+–e^- collisions. The diehard CP-conservationist prediction would be that both the Fitch–Cronin and the Steinberger–Schwartz asymmetries would be absent in the behavior of such symmetrically derived K^0s.

To impose this further burden on the experiments may however be difficult. If one could order *anything*, one would further specify either a dead vacuum in the long decay zone, or an antimatter atmosphere!

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Improving physics teaching

I would like to comment on Robert G. Fuller's recent editorial (December, page 112) on improving physics teaching. I have directly observed many teachers of many disciplines over a period of many years, as has virtually everyone likely to read this letter. I did so as a student, from grade school through graduate school. It was easy to see that the good teachers were those who carefully organized their course material, developed detailed notes which they reviewed before class, spoke and wrote clearly, anticipated questions, showed enthusiasm for their subject and sensitivity to their students' backgrounds and aptitudes and evaluated students' performances in a challenging but fair manner. What stood between me and a better education was the poor teacher. A teacher was poor, not for lack of a Keller plan or computer-based instructional technology, and not because he hadn't read Piaget. The poor teacher was just sloppy, insensitive and/or disorganized. It seems to me, therefore, that Fuller's editorial misses the main point, that the overwhelming need has always been to eliminate poor teaching, not merely to improve good teaching.

Fuller takes heart that some physics departments are hiring young "physicist-teacher practitioners" as a "first step towards redressing the current imbalance between the rewards for doing research in physics content versus research in physics teaching." This statement disturbs me very much, because it suggests a further development in the old specialty game, whereby familarity with fashionable teaching innovations, gadgetry, and behavioral psychology becomes a prerequisite for certain faculty positions, while most positions continue to be filled purely on the basis of the usual narrow research criteria. Surely, a commitment to better teaching is not demonstrated by hiring a couple of instructional specialists to set up rooms full of computer terminals, hold seminars on teaching technique and churn out articles for the *American Journal of Physics*. These things all have their place, but they are not the solution to poor physics teaching.

Let's ask a simple question: How do physics departments ensure excellence in *research*? Answer: by denying tenure to weaker researchers to make room for the hiring of others who may do better. Similarly, a commitment to improved teaching means making tenure and promotions for *all* faculty more heavily dependent on teaching performance. In turn, that implies a commitment to *measure* teaching performance—not in terms of attendance at workshops (though individual faculty may find them useful), and not necessarily on the basis of innovations, but simply in terms of how much students learn and how well they learn it. The students know darn well who the good and poor teachers are; can't we find out, too? Are we willing to use that information?

As an aside, I would like to suggest that the way for physics departments to get a head start on developing a better overall combination of teaching *and* research is to avoid advertising positions on the basis of narrow specialties and start looking for people with a broader range of interests.

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THE AUTHOR COMMENTS: Allan Walstad's desires to have teaching objectively evaluated and to use such evaluations in the promotion and tenure process are desires with which I can agree. But are poor teachers just sloppy, insensitive and/or disorganized? I doubt it!

Teachers are improved as they understand the learning process better. To that end I recommend the article, "Bike Riding and the Art of Learning," by Robert G. Kraft, (Change **10**, 36, 1978).

There are a number of aspects of the learning of physics that we do not understand. In recent years physicist-educational researchers, such as Robert Karplus (University of California, Berkeley) and John Clement and John Lochhead (University of Massachusetts, Amherst), have helped us know more about the cognitive processes used in physics. We need more such knowledge.

For me, the sure road to improving physics teaching is not more of the carefully organized, enthusiastically spoken lectures. It is rather to have physics teachers committed to having the students able to build useful mental constructs of the world so that the students can not just repeat the old physics, but can formulate new understandings of the world.

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Wave focusing

James Wolfe's presentation of the beautiful work on the focusing of ballistic heat pulses (December, pages 44–50) impels me to remind the readers of similar work in plasma physics, begun well over ten years ago.[1–3] These magneto-plasma waves have been observed in the laboratory[1] and inferred from space observation of "saucers."[2] Although the original theory was intended for satellite antennas in the space magneto-plasma,[1,2] the importance and singular behavior of zero slowness surface curvature was clear. More recent gas plasma work has been published on the details of the interference phenomena near such resonances and on ponderomotive effects[3] associated with the rather high fields in the resonance regions.

It is always instructive to see how themes repeat in different subfields. As usual, the solid-state experiments can be done much more precisely than experiments in gas plasmas. Still, like helicons/whistlers, the phenomenon was looked at in gas plasmas before it became a solid-state experiment.

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