Professional Development Is One Part of Science Education Solution

Much of the wonderful work on K–8 science education reported by Ramon E. Lopez and Ted Schultz (PHYSICS TODAY, September 2001, page 44) will be wasted, I'm afraid, unless greater emphasis is placed on training and supporting teachers.

Not long ago, in an affluent suburb known for its excellent schools, I helped my son's fourth-grade class with a unit on pulleys. The students were to measure the weight required to lift a standard load and the distance the load rose, using one, two, and three pulleys. It sounded straightforward. But the pulleys had a lot of friction and were not light compared to the load. Neither the students nor the teacher realized that the distance needed to be measured from the load's initial height, not from the floor; and no effort was made to keep the strings close to vertical. With attention to these details and judicious use of some oil, we obtained reasonable results, and had a good discussion about what they meant. Apparently that had not happened before. Another teacher who saw the results on the blackboard was astonished to see patterns that actually made sense. I can only imagine the impression of science that this unit had left on previous classes.

In other subjects, these talented and experienced teachers had no trouble improvising, identifying and solving problems on the fly, and helping students understand what they were doing. But in science they were adrift, and the kids could sense it. Teachers need help. At a minimum, every elementary school should have a full-time science specialist. In my son's school, that position had been eliminated to fund a computer room.

ROGER G. TOBIN
(rtobin@tufts.edu)
Tufts University
Medford, Massachusetts

LOPEZ AND SCHULTZ REPLY: Roger Tobin illustrates quite well what can go wrong in science education. Teachers must have both the scientific and pedagogical content knowledge to teach science effectively. But good professional development is not enough. Tobin also has put his finger on two other essentials.

First, good instructional materials should not suffer the kinds of design problems Tobin indicates, and they should come with a teacher's manual that makes clear the nature of the measurements and the physics underlying them. Good materials should also not be isolated, however good their "gee-whiz" features, but should be part of an extended investigation that will lead to a deep understanding of the science and will enable students to develop increased skill in actually doing science.

Second, to implement such a program, understanding and support must come from both the administration and the community. Tobin's involvement in the classroom seems to have been key to a successful outcome in the case he reports. This is a good example of why scientists must be involved in science education.

As we discussed in our article, not only are ongoing professional development, good instructional materials, and administrative and community support essential, but so are a system for supporting the materials and an assessment program aligned with the curriculum. Only when these five elements are in place has a school system undergone what we believe to be a systemic reform that is likely to mean a good and sustainable science education program.

RAMON E. LOPEZ
(relopez@utep.edu)
University of Texas at El Paso
TED SCHULTZ
(schultz@aps.org)
American Physical Society
College Park, Maryland

In his letter "Educating Students to Appreciate Physics" (PHYSICS TODAY, October 2001, page 11), Stewart Brekke makes excellent points about developing high-school physics courses that are more "user-friendly." There is no curricular magic bullet for motivating students, but there is a magic gun—high-caliber teachers like Brekke. An enthusiastic and knowledgeable teacher inspires students to go where they've never gone before, leaves them yearning for another trip, and usually manages to avoid being too limited by course materials or administration.

We don't usually remember our textbooks or the flow of a course that we took. We do often remember our teachers, and may strongly identify the subject they taught with that memory. True, some students are inspired entirely from reading texts and find the teacher nearly irrelevant, but those are a small minority, probably well-served by existing resources.

Writing a finely tuned curriculum can be a validating experience for an inspired teacher, but once the product is adopted, it often ends up serving inflexibly as a crutch for poor teachers and a constraint on good ones. We certainly need competent texts and solid curricula. But the greater need—and the greater challenge—is to develop teacher training that requires really learning the technical subject matter and demands good skills, both teaching and interpersonal. Another important requirement is to filter out those without a spark for teaching before they become protected by a tenure system that is the envy of most other professions. Then we can deal with how much teachers get paid and how burnouts should be handled in a tenure system.

STEVEN BITTENSON
(sbittenson@odysseyconsortium.com)
Odyssey Consortium
Bedford, Massachusetts

Physical Truth Without the Relatives

Daniel Kleppner writes, in the March 2001 issue of PHYSICS TODAY (page 11), "Nevertheless, essentially all physicists share certain beliefs" and then lists several key articles of faith. I commend Kleppner for pointing these out and

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for the generally excellent opinion piece that followed. However, physicists must hold to another key belief, without which physics would be impossible: the idea that physical truth is ultimately absolute. Adherence to this belief is important since the prevailing mood today is quite the opposite. Many people seem to want truth to be relative, a pure construct of the feelings and social preferences of the observer. Indeed, in other areas of academic inquiry, people do argue passionately for a relative view of truth that is decided by social convention or status. They often have little tolerance for those who view truth as absolute. Within such a worldview, physics becomes an intolerant intellectual activity.

Granted, our knowledge of physical truth is only an approximation of a more complete understanding. However, once we subscribe to the idea that a complete understanding is and always will be a human invention and that no knowledge stands independent of human viewpoint, then physical inquiry ceases to be a rational activity.

DOUGLAS KEIL
(douglas.keil@lamrc.com)
Fremont, California

Theory, Experiment, and Shelter Island

Norman Ramsey has added to the discussion of the relative importance of experimental and theoretical events in major advances in physics (PHYSICS TODAY, July 2000, page 15; January 2001, page 13; September 2001, page 78). Ramsey discusses the two deviations from the predictions of Dirac theory for atomic hydrogen that were measured with high precision in early 1947. The presentation of these results at the Shelter Island meeting in June 1947 to an audience that included theorists such as Hendrick Kramers, Hans Bethe, Julian Schwinger, and Richard Feynman provided the impetus for solving the problem of divergences and completing the structure of quantum electrodynamics (QED). Ramsey mentions especially the measurement of the hyperfine anomaly in hydrogen, an experiment he and I. I. Rabi had designed. He also stresses the importance of the measurement by Willis Lamb and Robert Retherford[1] that definitively proved the reality and magnitude of the splitting between the $2^2S_{1/2}$ and $2^2P_{1/2}$ hyperfine levels of atomic hydrogen, the famous Lamb shift. I can add a footnote to that history.

In 1953 or early 1954, Lamb was a visiting professor at Harvard University. He devoted his course (which I took) to a detailed review of theoretical and experimental aspects of the Lamb and Retherford work, based on the series of *Physical Review* papers describing it.

I still have a clear memory of his telling that class how William Houston's measurements, in 1937, of the fine-structure splitting in the Balmer lines showed a distinct displacement from the theoretically expected value,[2] and what a mistake theorists had made in relying on a claim that it should be explained away as an experimental error. Houston was a supremely skilled spectroscopist. It was very unlikely that he could have been misled in such an important measurement. Lamb used this history to point out the perils of the temptation to treat evidence from a particularly reliable source with diminished weight because it deviates inconveniently from a well-established prevailing model. Since this is the thing that most clearly comes back to my memory 48 years later, he must have been very impressive about it. It was a good lesson to heed.

Lamb must also have mentioned the results, in 1938, of Robley C. Williams,[3] which confirmed Houston's observation of the shift, and the work of J. W. Drinkwater and colleagues in 1940,[4] whose results contradicted Houston and Williams and supported the Dirac theory; Drinkwater and colleagues put forward the argument that both Houston and Williams were misled by impurities in the source. All three papers and the experimental controversy are mentioned in the first short paper by Lamb and Retherford.[1]

My memory of this incident may be of historical value in that it reveals the special credence Lamb gave to Houston, and therefore to the value of his results. There is no clue to this in the Lamb and Retherford paper, where the three papers are treated totally even-handedly. Lamb, however, had specific reason to evaluate the Houston and Williams papers in 1939. He was then engaged in a controversy about some calculations that aimed to explain the 2S shift by a deviation from Coulomb's law arising from a meson contribution to the electron–proton interaction.[5] (Incidentally, Lamb never thought this shift was an electrodynamic effect, even after the discussion at Shelter Island in 1947, until he received Bethe's calculation of it a couple of weeks later.[6])

I conclude that Houston is a half-forgotten hero of this story, the grandfather of the Lamb shift measurement (and a great uncle, maybe, of the QED revolution after Shelter Island). Lamb must have had confidence from the beginning in just about what the result would be, and that confidence would have helped enormously in supporting the continuing determination needed for such a tour de force experiment.

Lamb was not the only theorist who believed Houston. Victor Weisskopf, the only theorist to start (with Anthony French) the electrodynamic calculation before Lamb's results were known, began it because he believed the Houston and Williams measurements (ref. 6, p. 245).

All the tracks lead back to Houston, then. And those experiments were done to test the Dirac theory—and the Coulomb law. So, which came first, theory or experiment?

Houston was right, of course. From his results, the value of the shift was about 0.03 cm^{-1}. Lamb and Retherford's first result in 1947 was 10% higher. The final value moved up about 6% more. Not bad for 1937.

References

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6. S. S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger and Tomonaga*, Princeton U. Press, Princeton, New Jersey (1994), p. 191.

FELIX T. SMITH
(ftsmith@mclvax.sri.com)
SRI International
Menlo Park, California

Correction

November 2001, page 26—In the figure caption, Hans Bethe and Boyce McDaniel are bicycling around the 10 GeV Cornell electron synchrotron tunnel, not the Cornell Electron Storage Ring. CESR was added to the tunnel in 1979, more than 10 years after the photograph was taken. ■