# LETTERS

## Put the Real World in the Physics Curriculum

During the decades that I have been teaching physics, I have observed that university degree programs have been transforming into a predominantly theoretical physics curriculum. The graduate program at Southern Illinois University at Edwardsville leading to a master's degree has a total requirement of 30 semester hours, out of which 23 hours are in core theoretical courses (mechanics, electricity and magnetism, quantum theory and so on) and 7 hours are electives to be taken from physics, mathematics, engineering and so on; 6 hours of thesis work are required. It appears that only in the thesis area can a student plan to learn about experimental physics (unless the student also does the thesis in theoretical physics). Hence the theoretical physics content of the curriculum is between 80 percent and 100 percent.

I would imagine that this curriculum is just about typical. Being a theoretical physicist myself, this suits my repertoire, since I am not trained to supervise any advanced laboratory courses. However, it seems that our curricula are getting quite lopsided and do not provide any training to the student who may one day want to contribute to applied physics problems or work on problems of great importance to industry.

Not only is there a paucity of actual laboratory courses, but there is a sheer absence of anything remotely related to a laboratory measurement or an experimental method in the theory courses and the textbooks. If the student is somewhat like me, less an experimentalist and more of a theorist, he or she is going to graduate with little idea of how various physical quantities are measured and with no idea of how laws and theoretical models are put to test. The instructor cannot do anything in this matter because of the manner is which the textbooks are written and because it is not easy to dig up supplementary material to deal with experimental methods. In my department, it is fair to say that I don't blame some of my experimental physics colleagues, who routinely avoid graduate course teaching assignments.

This shift to a theoretical emphasis has also crept into the undergraduate curriculum. The baccalaureate degree program here requires a total of 34 hours in physics, out of which only 6 hours are required laboratory courses. Even the standard courses after the introductory physics courses have become more and more theoretical. For instance, the junior–senior electromagnetic theory course uses a textbook that deals with electric and magnetic susceptibility, molecular polarizability, impedance, skin depth in metals, complex index of refraction and so on as mere theoretical constructs and never mentions a single experimental method to determine or measure these quantities. The required laboratory courses do not deal with these matters. I am afraid that the student taking such a course is going to come away with the idea that these are purely theoretical concepts with no relation to the real world.

We all agree with Einstein that "the creative principle resides in mathematics, but experience remains of course the sole criterion of the physical utility of a mathematical construction." Textbook authors need to make sure students get some understanding of this "experience" by including lab- and measurement-related material in their books. The onus is particularly on authors because professors tend to follow their lead.

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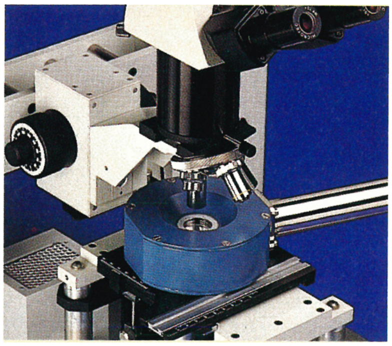
As a nonphysicist and humanist who nevertheless occasionally reads parts of PHYSICS TODAY, I would like to offer my perspective on the exchange between Victoria Kaspi and Jearl Walker (March, page 128) about violent images in the new, fourth edition of *Fundamentals of Physics* by David Halliday, Robert Resnick and Walker (Wiley, 1993). I agree that many of the images as described by Kaspi are indeed a bit gruesome.

On the other hand, I'm not convinced that presenting images showing the "inherent, natural interest of the subject matter itself," "elegant equations" and "beautiful concepts," as she thinks the third edition did, is

*continued on page 121*





the way to attract first-year students to the discipline. Interesting, elegant and beautiful, yes, but to whom? To physicists, naturally.

If, as a student trying to decide on a course of study, I had taken a freshman physics class whose textbook images showed magnetic levitation, a laser used in fusion research or Maxwell's equations—examples cited by Kaspi from the third edition—I would not have seen their appeal. With little previous background in physics, what basis would I have had on which to judge the beauty and elegance of the concepts illustrated?

I do not mean to say that going to the other extreme by capitalizing on the human fascination (even against our will) with scenes of disaster and destruction is a good approach. Just because a tactic gets students' attention doesn't mean it is a valid means to an educational end.

There must be some middle ground, which in fact I think the fourth edition of *Fundamentals of Physics* reaches very nicely. Without referring to Kaspi's letter, I looked through the new textbook and really had to search hard to find the violent images she describes. What I did find instead was a pleasant surprise—not laboratory images but real-life, familiar examples taken from the natural world and the fields of art and music. Those images far overshadowed the sensationalist ones Kaspi mentions and caught my interest immediately, making me think, "Why weren't there textbooks like this around when I was in school?" Maybe if there had been I might have studied at least some physics past the high school level.

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## Torrey on the History of NMR

Edward Apgar (May, page 88) gives me far too much credit when he says that my "expertise" (derived from some supposed "unique insight") "made possible the design of the first successful experiment on nmr in solids."[1] In particular, his statement that this insight "led to improved estimates of spin–lattice relaxation time and of the rf voltage level needed to avoid saturation" is simply incorrect. Actually, I did estimate the spin–lattice relaxation time for paraffin using the Waller theory[2] for paramagnetic crystals involving the two-phonon process for the exchange of energy between the lattice vibrations and the spin system and got a $T_1$ of about an hour. It turned out later[3] that there is a more efficient relaxation mechanism, involving slow diffusion processes, leading to a relaxation time (in accord with experiment) of about $10^{-2}$ seconds. The precaution we took of "cooking" the sample in a magnetic field for several hours prior to measurement was therefore quite unnecessary, and no special measures to avoid saturation were required (none in fact were taken).

When in December 1945 it came time to sign the letter to the editor of the *Physical Review*[1] announcing our first observation of nuclear magnetic resonance in a solid, Robert Pound and I insisted that Edward Purcell be the first to sign, in acknowledgment of his leadership and central role in our endeavor; the order of the other two names was decided by a coin flip.

Apgar's expressed hope for a more detailed account of the first successful experiment will be at least partially satisfied by the expected appearance later this year of the historical volume of the *Encyclopedia of Nuclear Magnetic Resonance* (later volumes to follow in a few months), to be published by Wiley. That volume will contain articles by many people, including Pound and myself, outlining their contributions to the beginnings and early development of nmr.

### References

1. E. M. Purcell, H. C. Torrey, R. V. Pound, Phys. Rev. **69**, 37 (1946).
2. I. Waller, Z. Phys. **79**, 370 (1932).
3. N. Bloembergen, E. M. Purcell, R. V. Pound, Phys. Rev. **73**, 679 (1948).

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## A Supersymmetrical Decade

I read with interest the news story "Supersymmetric QCD Sheds Light on Quark Confinement and the Topology of 4-Manifolds" (March, page 17). I should like to point out that important contributions that were precursors to the astounding work of Nathan Seiberg and Edward Witten also merit recognition.

In 1974 Bruno Zumino and Julius Wess were responsible for the introduction into the Western physics literature of the concept of supersymmetry in field theories over 4-manifolds. Even prior to that time, supersymmetry was known in the Russian physics literature due to Y. A. Gol'fand and E. P. Likhtman. The topic of $N$=2 supersymmetry on 4-manifolds was initiated by Pierre Fayet in 1976, and $N$=2 supersymmetric Yang–Mills theory was proposed in its most concise form by Richard Grimm, Martinus Sohnius and Wess in 1978. Finally, in 1984 S. James Gates made the critical discovery that the action of $N$=2 supersymmetric Yang–Mills theories allows for a description in terms of a holomorphic function.

It may also be of interest that Gates, a faculty member at the University of Maryland at College Park, is an African-American, the first recipient of the APS's Visiting Minority Lecture Prize and the current president of the National Society of Black Physicists.

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*[Editor's note: Other discussions of the history of supersymmetry appeared in letters in* PHYSICS TODAY, *May 1992, page 13 and November 1992, page 119.]*

## 'Birth Control' of PhDs: Can It Work? Should It?

Sudip Chakravarty's Opinion column (January 1995, page 53) misses the point of the argument for "birth control" of PhDs in physics. He posits a perfect world in which everyone is allowed to study whatever they want and it all works out in the end. As he puts it, "We may have to wait another 20 years before we can assess their careers, but the anecdotal accounts I have heard have been very reassuring." But the problem with relying on anecdotal evidence is you only hear from those who want to talk to you. The anecdotal accounts *I* hear are not very reassuring: People spend years in graduate school, then more years in multiple postdocs, then more years in part-time or temporary employment before finally settling for positions that their employers would have happily offered to people with less formal training but more industrial experience. (In other words, many of us these days get positions in spite of our PhDs, not because of them.) Yes, in the end most of those people are able to find stable, interesting careers; people with drive and talent usually are able to overcome the mistakes they make when they are young. Is that any reason to urge them to make those mistakes? In the job market for