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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!

ELIHU LUBKIN
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2/81 *Milwaukee, Wisconsin*

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.

ALLAN WALSTAD
University of Pittsburgh
1/19/81 *Johnstown, Pennsylvania*

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.

ROBERT G. FULLER
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3/81 *Lincoln, Nebraska*

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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3/81

Science and nationalism

Thank you for the enlightening February editorial, "Role of Physicists in the 1980s." It contains such logical gems as, "there is hardly anything lasers cannot do" and "good science cannot be directed," but, "while working on one important phase of survival, missile defense, we shall of necessity be interested in space technology." I had not realized that my *raison d'etre* as a physicist is to ensure the military superiority of one political system over another. Has not the experience of the atomic scientists taught us anything about blindly mixing science and nationalism? Perhaps with reception of the doctorate, we should each receive a ring through our nose. I pray that the role of the physicist in the 1980s is not that of Dr. Strangelove.

THOMAS H. CHYBA
Ann Arbor, Michigan
3/81

Editorial bias

I usually read PHYSICS TODAY with a great deal of pleasure. It is generally a well-constructed and comprehensible source of information on a broad spectrum of topics. However, I found your editorial in March peculiarly disturbing. You seem to assume that the "prestige" physics is in academia, and that industrial research is merely "salable." Well, I suppose your prejudices are your own affair. However, I really must object strongly to the sentence, "If he is really doing physics, the young PhD, while striving to produce a more salable product, will also hope to learn something new that maybe he can publish a paper about." How patronizing!

I am, I suppose, a young PhD, trained in theoretical physics. I am currently involved in research into techniques of oil exploration. It is an exciting, challenging field; moreover, discoveries can have a significant impact on our society. My joy is in seeing our work *used* in a practical way. Publishing papers, which I found gratifying as a particle physicist, is of distinctly secondary interest to me now.

Yes, I think there is a valid distinction to be made between basic and applied research. Like most classifications, it probably cannot usefully be exactly defined—there is presumably a continuum of activities. But if you avoid the difference, you risk denying the validity of motivations different from those commonly found in universities.

Why, you may ask, have I written this letter? It is because I would like to ask you to examine your own biases in the light of what your words may influence others to do. I fear that casually patronizing remarks in reputable journals may influence young physicists to avoid satisfying, rewarding careers in industry.

R. T. CUTLER
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4/81

Science for peace

The following notice was posted on a prominent notice board at the recent APS March meeting in Phoenix, Arizona:

Objectives

1. To raise consciousness, especially among Canadian scientists and educators, of the clear and present danger of war waged with weapons of mass destruction, especially nuclear weapons;
2. To make specific proposals and take positive action towards reducing the danger of war;
3. To encourage scientific activities directed towards peace, and to urge the publication and dissemination of the findings of peace research;
4. To encourage educational activities directed towards peace;
5. To interact with expert groups, such as Canadian Pugwash;
6. To encourage mass movements, such as Project Ploughshares, and assist them by providing technical advice.

Membership of Science for Peace is normally limited to Canadian residents. But we welcome exchange of information with sympathetic individuals or members of organizations outside Canada. Please contact at this meeting or write to: Eric Fawcett, President of Science for Peace (address as below).

The *total* absence of any response either at the meeting or subsequently forces me to conclude that, despite the gallant efforts of members of Forum on Physics and Society and of Panel on Public Affairs, the great majority of my colleagues in APS are so preoccupied with "advancing and diffusing knowledge of physics" (in the words of the APS constitution) as to ignore their larger responsibilities to society.

Perhaps I can shock some of them into action by pointing out that in the event of an all-out war waged with weapons of mass destruction one of the lesser casualties may well be physics itself. After all, the few of us who survive will be too much preoccupied with the struggle for existence to be able to devote energy to the pursuit of our beloved discipline.

ERIC FAWCETT
University of Toronto
Toronto, Canada
4/81

Origin of σ

The present common notation for a cross section is σ, but when was this notation introduced, by whom, and why? The questions have piqued my curiosity. The earliest use that I have been able to locate is by Rober Oppenheimer in December 1927. In the article entitled "On the quantum theory of the capture of electrons" [Phys. Rev. **31**, 349 (1928)], he denotes "the first order cross section" by σ_0 and a mean cross section by σ. There is, however, no suggestion that a new notation is being introduced.

Oppenheimer had just returned from his graduate work at Göttingen. Could σ have been standard German notation? Not likely. Volume 24, 1 of the *Handbuch der Physik*, published in 1933, was devoted to quantum theory. In it Gregor Wentzel used dQ for "Wirkungsquerschnitt" and Hans Bethe used $d\Phi$ for a cross section. No use of σ as a cross section is to be found in this prestigious and influential presentation of quantum theory.

I had thought that the notation might have arisen already in the latter half of the 19th century, among the founders of kinetic theory and statistical mechanics. The symbol σ is indeed heavily used in kinetic theory, but it denotes the diameter of an atom or hard sphere.

If anyone has an earlier or better reference—or a suggestion for where to look—I would appreciate hearing from him.

RALPH BAIERLEIN
University of California, Santa Cruz
4/81

More on junk mail

I am writing in response to the letter by Henry Blosser (April, page 74) concerning junk mail. Blosser advocates sending back business reply envelopes to force advertisers to pay postage to find out that the sender is displeased with junk mail. While this may create a momentary sense of euphoria, it probably will have little long-term effect. My main concern is that junk mail doesn't pay its fair share of the cost of running the Postal Service. Some of it (admittedly a very small percentage) even has some value. To change anything about the way in which junk mail