

LETTERS (continued from page 15)

both volumes provide an otherwise excellent exposition of the history of renormalization and the role in it played by Tomonaga. Consequently, we would like to outline the true sequence of events in this matter, which appear to remain not well known outside of Japan. One of us (Ito) was a collaborator of Tomonaga's during the postwar period, and the other (Nishijima) has recently reviewed Cao's book for the Physical Society of Japan's journal *Butsuri*.³

According to Cao, Tomonaga was a firm believer in Shoichi Sakata's hypothetical C-meson field, which was proposed for the purpose of canceling the "mass-type" divergence in QED, but he "obtained a proper understanding of renormalization only after the publication of the works by Bethe and Lewis, and after his gradual abandonment of the naive idea of the compensative (C-meson) field" (page 199). This statement is not accurate.

In fact, Tomonaga had doubts about Sakata's idea from the beginning,⁴ and to check it he started to examine the divergences in electron scattering by introducing the C-meson field. Concurrently, he was also pursuing the possibility of controlling all the divergences in QED with the renormalization program based on the covariant formulation of field theory that he had developed over the years. Indeed, the general idea of the renormalization program was presented at a symposium⁵ held in Kyoto in November 1947, and a full account of it was published the following year.⁶ The Kyoto symposium occurred a month prior to Tomonaga's own discovery that the C-meson does not lead to any new type of divergence characteristic of electron scattering.⁷ Ironically, this finding brought him to the realization that the divergence in scattering processes can also be eliminated by means of the mass renormalization without recourse to the C-meson, and he was convinced of the legitimacy of the idea of renormalization in QED. Unfortunately, he did not make this conviction public in his early publications because of his reluctance to present anything less than fully justified claims. Presumably, it was Tomonaga's reluctance that led Cao to make the mistake discussed above.

It should be stressed, however, that Tomonaga's renormalization program was a product of deep contemplation over a long period of time—namely, during and immediately after World War II, when the Japanese

physics community was cut off from the rest of the world. The Lamb shift caught Tomonaga's attention when it was reported in the science column of *Newsweek* on 29 September 1947. However, he had no access to the full account of Hans Bethe's work or to H. W. Lewis's paper until later.

During the war, Tomonaga worked on the damping theory, the covariant formulation of quantum field theory (that is, the super many-time formalism), the strong and intermediate coupling theories for the meson-nucleon interactions and also the general theory of ultrashortwave circuits. All of these developments were first reported in Japanese during the war and then much later in English. Following Tomonaga's wartime recognition of the importance of higher-order corrections in field theory, it was quite natural for him to be attracted by the problem of ultraviolet divergences in QED bound for the idea of renormalization first in mass and then in charge (the former was easier to grasp intuitively since it is subtractive, whereas the latter was more involved as a concept since it is multiplicative). The news of the Lamb shift and Bethe's work after the war only prompted Tomonaga to bring his idea to perfection.

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Physics Grad Students Should Figure Odds of Getting Faculty Slots

We physicists pride ourselves on being able to do back-of-the-envelope, order-of-magnitude type cal-

culations to get a quick handle on a wide variety of situations. Acquiring this ability is part of any good physics education. To test for this ability, I suggest that the following three-part problem be put to graduate students in their qualifiers.

There are approximately 8500 faculty positions in the US institutions that grant doctoral, master's and bachelor's degrees in physics—including soft-money positions (but not postdocs).¹

First, estimate the average employment lifetime of a permanent physics faculty member. Second, assuming a steady state (all retiring members replaced, no net immigration of physics faculty to the US), how many faculty positions open up each year, on average? Third, given that about 1400 physics PhDs are conferred each year,² estimate the proportion of recipients who will eventually get permanent (or quasi-permanent, soft money) university physics positions in the US.

This letter is not meant to discourage students from attending physics graduate school, but rather to make them aware of the odds they will face if they choose to pursue an academic career.

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Editors Are Key to Improving Quality of Journal Articles

I would like to stress a couple of points alluded to in Benjamin Bederson's piece, "Communications in Physics," in the November 1997 issue of *PHYSICS TODAY* (page 63).

First, the task of ensuring that articles are clearly written is the responsibility of the journal editors. Article quality will improve only when editors insist upon it and refuse to publish articles—even those said to contain important results—that are not well written.

A number of years ago, I refereed a paper for a journal, and told the editor that I could not understand it. It

was published anyway, the excuse being that it contained a very important result that was already being referred to in other publications. Since then I have not had a great deal of sympathy for editors who complain that they receive too many papers. If the first criterion applied to every submission were clarity of presentation, and if any manuscript failing to meet this test were returned to the author with instructions not to send it in again until it had been rewritten, the number of submissions would soon drop, as writers got the message, and the quality would go up. But until editors apply such a standard, the situation will not change.

Second, journal editors who use referees should consider instructing them that an unclear presentation is quite sufficient grounds for rejecting a paper; there is no need to try and figure out what the author is trying to say—that is, to evaluate the scientific contents. That way, referees would only have to pay serious attention to the contents of clearly written manuscripts, which could reduce their load by 80–90% (judging from my own experience), and free up their time for serious consideration of better manuscripts. Scientists who have something useful to say but do not know how to write clearly should be encouraged to seek help from colleagues (not limited to their own institutions); their trying to rewrite something on the basis of detailed suggestions from an anonymous referee is not a good solution to the problem.

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Peer Tutoring Proved Successful in Past, Could Be Useful Today

John M. Clement's letter supporting the efficacy of peer tutoring (February, page 97) prompts me to point out that peer tutoring has an old and very successful history that seems to have been forgotten or ignored by today's radical education ideologists. An English educator named Joseph Lancaster (1778–1838) developed a form of elementary education known as the monitorial, or mutual, system that was based on the use of a kind of peer tutoring. The crowds of poor children who came to him spurred Lancaster's innovations. The students would gather in a single room filled with benches, each of which had a monitor—an older and more ad-

vanced student. An adult master taught the monitors, and then each monitor taught his row of perhaps ten students the lesson in reading, writing, arithmetic, spelling or higher subjects. The monitors also took attendance, examined and promoted pupils, checked books and slates and so forth.

Lancaster's school was quite successful. It was later reorganized as the Royal Lancasterian Institution, but severe financial problems led Lancaster to leave the project and emigrate to America, where he lectured extensively but failed in his efforts to start another school. Nevertheless, during the early 19th century, according to the *Encyclopaedia Britannica* (1960 edition), the monitorial system, "as developed by Lancaster, Andrew Bell and Jean Baptiste Girard, became perhaps the most widespread means of providing the rudiments of education for children of the common people on the [European] continent and in England and America, and helped pave the way for universal education supported and controlled by the state."

A modern example of success with peer tutoring was my own experience years ago as a student at Stuyvesant High School in New York City. As a new member of the school's math team, I learned almost all of my high school math before ever encountering it in class. Our team captain, Andy Farkas, would rapidly review solutions to contest problems, and would taunt us harshly if we were slow. We learned very quickly. No one cared about being insulted by just another student. The standard for math team members was getting a perfect score on the New York State Mathematics Regents examinations.

In my opinion, peer tutoring, properly supervised, could significantly improve academic performance in many subjects in today's urban schools!

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DOD Monitors Nuclear Tests Worldwide, Runs CTBT-Related Web Sites

PHYSICS TODAY readers interested in additional technical detail beyond that presented in Jeremiah Sullivan's excellent article on the Comprehensive Test Ban Treaty (CTBT) in the March issue (page 24) would be well advised to visit the World Wide Web site (www.pidc.org) of the prototype CTBT International Data Center

(IDC). The Web site presents the prototype products of the CTBT IDC. Updated hourly, it provides access to the nearly 70 000 elastic wave and radionuclide events recorded since 1995, when the initial monitoring system was put into 24-hours-a-day, 7-days-a-week operation. Although most of the events recorded are earthquakes, all of the French, Chinese, Indian and Pakistani nuclear tests conducted since January 1995 are also well recorded and the data are available for inspection. The Web site also contains a wealth of other information, including downloadable copies of the CTBT itself, a nuclear explosion database and monthly performance reports on the state and status of the prototype IDC. It also provides information on the current status of the seismic, hydroacoustic, infrasound and radionuclide international monitoring system. (Approximately 50% of the predominantly seismic IMS stations or their stand-ins are connected on-line to the prototype IDC, which is located in Arlington, Virginia.)

The US Department of Defense (DOD) is responsible for the implementation and operation of the US monitoring infrastructure in compliance with the CTBT. Within DOD, the Nuclear Treaty Programs Office (of which I am the principal program director) is managing the development of the prototype IDC, as well as its transition to the permanent CTBT IDC to be located in Vienna, Austria. Additionally, this office, with the Defense Special Weapons Agency as executive agent, is sponsoring a multi-million-dollar, peer-reviewed R&D program for the advancement of basic and applied research in the area of nuclear monitoring.

Through the Air Force Technical Applications Center, DOD also operates the US Atomic Energy Detection System and the US National Data Center (which are both headquartered at Patrick Air Force Base in Florida). The data center's Web page (www.tt.aftac.gov) can provide readers with useful CTBT-related information.

The US places great store on the implementation of an accurate monitoring system for the CTBT. To that end, DOD has supported the development of automated monitoring software and the training of future operators of this system. The US will be transferring all of those software assets to the CTBT IDC in Vienna in a series of four software releases. The first such release was made in early June, and the final release is planned for early 2000. Together, the automated monitoring system and the