

Former Student Remembers Teller and Fermi with Gratitude

Edward Teller's science activities while at the University of Chicago are described in PHYSICS TODAY, August 2004, page 45. A similar article was published some time earlier (PHYSICS TODAY, June 2002, page 38) concerning how effective Enrico Fermi was as a science adviser, saving some researchers much time by telling them ways to improve their approach to the problem they were working on. Neither article mentions the interactions these great men had with those of us who were graduate students there at the time.

Teller was approachable by students, but he was also very busy. A student might find someone at the blackboard doing a problem under Teller's watchful eye while Teller was also talking to a US Army major from some Department of Energy group seeking advice on a weapons issue. But the most striking help we got was indirect. Those of us taking a class with Maria Goeppert Mayer heard her advice on how to go about solving a real problem, as opposed to a class problem. She said, "Save yourself time by asking Teller to guess the answer. He has such great physical instincts he can guess the answer within a few percent and thereby give you a running start."

Fermi was a hero to us when, during a visit to Los Alamos, he used the rudimentary computer recently installed there to show how previously unsolvable science problems could be solved. When he returned to the University of Chicago physics department after that very productive visit, he posted a notice to the students saying that he believed that the computer would become an essential tool for future physicists. So he proposed to teach a course in programming over several evenings, and he urged the graduate students to attend.

Letters and opinions are encouraged and should be sent to Letters, PHYSICS TODAY, American Center for Physics, One Physics Ellipse, College Park, MD 20740-3842 or by e-mail to ptletter@aip.org (using your surname as "Subject"). Please include your affiliation, mailing address, and daytime phone number. We reserve the right to edit submissions.

The course was in machine language, of course—a tedious and soon-to-be-abandoned process as higher languages were devised—but it clearly demonstrated the basics of how computers calculated and gave us each a head start on understanding how to use this new device, which indeed soon became essential, just as he had forecast. For a Nobel laureate to offer us that help seemed noble indeed.

On a later occasion, the chairman of the physics department told me that his job was very difficult. The staff consisted of mostly famous scientists, all of whom had active research projects under way; each pleaded not to be asked to teach during the coming term. Fermi, the exception, would wander into the chairman's office and say something like, "I need to ask you a favor. I am a bit weak in my solid-state physics just now; may I teach it this fall?"

Fermi's name is well established as a scientist of tremendous creativity and mathematical skill, but to a smaller group he bequeathed an example of life conducted generously and with grace.

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More Details on Hubble and Shapley

I noticed Sidney van den Bergh's letter on Edwin Hubble and Harlow Shapley in the September 2004 issue of PHYSICS TODAY (page 15), and I believe a clarification about the "Different Views" of these two observational astronomers is in order. Any reader not familiar with the history of galactic astronomy would be deprived of a most important footnote, namely the distinguishing definitions of the universe in the early 20th century.

Van den Bergh is essentially correct when he states that the discovery of "the existence of galaxies beyond the outer limits of our Milky Way system" is much to the credit of these men. However, he does not qualify that into the 1920s there were two

diverging theories about the extent of the universe: the old theory that defined the Milky Way galaxy as the universe, including all the "spiral nebulae" observed for over a century and denoted as gas clouds, within the Milky Way boundary; and the new one, that these other spirals were, on the contrary, external individual galaxies, coined as "island universes."

Shapley was the central figure of the former opinion based on his conclusions while observing globular clusters at Mount Wilson during the years 1914 to 1917. He declared that these clusters—and essentially all others—were confined within the Milky Way boundary. To the north, at Lick Observatory of the University of California, Berkeley, Heber D. Curtis had been studying for some 10 years the spiral nebulae that Shapley assumed without investigation to be gas nebulae, at distances similar to those of the clusters Shapley had studied. Curtis concluded they were spiral galaxies, and he became the chief proponent of the external universe view that the Milky Way was only one of many galaxies in a much larger universe. The scale of the universe became the central theme of the 1920 meeting of the National Academy of Sciences in Washington, DC. What has been called the "Great Debate" during that meeting was presentations by Shapley for the old theory and Curtis for the new. But it was never a debate; Shapley did not want to get into what Curtis projected enthusiastically to be a "scrap" over the two views. Shapley presented a safe astronomy lecture, whereas Curtis successfully made his point with a powerful, comprehensive research presentation.

The real debate was in the seesaw of papers that characterized the two views thereafter into the 1920s. Hubble's work at Mount Wilson began in 1919, where he used the recently completed and largest reflector in the world, the Hooker 100-inch, to also concentrate on the spiral nebulae. He and Shapley had very personal and professional differences that developed in the short time they were both at Mount Wilson. Hubble became convinced that the nebulae of the controversy were galaxies, and his

proof came in early 1924 when he determined that the so-called “Andromeda Nebula” was a separate galaxy far beyond the Milky Way.

So Shapley’s universe was profoundly smaller than the one Hubble revealed as an emerging cosmos—though the general applications of Shapley’s work deserve all the credit of astronomical history.

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A Brief History Lesson in Deep Ice Core Drilling

In his article on rapid climate change (PHYSICS TODAY, August 2003, page 30), Spencer Weart incorrectly credits Willi Dansgaard’s Danish team for augering the first deep ice core to reach the bottom of an active ice sheet from Camp Century, Greenland. This honor rests with B. Lyle Hansen and associates Herbert Ueda and Donald Garfield from the US Army Corps of Engineers’ Cold Regions Research and Engineering Laboratory in Hanover, New Hampshire. In July 1966, after a five-year field effort, they reached a depth of 1387 meters.¹ One of us (Langway) was responsible for developing the international study program for the Camp Century ice core.²

The Hansen crew also drilled the second ice core ever to reach bottom ice, in January 1968, at a depth of 2164 meters, from Byrd Station, Antarctica.¹ Both core drillings were extensions of the successful US International Geophysical Year projects in Greenland and Antarctica (1957–58) to deep-core drill into polar ice sheets for scientific purposes.³ The IGY studies were proposed, initiated, and led by Henri Bader, chief scientist, under an interagency agreement with NSF.

It was data obtained in these early drilling projects that ultimately led to the discovery of rapid climate changes and served as the foundation and justification for the follow-up international, multidisciplinary Greenland Ice Sheet Program by researchers from the US, Denmark, and Switzerland.^{4,5} It was also during the final three years (1979–81) of the GISP 10-year field and laboratory investigation that Danish drilling participants, led by Niels Gundestrup and Sigfus Johnson, augered the 2037-meter-deep third ice core to

reach the bottom of the ice sheet at Dye-3, in August 1981.

References

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Weart replies: Historians should work hard to be accurate, and the same applies to those who would criticize historians. What I actually wrote, and which is true, was that in the 1970s the most convincing evidence for rapid climate change came from an ice core drilled by Willi Dansgaard’s Danish group in cooperation with Americans led by Chester Langway Jr. I never said that theirs was the first deep core. The constraints of a brief article, which attempted to cover a great deal of ground, left no space to describe how the drilling campaign was but one stage in a prolonged effort of heroic proportions—an effort that began in the 1950s and continues today. (Attentive readers might have noticed brief mentions in my photo captions.) I have written more about the drilling campaign in the essay cited in the article, available at <http://www.aip.org/history/climate/rapid.htm>. Those interested in ice drilling history are also urged

to review and contribute to the additional but fragmentary information collected at <http://www.aip.org/history/sloan/icedrill>.

I am glad that Langway and Johannes Weertman have taken the trouble to draw attention to early deep ice drilling developments. Those named in their letter, and the many other institutions and people who contributed to that important task, deserve more recognition than they have received.

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Mixing the Practical and the Scholarly in Physics Education

John Neumann’s letter calling for inclusion of fluid mechanics in the physics curriculum (PHYSICS TODAY, June 2004, page 14) is quite interesting. I suggest, however, that mechanical engineers are generally better trained in computational methods than physicists are, and it is this training, rather than an academic course in fluid dynamics, which gives them an edge in applied problems.

I like the fundamental approach of the physics curriculum. In fact, I would argue for the reestablishment of professorships of natural philosophy and physics. Today’s graduate training seems to suffer from a fissure between course work on the one hand and, on the other, research in which professors and students are overly dependent on the tools of the trade—for example, canned computer codes for theoretical studies. That situation in turn leads to the stifling of really innovative and trenchant work.

Clearly there is a danger that an overemphasis on practical training and technical skills could shift the physics curriculum toward a course of study expected for a certificate from a vocational-technical institute rather than for a PhD from a major university. I have always found that the chemistry curriculum tends to have an orientation that emphasizes the practical rather than the scholarly, such that the poor physical chemist, for example, is offered no courses in optics, no classical or quantum electrodynamics, and just enough quantum mechanics so that the student can make sense of spectroscopy for chemical analysis. It seems to me that the American Chemical Society is minimalist in acknowledging the existence of quantum or theoretical