letters

THE AUTHOR COMMENTS: The first two paragraphs of Robert Hermann's letter are interesting in that they trace the mathematical concept of fiber bundles to Cartan's work, which in turn was influenced by the developments in physics before and during the 1920's. The last paragraph is also interesting since it reveals Hermann's resentment (which is shared by some mathematicians, but not all) that physicists are not listening to the mathematicians enough. In my opinion there is some truth in this complaint, but the reason for it is not that physicists are fatalistic or smug, but that our two disciplines fundamentally have different value judgments, as I emphasized in the last paragraph of my article.

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Unaffiliated physicists

The letter by Lance Kethley in May (page 82) calls attention to the publication plight of the unaffiliated physicists. In the last ten years I have been, off and on, in the same situation as the author. While my experiences agree *grosso modo* with his observations I do believe that some APS editors deserve a friendlier word.

I have published several times in APS journals using my private address. No page charges were paid, and I don't believe my publications were unduly delayed.

Such accomodating attitudes may not in general be expected from APS publications that fall into the so-called "prestige" category. Since prestige is, in so many ways, synonymous with funding, one may understand that the chances for a "have-not," of getting a word in edgewise, are indeed slim.

The reviewers used by prestige journals frequently tend to be intimidating rather than to the point and businesslike. One may have some doubt whether some editors read the reviews they send off to their authors; because why forward comments that abuse the privilege of anonymity with irrelevances or even insults? To give the editors the benefit of the doubt, shall we assume that such oversights have occurred at the secretarial level?

When suffering indignities it is good to remember that APS membership is neither mandatory nor a license for publication privileges. There are European and even US journals of good standing that do not require page charges. Keep in mind though: European editors are apprehensive of accomodating an increasing number of American physicists, who are refugees from the page-charge plague. In case of a European refusal, remember: page charges are ironically (and as usual with best intentions) funded by our own Federal government.

Furthermore, a good-sounding affiliation also helps with European editors. One editor quietly changed my private address for a university affiliation I had in the past.

Indeed, the not always rational criterion, called prestige, rules well in excess of its limited intrinsic potential. Prestige can push the income of some into the six-digit bracket, and the lack of it can sometimes be translated into: no income at all. Drawing a 19th-century parallel, one wonders whether the pay scales of Faraday and Maxwell exceeded the pay scales of their less renowned colleagues.

Just for the sake of inviting a Proxmire citation, I have, at times, been tempted to add a note to my papers: "Work supported by the US Social Security Administration." So far I have felt that, in doing so, I might overextend my welcome with our American editors, many of whom have a hard enough job as it is.

Indeed, I concur with Kethley. There is room for more human rights here at home, but let it be human rights with spirit and sincerity rather than human rights solely by rule and legislation, or worse: solely for publicity.

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High-energy physics?

I read with great interest the article by W. K. H. Panofsky on future high-energy accelerators. I must take exception, however, with his statement that "high-energy physics, . . . is almost synonymous with elementary-particle physics." "High-energy physics" is a poor term requiring periodic redefinition. This is reminiscent of 1980 physics texts entitled "Modern Physics." "High-energy physics" describes research being performed at "high-energy" accelerators, although presumably it would not include biomedical or synchrotron-radiation work being performed at such machines. The distinction based on energy is somewhat blurred: Is research at the 6-GeV KEK accelerator high-energy physics? What about LAMPF (800 MeV)?

Much work of paramount importance to elementary-particle physics has not taken place at high energies. In the past several years there have been important low-energy experiments performed on parity violation in weak neutral current atomic transitions and muon number violation. Interest has recently been stirred by a reactor ex-

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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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