measures Stony Brook continues to fare well. According to a letter from John Perdew and Frank Tipler (PHYSICS TODAY, October 1996, page 15), data from the Institute for Scientific Information places Stony Brook in thirteenth place among American physics departments for citation impact (citations per paper) in the period 1981–94, and third among public universities. Another study, similarly based on empirical criteria, concluded that the College of Arts and Sciences at Stony Brook was tied for second place among American public universities in the per capita quality of its research programs.[1]

Reference

1. H. D. Graham, N. Diamond, *The Rise of American Research Universities: Elites and Challengers in the Postwar Era*, Johns Hopkins U. P., Baltimore (1997).

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Japanese Researchers Reveal Tomonaga's Path to QED Renormalization

The year 1997 marked not only the centenary of J. J. Thomson's "discovery" of the electron (as commemorated in the October 1997 issue of PHYSICS TODAY) but also the golden jubilee of the birth of the renormalization program in quantum electrodynamics. Sin-itiro Tomonaga played a crucial role in this QED development, along with Julian Schwinger, Richard Feynman, and Freeman Dyson, as recorded in Tian Yu Cao's *Conceptual Developments of 20th Century Field Theories*.

In his review of the book by Cao (PHYSICS TODAY, June 1997, page 79), James Cushing states that "Cao displays a truly impressive understanding of both the history and content of these profound developments [quantum and gauge field theories]. Certainly this extended case study . . . is likely to become the standard reference on the history and conceptual underpinnings of modern field theory." We have found, however, that part of Cao's description of Tomonaga's contributions to renormalization is not accurate. We have also determined that the same basic misunderstanding appears in the 1980 book by Schwinger[1] and the 1994 book by Silvan Schweber,[2] although

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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 symposium[5] 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.

References

1. J. S. Schwinger, *Two Shakers of Physics*, Nishina Lecture Series, no. 13, Nishina Memorial Foundation, Tokyo (1980).
2. S. S. Schweber, *QED and the Men Who Made It*, Princeton U. P., Princeton, N. J. (1994).
3. K. Nishijima, Butsuri **53** (1), 55 (1998) (in Japanese).
4. S.-I. C. Tomonaga, *Tomonaga Sin-itiro Chosakushu* (Collected works of Sin-itiro Tomonaga), vol. 11, Misuzu Shobo, Tokyo (1983) (in Japanese).
5. Nihon-Butsuri-Gakkaishi **3** (3–4), 108 (1948) (in Japanese).
6. T. Tati, S.-I. Tomonaga, Prog. Theor. Phys. **3**, 391 (1948).
7. D. Ito, Z. Koba, S.-I. Tomonaga, Prog. Theor. Phys. **2**, 216 (1947); **3**, 276 (1948).

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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 calculations 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 replaccd, no nct 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.

Reference

1. J. M. Curtin, G. Blake, C. Cassagnau, *1995–96 Academic Workforce Report*, American Institute of Physics, College Park, Md. (March 1997).
2. P. J. Mulvey, S. Nicholson, *Enrollments and Degrees Report*, American Institute of Physics, College Park, Md. (January 1998).

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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