

letters

answers the question posed by Izaguirre.

The undergraduate and graduate training of most scientists is extraordinarily lacking in the topological perspective. This is a problem that the academic and scientific community must face and rectify. Scientists must be trained to separate geometrical (constitutive and local) features from topological (global or deformation-invariant) features of physical systems.

Reference

1. E. J. Post, *Phys. Lett. A* **94**, 343 (1983).
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Industrial astronomers

In his editorial (February 1986, page 128) urging that astronomy departments offer graduate training in "applied astronomy," James Wertz points out that "most astronomy faculty members have never held a job outside of academic astronomy." I strongly suggest that any applied astronomy program must include several faculty members with at least ten years' experience in industry or nonacademic research, in order that the applied astronomy taught be based on experience. We don't expect training in spectroscopy, for example, to be given by professors who have never done spectroscopy. If faculty with industrial experience are not present, the departments will be effectively telling the students that astronomers who work outside academia do not rate as "real" astronomers.

I do question Wertz's assertion that "the astronomer has a good conceptual understanding of orbits, attitude and celestial geometry." Twenty years ago, when I was a graduate student interested in satellite dynamics, most of the astronomy graduate students I knew were not particularly interested in orbits, and attitude dynamics was not considered in our celestial mechanics courses. Has the orbital mechanics content of astronomy education changed since then?

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Heisenberg and isotopic spin

In connection with Arthur I. Miller's discussion (November 1985, page 60) of Werner Heisenberg's contribution to nuclear and particle physics (they were

indistinguishable in the 1930s) I would like to point out especially the idea of isotopic spin.

The symmetry between proton and neutron as well as between pairs of mirror nuclei ($\text{He}^3\text{-T}$, $\text{C}^{14}\text{-O}^{14}$ and so on) was rather obvious after the discovery of the neutron as a real particle. But there was a temptation to treat this as a discrete mirror symmetry. It needed the talent and deep thinking of Heisenberg to introduce the idea of isotopic spin, that is, of a continuous rotation in abstract space gradually changing protons into neutrons and vice versa—the SU(2) group.

This idea led immediately to the prediction of the neutral pion as a necessary member of the representation comprising experimentally known charged pions. Later, with the discovery of strange particles, it led to the SU(3) group. So was laid the way to quarks.

Isotopic spin turned out to be an approximate symmetry, but it was the beginning of the long road leading to the idea of exact color symmetry and all of quantum chromodynamics.

Do not forget ancestors!

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Mathisson identified

The "unidentified conferee" in the photo also showing Wolfgang Pauli and Gerhard Dieke (PHYSICS TODAY, December 1985, page 40) is Myron Mathisson, a Polish mathematical physicist. Contrary to what is stated in the caption, the photo must have been taken in 1937 and not in 1934 because the 1937 conference at the Bohr Institute was the first and only one that Mathisson ever attended. Bohr invited Mathisson to that conference at H. A. Kramers's request.

Mathisson's knowledge of mathematics, and especially of the mathematics of general relativity, was probably not inferior to Pauli's—nor was his dislike of sloppy thinking weaker. In 1935 I heard Jacques Hadamard highly praise Mathisson for his work on the Cauchy problem, on the occasion of Mathisson's seminar lecture at the Collège de France. However, Mathisson was also a dreamer of the kind Pauli could hardly stand (in particular, Mathisson believed that there was evidence for some yet undiscovered links between general relativity and quantum mechanics). This explains Pauli's stronger interest in the sandwich he was eating (see the photo) than in Mathisson's ideas. Mathisson had better luck

with Paul A. M. Dirac: When Mathisson died in 1940 at the age of 43, Dirac wrote his obituary and published it in *Nature* (9 November 1940).

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Oscillation omission

The October 1985 news story (page 17) on the exciting discovery of quasiperiodic x-ray oscillations in low-mass x-ray binaries, in which we were involved, mentions a dozen scientists, but not the one person who most deserves credit: Michiel van der Klis of the European Space Research and Technology Centre.

Van der Klis was one of the coinvestigators on the EXOSAT proposal that led to the discovery of quasiperiodic oscillations in GX5-1. He analyzed the data (together with graduate student Fred Jansen of Leiden) and was the first to recognize the oscillations, and he demonstrated their unique relation to the source intensity. This was a remarkable accomplishment, and consequently he became the first author on our discovery paper.

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Pseudopotential pseudocredit

In John C. Bean's article "The growth of novel silicon materials" (October, page 36), he mentions on page 39 "pseudopotential calculations (performed by Richard Wright and Chris Van de Walle at the Xerox Palo Alto Research Center)." The reference should be to Richard Martin, not Richard Wright. This work is described in C. G. Van de Walle, R. M. Martin, *Phys. Rev.* **34**, 5621 (1986).

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Correction

March, page 88—In the obituary for Henry Levinstein, Nathan Ginsburg's name was misspelled. □