trouble. Here though, unfortunately, Kantor is correct in implying that if x were to start barking physicists would surely set out to show that horses can bark. And they would probably do it with circular reasoning, for example: "It is well known that x is a horse; experiments prove x can bark; ergo, horses can bark, so where's the problem?"

But Kantor is *incorrect* in claiming that there is a reliable experiment that refutes special relativity. Papers that purport to the contrary describe experiments that are usually not even intelligible, much less reproducible. Hence, for now, special relativity is a very useful theory, and that is the point.

On the matter of light speed, Kantor has the concept of frame-independent light speed mixed up with the concept of source-independent light speed. This is evidenced by his having interpreted my computer's remarks on starlight aberration to be a refutation of source-dependent light speed, and by his denigration of the importance of the Michelson–Morley experiment with remarks that begin "Absent an ether..." Kantor has a lot of company in this regard, including many professional physicists, so it is worth reviewing the subject.

Einstein did *not* propose that the speed of light is independent of its source. He didn't have to. This was known all along. Experiments one can do at home on this question are plentiful. Shine some sunlight through a prism and watch it disperse into a rainbow of different colors. Or hold your hand between a lamp and a wall and notice that the shadow is not very sharp, but gets sharper as your hand moves closer to the wall. Or change the channel on your TV set and watch the picture change. This all means that light propagates as a wave, and waves usually have a speed that is source-independent. Thus it was long assumed that the speed of light is source-independent. Today we have a great deal of direct experimental evidence that this is the case, and Lubkin[2] has reviewed those experiments in PHYSICS TODAY. (If Kantor believes experimental refutations should be heeded, one wonders what he makes of all these experiments, which are very straightforward and compelling.)

Of course, waves usually have a field R of propagation such that the speed of the wave depends upon one's speed with respect to R. The Michelson–Morley experiment failed to detect such a field R for light, that is, the luminiferous ether. However, this experiment presupposed that Earth was moving through the ether. It might be that it was merely this assumption that was wrong. Perhaps Earth drags an envelope of ether along with it. But the way starlight aberration works, we know that the light from distant stars travels to us in a straight line, which it would not do if the Earth were dragging an envelope of ether along. Hence the dilemma was, "What happened to the ether?" Einstein's original contribution here was to assume that there just is no ether, that is, no frame R such that one's speed with respect to R affects the speed of light. Einstein did not make this unusual assumption just to account for the Michelson–Morley experiment. Rather, that experiment simply provided him with the freedom to make the assumption. Nor, as Kantor notes, did Einstein make the assumption for purposes of mathematical elegance. Rather, Einstein adopted this hypothesis because, given the already established principle of source-independent light speed, the additional assumption of frame-independent light speed permitted Einstein to resolve many unresolved problems related to electromagnetism, the momentum of light, and the strange mechanical properties of fast-moving electrons. In other words the assumption was *useful*.

To summarize, invariant light speed comes from two different areas of physics:

- Ordinary optics, which states that light speed is source-independent, and
- Relativity, which states that light speed is frame-independent.

Thus, those, like Kantor, who favor a ballistic theory of light are not merely critical of special relativity, but of all the classical physics that preceded it as well. That they usually do not know this is a note added in proof to my contention that foundational theory must be studied by anyone who wants to improve upon it.

References

1. A. D. Allen, *Fire of confusion*, Spec. Sci. Tech. (in press).
2. G. B. Lubkin, PHYSICS TODAY March 1978, page 19.

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Stellarator defended

I would like to rectify a misstatement that appears in the story entitled "Garching shows stellarators may be good after all," (August, page 17). In discussing the stellarators built at Princeton during the 1950's and 60's, the author states that "the stellarators of that generation never exceed a plasma temperature of 200 eV." While this was true for electron temperatures obtained in ohmically-heated plasmas, the statement is not true for ion temperatures obtained by other means.

In a series of experiments exploring the use of ion-cyclotron resonance heating in the Model C stellarator (in 1968), the group with which I worked was able to reach an ion temperature of 600 eV all around the torus, while temperatures twice that high were obtained in local "hot spots." (M. A. Rothman, R. M. Sinclair, I. G. Brown and J. C. Hosea, *The Physics of Fluids*, October, 1969, page 2211.) The 600 eV we obtained in 1968 is not far from the 700 eV now found in the Wendelstein VIIA.

True, our confinement time was about 1 msec, compared with the 5–20 msec currently obtained. But as far as ion heating in concerned, the headline should read "Stellarators were never as bad as people thought."

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Correction

September 1980, page 76—In the "new books" section, the price for each volume of *Growth Points in Nuclear Physics* was stated as $81.00; the correct price is $18.00 per volume.

Travel restriction protest

The 1980 "Rochester Conference" on High Energy Physics was held at Madison, Wisconsin last July. L. Okun of the Soviet Union had been invited to deliver the conference summary talk, but did not come. It is known that at least until shortly before the conference Okun was planning to attend and had all the necessary documents for travel. As of the close of the conference, no official explanation for his absence had been given.

It has been a recurring pattern over many years at these conferences that prominent Soviet physicists have been prevented from attending. Many delegates felt that it was time to make some statement about this practice. The following letter was drawn up, approved, and circulated by an ad hoc committee of about forty delegates, and addressed to Academician A. P. Alexandrov, president of the Soviet Academy of Sciences, USSR.

We, the undersigned delegates to the Twentieth International Conference on High Energy Physics, are deeply disturbed that the key-note speaker, Professor L. Okun of the Soviet Union, was unable to attend. Personal interaction at international conferences is an essential part of scientific cooperation. The fact that