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tists outside China (Fang Li Zhi, for example) who call for boycotts; however, I think the desires of scientists who work in China rather than those outside should be the basis for action. After all, it is the careers of those in China that will suffer from lack of scientific exchange. Most Chinese scientists remain in China to work not because they support Chinese government policies but because they believe strongly that only by hard work from within, by helping to educate the next generation of scientists and by promoting science and scientific ideas in China will they be able to change China's economic and political conditions.

This brings me to the second reason: China is a third world country; it lacks the strong scientific foundation and expertise the Soviet Union enjoyed. Scientists working inside China have long faced the double burdens of primitive working conditions (for example, old equipment and insufficient subscription to scientific journals, which the Soviet scientists only began to experience amid the chaos of reform) and a restrictive political environment. International conferences held in China are therefore especially valuable opportunities to scientists there. It seems ironic to me that after the effort made by APS through its China program to help Chinese scientists recover from the isolation and political turmoil they suffered during the Cultural Revolution,¹ Western scientists now seek to express their dismay with the Chinese government by isolating Chinese scientists again.

For these two reasons, I do not think a difficult moral choice exists over whether to participate in conferences in China (contrary to the position of Herman Winick, chair of APS's committee on the international freedom of scientists, as described in the May news story). Western scientists go to conferences as individuals, and they can help individual Chinese scientists by providing them with current information. Should the Chinese government try to make political hay out of a scientific conference, the Western scientists in attendance can publicly denounce such maneuvering. They also can choose to send letters of concern to the authorities regarding political prisoners and detained scientific colleagues once they are there. (I think their presence in China adds weight to their concerns.)

Open and fair exchange is the only way Western scientists will get to know the conditions in China. As

scientists we should strive to get as much information as possible before making a judgment, and face-to-face exchange is one of the better ways for doing so. It is important not to limit ourselves to the few voices we can hear and act on them alone.

Reference

1. *APS News*, May 1992, p. 19.

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5/92

Democracy Demands Science Education

I read with great interest the Reference Frame column by Leon Lederman in the May 1992 issue (page 9). I too am a strong advocate of informed participation by scientists in pre-college education. However, I am concerned about Lederman's statement that if we do not get involved, "we won't have the new recruits we need to keep our subjects going." I do not believe that there is a shortage of physicists. In fact, judging by the lack of permanent positions, there appears to be an overabundance of physicists. Therefore it seems unreasonable to tie involvement in pre-college education to a desire to produce more physicists.

There are far more compelling reasons why physicists should concern themselves with pre-college science education. In an era when public policy will be formed more and more around scientific issues, it is critical that citizens be educated in science and that they acquire science process skills. The abilities to observe and measure, think quantitatively and come to a logical conclusion based on the available evidence are a set of skills that everyone needs to acquire. If our people as a whole do not possess these skills, then our republican institutions will inevitably reflect that deficiency. Therefore it is crucial that science education undergo systemic reform nationwide and that new pedagogy, informed by the most up-to-date understanding of cognitive development, be at the center of that reform. (See the September 1991 special issue of *PHYSICS TODAY* on pre-college education.)

Science education must focus on teaching science process, instead of the all-too-common presentation of science as a bunch of facts to be memorized. This approach, by neces-

sity, requires a detailed examination of phenomena through experimentation, and a consequent reduction in the areas covered. One should dig a few deep wells rather than scrape the surface of everything. Moreover, science should be a major focus of pre-college education, with other subjects, such as language arts and history, integrated directly into it. An in-depth, hands-on investigation into the life cycle of fast-growing plants opens up discussions of agriculture, economics and history in a way that no textbook can.

For such reforms to be effective and sustained, we as a community must be informed and involved. We should provide in a collegial fashion the technical support that school systems need. We should act as advocates for effective science education and systemic change. We also should point out, in the "content vs process" debate, that with process comes content and that learning to think is the ultimate goal. And while we recognize that our self-interests will also be served if general interest in science increases, we must always keep in mind the central goal of bringing science as a living subject into the lives of all children, for the greater good of society as a whole.

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8/92

Where Pauli Made His 'Wrong' Remark

In answer to the query raised by Leonard X. Finegold (September, page 103): Wolfgang Pauli's remark "Das ist nicht einmal falsch" ("That is not even wrong") was made not as a comment on a seminar talk but as a reaction to a paper by a young theoretician, on which a colleague (I believe it was Sam Goudsmit) had invited Pauli's opinion.

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10/92

CPT Violation Error Reversed Just in Time

In my review of Michael C. Mackey's book *Time's Arrow: The Origins of Thermodynamic Behavior* (September, page 72), I referred to "Val Fitch and James Cronin's Nobel Prize-winning experiment on CPT violation in K^0 meson decays." I meant to say, of course, that the experiment measured CP violation and hence implied

that time reversal also must be violated if the product *CPT* is to remain invariant.

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9/92

More \$ for Equipment, Less for Grad Students

I read and very much agreed with Stephen Sacks's letter (December 1991, page 103). It seems to me that university-based physics research is moving in a very unhealthy direction, because to advance, each professor has to produce additional PhDs—PhDs who will ultimately be clamoring for research support of their own. As Sacks points out, this is a very unstable situation.

I believe that the resources to fund university-based physics research should be allocated in a very different way. The amount for graduate student education should be cut, and the money saved should be spent for capital equipment as well as for the hiring of permanent technical, engineering and scientific staff at the university. Such a reallocation of resources might be accomplished by accrediting agencies and funding agencies such as NSF, as well as by a university itself.

I have heard many times about surveys predicting an imminent shortage of PhD physicists. However, in my 25 years as a scientist, I have never really seen this shortage materialize. In the medical profession, the highest obstacle is at the entrance to medical school. I strongly believe that if physicists were more like doctors in this respect, the profession, the individual physicists and the country would all benefit.

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1/92

A Brief for Brevity

I have been following the debate in *PHYSICS TODAY* about replacing traditional archival journals with electronic publishing and thereby avoiding involvement of referees and editors. Such a scheme would, I think, aggravate an already bad situation and lead very quickly to the death of useful scientific literature. All too often papers submitted for publication today have not had even proper

proofreading. And the great majority of papers are now read and referenced by very few scientists besides the authors themselves. Who will have the courage to scan through all the mediocre (or worse) entries that would accumulate in an electronic system, in a vain attempt to locate the few gems?

Recently, however, I have been shaken from my own complacency by demands from some journals to reduce the size of all papers. As an author, I am confronted with higher charges for papers longer than an arbitrarily set length. As a referee, I am asked to advise other authors on how they could shorten their papers. Such requests are not at all easy to fulfill. It seems to me that we are experiencing a critical phenomenon soon to be followed by a phase transition. The question is what the new phase should be. (Electronic publishing? Not!)

One possible answer I have not seen seriously discussed is organizing a major change in the style of the papers themselves. My research has once again led me into a subject new to me, so I have recently spent some time reading original references from the physics literature of the 1930s, '40s and early '50s. I could not help but notice a startling difference between many of these older (and often famous) papers and virtually all the modern papers I see: They were short—only one or two pages! They go directly to the subject and the new results, without introductory or concluding discussion. This style is contrary to the one I learned in school: "Tell them what you are going to tell them. Tell them. Tell them what you told them." While I still think highly of the introduction-body-conclusion style, in general we can no longer afford to use it for all our papers. Perhaps if we worked harder at saying what we have to say well once, the other two times might not be necessary.

Why do we use this long-winded style when our forebears were able to present their results in a more concise format? Part of the problem arises, no doubt, from the far greater number of scientists working today and the diversity of their background and training. Also, the interdisciplinary nature of much modern research constrains authors to avoid the use of specialist jargon, whose purpose is to abbreviate discussion of commonly understood topics. Authors writing papers for journals like *Physical Review Letters* are forced to deal with the same constraint by the requirement that all papers appeal to

a general (nonspecialist) audience. Thus both the subjects written about and, in some cases, the journals force papers to be longer.

My recommendation? Clarity without redundancy—or at least without so much redundancy. If an author is going to write just one paper on a given topic, then he or she should be allowed to write the clearest possible paper. That probably means using the introduction-body-conclusion style. However, if the author foresees a series of papers on the same subject, she or he should dispense with the "self-contained" criterion in most of the papers. I think this modest proposal could easily reduce the number of journal pages by a factor of two, and possibly more if everyone played by the same rules.

I am willing to try it if you are.

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Whitehead's Science-Religion Reference

Art Hobson (April 1992, page 120) asks for help in locating the exact reference for his quotation from Alfred North Whitehead, "When we consider what religion is for mankind, and what science is, it is no exaggeration to say that the future course of history depends upon the decision of this generation as to the relations between them." I suggest that Hobson look, for example, at Whitehead's *Science and the Modern World* (Mentor Books, 1954, page 180).

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Another Boltzmann Trip Translation

I read with interest the article "A German Professor's Trip to El Dorado," by Ludwig Boltzmann, abridged and translated by Bertram Schwarzschild (January 1992, page 44). I would like to point out that this article by Boltzmann was previously translated by Margaret Malt and appeared in *Annals of Nuclear Energy* 4, 145 (1977).

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