periment studying neutrino oscillations. Great interest surrounds the upcoming proton decay experiments, which take place at zero energy.

Equating elementary-particle physics with high-energy physics is to take a very narrow view of the subject. Indeed, one of the most fascinating aspects of elementary-particle physics is that important research can be performed at all energies and with systems as small as individual particles to those as large as the universe.

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THE AUTHOR COMMENTS: I read with interest the response by Cyrus M. Hoffman of LASL to my article on high-energy accelerators. I deliberately introduced the word "almost" in my sentence, as quoted by Hoffman, that "High-energy physics is *almost* synonymous with elementary-particle physics." I am fully aware of the fact that terms like high-energy physics, medium-energy physics, nuclear-structure physics, and so on have elements of arbitrariness and that the correspondence between elementary-particle physics and high-energy physics can only be a correlation rather than a rigid identity. Obviously, some work carried out at LAMPF, which is formally a medium-energy machine, operating below 1 GeV, is dedicated to very powerful elementary-particle physics, and conversely, some work at the high-energy machines serves nuclear-structure physics. Nevertheless, as a direct consequence of the uncertainty principle, the power of accelerators as tools for elementary-particle physics increases with energy. One can, of course, frequently substitute accuracy for energy. The example cited by Hoffman on the demonstration of parity violation in weak neutral-current interactions is an excellent case in point. Indeed, low-energy experiments have been carried out during the past several years aiming at investigating weak neutral-current effects in atomic transitions. They require a sensitivity of 10^9, and the results are still ambiguous, both due to the great experimental difficulties and some of the problems of isolating atomic physics effects directly from the problem under investigation. Conversely, the beautiful experiments of Charles Prescott, Richard E. Taylor and collaborators, described in PHYSICS TODAY in September 1978 (page 17) investigated the same interaction using a polarized high-energy (near 20 GeV) electron beam. At this high energy the sensitivity required is "only" 10^{-5} and interpretation is totally unambiguous.

Hoffman also refers to interactions involving particles at rest, such as proton-decay experiments, which are of obvious high relevance to elementary-particle physics (incidentally, the US experiments on this topic are supported under the High Energy Physics Programs of the Government). Indeed this is true, yet the incentive for the worldwide search for proton decay now in progress comes from the hope for the unification of theories of electromagnetic, weak and strong interactions which, in turn, have largely been nourished by recent results in high-energy physics.

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Experimental relativity

Rather than ignore challenges to the special theory of relativity, Allen D. Allen (May, page 86) has as a "matter of conscience" and "sheer tenacity" claimed to have explained the theory to those whose challenges are based on a misunderstanding of the theory. He presented a response scheme that uses a computer to provide automated answers.

Allen noted that confusion in the Special Theory "is almost always centered on one or more of a handful of underlying physical concepts," such as "covariance." In 1917, E. Kretschmann[1] showed, with Einstein's concurrence,[2] that covariance is a purely mathematical concept without any necessary physical consequences. Argument, heated or dispassionate, on theory—any theory—can, given sufficient ingenuity, be made to be endless. What, then, would it take to *compellingly* convince a reasonable physicist that a given theory is or is not correct? Unhesitatingly, it is its experimental confirmation or contradiction.

The experimental evidence published in support of the Special Theory has been uncritically accepted as valid, when, in shocking contradiction to consensus, none of it *unambiguously* confirms the theory. There are, further, several experiments that unambiguously contradict the theory.

As confirmation, Allen notes the particular time dilation work involving muons. S. J. Prokovnik[3] suggests that this evidence must be considered inconclusive, the nature of the time dilation being assumed in the interpretation of the data. H. Arzliès,[4] a firm proponent of the Special Theory, concedes that the experiments are indeed ambiguous, that they are "not very exact," and that "fresh experiment is necessary." In the meson experiments, the speed of the muons was *not* definitively measured independently. The speed was

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inferred, based on the relativistic expression for energy. The assumption of one Special-Theory effect (energy) to infer another (time dilation) only circularly begs the question; no convincing productive experimental conclusion is provided.

Allen cites the Michelson–Morley interferometer experiment, designed to detect motion through an assumed ether, as evidence that the speed of light from a moving source can *not* be $c' = c \pm v$. We have all had it drummed into us, as an awesome piety not to be questioned, that this experiment, in which the light source is *at rest* relative to the interferometer, is crucial; it is not! Absent an ether, it could give nothing but the null result observed, having nothing whatever to do with an effect to be expected from a relatively moving source.

Allen asserts that the observation of starlight aberration contradicts a ballistic, $c \pm v$, light propagation; it does not! To begin with, passive observation is not a controlled laboratory experiment; the relative motion of the source is beyond our influence. The first-order (v/c) aberration effect is not at all discriminatory between an etherless ballistic effect or an absolute propagation effect. An elementary algebraic deduction leads to the same first-order effect in each case, much like leading a moving target with a rifle.

The only first-order effect experiment that is discriminatory deals with the convection of light propagating in a moving medium—the so-called dragging effect. The unsuspected results,[5,6] by Macek et al, on the skew convection of an infra-red laser beam in flowing carbon-tetrachloride flatly contradicts, by a factor of 3:1, the convection effect expected from *both* the ether theory and the Einstein absolute propagation hypothesis. Prior experiments by Fizeau, Michelson, Zeeman, and others were woefully inaccurate (See reference 6, page 54.)

The Mössbauer-effect experiments, in which the emitter and the absorber were rigidly mounted at separate positions on a rotating disk, were, oddly enough, interpreted as the result of a second-order Doppler effect. The Doppler effect requires relative motion between the source and the receiver; in these experiments there was no such motion. Further, the emission and absorption were broadly uncollimated, so that first-order effects—had there been a Doppler effect to begin with—would have exceeded any second-order Doppler effects by a factor of about 10^5 (see reference 6, page 138). The actual effect observed, to an accuracy of about $\pm 2\%$, definitively contradicts the absolute propagation hypothesis by clearly confirming the usual additive *etherless* ballistic propagation idea (see reference 6, pages 137–142).

A letter communication precludes a detailed critique of all the experimental foundations of the Special Theory of Relativity. The essential point presented here is that an experimental challenge to the theory should not be allowed to go unanswered by a conspiracy of silence or by automated replies from a mindless machine, not programmed for critical answers to questions of experiment. Avoidance of the challenge merely delays the inevitable in the progress of the *experimental* science of physics. The overwhelming majority of physicists certainly maintain the clearest opinion on the merit of the Special Theory as a theory; at the same time they have been led to an *uncritical* acceptance of its experimental basis. Even the most resolute believers will recognize the obligatory need for the clarification to be gained by an open literature discourse of the matter; clarification is something to be welcomed rather than shunned, as with past wrangles on theory.

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6/24/80

THE AUTHOR COMMENTS: Wallace Kantor seems to be under the impression that our little automated relativity program is only meant to silence those who are critical of special relativity. This is far from the truth. The purpose of our program is to explain relativity, and/or related physical phenomena, to anyone who does not fully understand the subject and wants to (or needs to) learn more about it. The class of people who do not fully understand the subject includes a large number of people who are convinced of the validity of special relativity, not just those who would challenge that theory. As an example of the former, I recently found six out of six referees provided by three major journals, none of whom knew how to derive Lorentz time dilation from the postulate of invariant light speed. Indeed, they denied it could be done. When I finally convinced one that such a derivation is possible he replied that it was "just a mathematical coincidence and has nothing to do with relativity theory."

Furthermore, I agree with the implication in Kantor's remarks that physics is suffering from a certain amount of dogmatism that proscribes even a valid criticism of accepted theory, and such a state of affairs invariably impedes progress in any field of inquiry. However, *misinformed* criticisms only make this problem worse, as I have discussed at length elsewhere.[1] And the fact is, the majority of those who are critical of special relativity have little or no training in theoretical physics. While the reader of these pages has recently been made aware of the contributions amateur scientists can make, it is unrealistic—perhaps even delusional—to suppose that one can make a remarkable contribution to foundational physics without having studied the subject in any depth. Yet a significant number of people pursue that impossible dream when it comes to special relativity, probably for reasons that include the simplicity of the mathematics involved, and the deification of Einstein. Thus what makes Kantor's letter important is that he represents a peer group that is relevant for the psycho-sociology of physics. Ultimately it is physics education, or its inaccessibility, that is at fault. With my promised tenacity, I will therefore attempt to respond to some of Kantor's technical comments.

First of all, it is quite true that there is no experimental evidence that unambiguously confirms the special theory of relativity. There can be no experimental evidence that unambiguously confirms any theory, which is why they are called "theories." This has to do with the properties of logical implication ("If A, then B," or in formal terms, "Not A, or B") and can be intuitively explained with the following parable:

> Experiments are conducted demonstrating that an object x (thought to be a tree) has four legs and a tail. After some thought, Dr Einstein proposes the theory that x is a horse rather than a tree. At first, no one takes Einstein seriously. However, when it is experimentally determined that x has a mammalian metabolism, Einstein's theory begins to win acceptance. By the time experimentalists find that x has two pointed ears, no one is surprised, and Einstein's theory has become universally accepted.

Now, all these data on x do not *confirm* Einstein's horse theory. Given those same data, x might be a dog. In general, we can never confirm a theory; so a theory is at best *useful*, rather than "true" (which, again, is why we call them "theories"). However, a theory *can* be refuted. If x should start to bark, the horse theory would be in

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.

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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