and UF_6 spectroscopy, involving as it did *heavy* spherical tops, was the first to reveal the clustering phenomenon fully, but this does not constitute discovery of the effect. Clustering was, in fact, of peripheral concern to our interest in identifying the SF_6 transitions that couple with CO_2 laser radiation, and it was not used in our analysis.

This is no way diminishes Dorney and Watson's discovery and explanation of the effect, nor Harter and Patterson's elegant further development of the theory. Their work has provided a useful semiclassical explanation for *why* the energy levels of spherical tops behave as they do, and it is currently finding application to the hyperfine structures of high-J rotational states that are now being observed as resolving power increases still further.

References

1. J. Moret-Bailly, Cah. Phys. **15**, 237 (1961); J. Mol. Spectrosc. **15**, 344, 355 (1965).
2. C. D. Cantrell, H. W. Galbraith, J. Mol. Spectrosc. **58**, 158 (1975). H. W. Galbraith, J. Chem. Phys. **68**, 1677 (1978).
3. J. P. Aldridge, H. Filip, H. Flicker, R. F. Holland, R. S. McDowell, N. G. Nereson, K. Fox, J. Mol. Spectrosc. **58**, 165 (1975).
4. I. Godnev, Acta Physicochim. URSS **20**, 189 (1945).
5. B. J. Krohn, Los Alamos Report LA-6554-MS, Los Alamos National Laboratory (1976); J. Mol. Spectrosc. **68**, 497 (1977).
6. R. S. McDowell, H. W. Galbraith, B. J. Krohn, C. D. Cantrell, E. D. Hinkley, Opt. Commun. **17**, 178 (1976). R. S. McDowell, H. W. Galbraith, C. D. Cantrell, N. G. Nereson, E. D. Hinkley, J. Mol. Spectrosc. **68**, 288 (1977).
7. A. J. Dorney, J. K. G. Watson, J. Mol. Spectrosc. **42**, 135 (1972).
8. K. Fox, H. W. Galbraith, B. J. Krohn, J. D. Louck, Phys. Rev. A **15**, 1363 (1977).

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Quantum Hall neglect

Five years have elapsed since Klaus von Klitzing's discovery of the quantized Hall effect (see PHYSICS TODAY, June 1981, page 17; July 1983, page 19; December 1985, page 17), yet little progress has been reported about its theoretical understanding. With all the experts who have had a go at it, one may now well assume that a theory of the effect cannot be a simple reduction to standard quantum methods. While this magazine is not the place to argue the pros and cons of alternatives, it does seem appropriate to call attention to a slight omission in your reporting.

Your reports of June 1981 and July

1983 mention a major theoretical hurdle that presented itself with the subsequent discovery of fractional quantization at Bell Labs in 1982. Permit me therefore to confront your readers with a published description[1] that quite naturally accommodates the original as well as the fractionally quantized Hall effect.

In my treatment, Hall impedance is expressed as the ratio of the two period integrals of flux and charge. They are straightforward space–time extensions of the time-honored Aharonov–Bohm[2] and Gauss–Ampère[3] integral laws. A rational fraction is now implied, because the first integral counts the flux units involved, and the second integral counts the electrons involved in this cooperative state of velocity order.

Neither in personal discussions with theorists here nor in their writings have I found any indication that they will even consider the just-cited integral laws as a replacement for standard quantum procedures. One may presume that Copenhagen convictions stand in the way of their acceptance. However, this would not be the first time in recent years that macroscopic quantum effects have favored an Aharonov–Bohm type description.

Yet more important than the merits or demerits of the cited description is the frightening closed-shop attitude of American physics. I gladly accept the good faith of your reporter; I am not too sure, though, about his sources. The need for proprietary claims and continued funding, so prevalent in American physics, may someday deceive the pursuit of truth.

References

1. E. J. Post, Phys. Lett. A **94**, 343 (1983).
2. E. J. Post, Phys. Lett. A **92**, 224 (1982).
3. E. J. Post, J. Math. Phys. **19**, 374 (1978).

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I recently read Francisco Izaguirre's letter (October, page 15) with regard to the quantized Hall effect, as well as your related story (December 1985, page 17) and Bertrand I. Halperin's article on the topic in *Scientific American* (April 1986, page 52). I was surprised to find no mention of E. J. Post's work, which is based on topological arguments.[1]

As Halperin and Izaguirre point out, the fractional Hall effect is surprising to many because it does not appear to be sensitive to size, shape or material. There exist a number of such physical properties, and the list is growing as physicists become sensitized to the idea that topology as well as geometry has an important place in physics. Independence from size, shape and sample means that a phenomenon is topological deformation invariant. The most obvious other example of such a topological phenomenon is the Planck blackbody radiation spectrum. A bit of reflection will show that other deformation-invariant laws of nature include Gauss's law, Ampère's law, the Bohr–Sommerfeld quantization condition, the Aharanov–Bohm effect and the transition probability.

I have a private letter from Post dated 15 September 1981, expressing his rational-fraction result for the Hall impedance and saying to watch for the pending publication. For some reason known only to referees and reviewers, his article was not published until March of 1983. Moreover, the work seems to be unknown to or ignored by the physical community active in work on the fractional Hall effect, and

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 (He^3–T, C^{14}–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. □