simple apparatus. Such experiments depart from the more technically demanding condensed matter experiments using dilution refrigerators, accelerators, reactors, and the like. Thus the poles of big and small physics have moved farther apart.

Of course, as Trilling says, all physicists should marvel at the intellectual achievement of the standard model. They should also marvel equally at the intellectual achievement represented by semiconductors and superconductors, critical phenomena, polymers, Bose–Einstein condensates, greater understanding of the universe, and others. Likewise, I agree that we all use tools that were developed by others. Not only do condensed matter physicists use accelerators, but particle physicists use superconductors and solid-state detectors. In this vein, I agree with Larry Merkle that we physicists do have much in common, but also find, as does he, that it is difficult to identify exactly what is the glue that holds us together.

The split that I see between theory and experiment is not the kind Trilling mentions. Simply put, I do not think that we train our students to do both kinds of physics. Very few, if any, of our "superstars" do both theory and experiment. In the past we had many great examples (starting with Galileo and Newton, going through Faraday, and up to Fermi) of such a cohesive approach. I do not think such a dichotomy exists in biology-related sciences; that is partly why those are such exciting fields for young scientists these days. The separation of theory from experiment has, of course, gone on for a long time; arguably, the separation occurs because physics has become a mature discipline.

There have been consequences to the split between theory and experiment. I cannot speak for other fields, but in my own area of condensed matter physics, we tend to think of theorists as the intellectual leaders. That is certainly not always true, but is true more often than I would like. I find it disappointing that we experimentalists have often yielded the intellectual leadership to theorists. This issue is complicated and may again be entwined with the maturity of physics. But I do not think it an accident that, of the physicists the general public knows and admires from the 20th century, nearly all are theorists. Richard Feynman and Stephen Hawking are only the two most recent examples. We physicists have raised them up as our common heroes for public admiration. I am uncomfortable that, for many decades, we have not had experimentalists that we have recognized as heroes across all disciplines.

John Rigden's letter also relates to the maturity of physics. I agree with his opinions, and I wish I had devoted more of my essay to education reform. That topic underlines one of the unappreciated problems of regarding physics as a mature discipline: Maturity may be admirable in many ways, but it may also be a final stage preceding dramatic decline. Educating bright students and training new researchers is the lifeblood of any science. If our curriculum, the artery bringing this lifeblood to our field, has become as rigid as Rigden maintains, we have a classic sign of impending doom. Because we certainly believe that our field has a bright future as well as a distinguished past, we must find ways of revitalizing our curriculum and keeping our field rejuvenated.

A further indication of the crisis in physics has recently appeared: the spate of horrendous data fabrication and scientific misconduct that has reportedly riddled such high-profile research as that of Jan Hendrik Schön on molecular-scale transistors and of Victor Ninov on element 118. As physicists, we have been trained to guard against errors in science in our own work and in that of others. We are probably pretty good at that, although greater care can always be taken. However, guarding against a hoax is entirely different from guarding against a mistake. Can we any longer trust that researchers are reporting their results to the best of their ability? The physics community on the whole seemed rather pleased when Alan Sokal, in *Social Text*, perpetrated a hoax about postmodern physics. The joke, if there ever was one, has now gone sour. We physicists are as easily duped and as open to ridicule as are our academic colleagues across campus.

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Datz's Early Work Clarified

The recent obituary of my longtime friend and colleague Sheldon Datz (PHYSICS TODAY, November 2002, page 88) contains several inaccuracies. As a result, the description of the work with molecular beams presents a confusing chronology.

The steps that led to the first successful crossed-beam experiment on a chemical reaction were as follows. In 1945, I joined the chemistry division of Clinton Laboratories (now Oak Ridge National Laboratory); by 1949, I was working toward a crossed-beam study of a chemical reaction, that of hydrogen bromide with potassium. The extent of reaction was to be determined by collecting the resulting KBr on a cooled foil and measuring the amount by neutron activation of the bromine. The surface ionization gauge with a tungsten filament could not be used to measure the resulting KBr separately, because it detected both K and KBr with high efficiency. It was to be used to measure the total number of collisions, both reactive and elastic.

When Sheldon accepted a position at the laboratory in 1951, we began designing and building an apparatus specifically for the study of such chemical reactions. We pushed ahead with adapting neutron activation, but we both hoped that some variant of the much more convenient surface ionization gauge would be found that could distinguish between K and KBr. One came by accident. Like many other researchers, we found the large and noisy background signal from tungsten to be a problem. When we heard that a filament of platinum—which, like tungsten, is a metal with a high work function—tended to show less positive-ion noise, Sheldon obtained a sample. He measured the relative efficiencies for K and for KBr, quantities necessary for analyzing the planned experiments, and found that platinum detected K with an efficiency around 60 to 65% but KBr with only about 0.1%. That was the variant we sought.

Thus, the two-filament gauge was the fortunate first step in the birth of experimental chemical dynamics, not the logical result of our two later papers on surface ionization. Nor were those papers the first quantitative studies of surface ionization detection, a subject going back to Irving Langmuir in 1913.[1]

For the record, Sheldon needed no guidance for his PhD thesis: It was his project from conception to interpretation. His thesis adviser was W. T. Smith, not Richard D. Present. In addition, Sheldon was not assistant director of the lab's chemistry division; he was associate director along with Ralph Livingston for the period mentioned in the obituary.

It was a privilege and a pleasure to be closely associated with Sheldon for so long. We were friends as well as colleagues from a few days after he joined my group on the Manhattan Project.

Reference

1. I. Langmuir, *Phys. Rev.* **2**, 329 (1913).

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RCA's Role in Early Superconducting Magnet Research

We would like to add a note concerning some of the historical information published in our article "Superconducting Magnets Above 20 Tesla" (PHYSICS TODAY, August 2002, page 37). During the 1960s, the RCA Laboratories in Harrison, New Jersey, also made significant contributions to the early development of niobium tin conductors and magnets, based on the chemical vapor deposition process. In particular, by 1968, RCA had developed a 14-T magnet using their Nb_3Sn tape conductor. The company discontinued development of superconducting magnet technology in 1970. We thank J. J. Hanak for bringing to our attention the details of RCA's activities.

Our article was not intended as a rigorous review, and the history was intentionally kept short to highlight the progress in the early days of development. Rather, our aim was to give some perspective for later large-magnet development at extreme magnetic fields, development that includes the use of the new high-temperature superconductors.

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Sometimes It's the Ref Who Fouls Out

Refereeing remains essential to scientific publishing, and most authors appreciate and benefit from it. The abusive report, fortunately uncommon, represents one of the exceptions to the ideal of careful, thoughtful refereeing. For veterans of publishing, a vitriolic report is more an irritant than anything else, but newcomers can become discouraged. My daughter's thesis adviser, a molecular biologist, told me that she was so upset by the abusive report on her first submitted paper that she nearly gave up research.

Even if the referee is justified in pointing out errors, the use of harsh language remains unjustified. Such lack of judgment in matters of professional courtesy may also be indicators of poor scientific judgment. For example, a recent paper of mine on Laplacian orbit determination was declared "hopeless" by the referee, despite the fact that I successfully calculated an orbit using the method. A careful reading of the report confirmed my suspicion about that referee's scientific judgment: He showed little concern with factual errors and apparently had a visceral reaction to anyone questioning Gaussian orbit determination, an alternative to Laplace's method.

The ideal solution to abusive referee's reports would be to eliminate anonymity. I always sign my referee's reports. If authors disagree with me, at least they know where to direct their ire. Editors, however, seem to prefer anonymity for referees. An author who receives an abusive report should immediately request review by another referee.

The editor of the journal to which I submitted the paper on Laplacian orbit determination afforded me an even better solution. He sent me an e-mail followed by a letter; in both, he apologized for the harsh tone of the report and offered to send my paper to another referee. That type of response should be standard among journal editors. It does add to their workload, but without submitted manuscripts, editors have no job to perform. And authors, who spend considerable time and effort to make a manuscript publishable, deserve an impartial review and, certainly, a modicum of courtesy.

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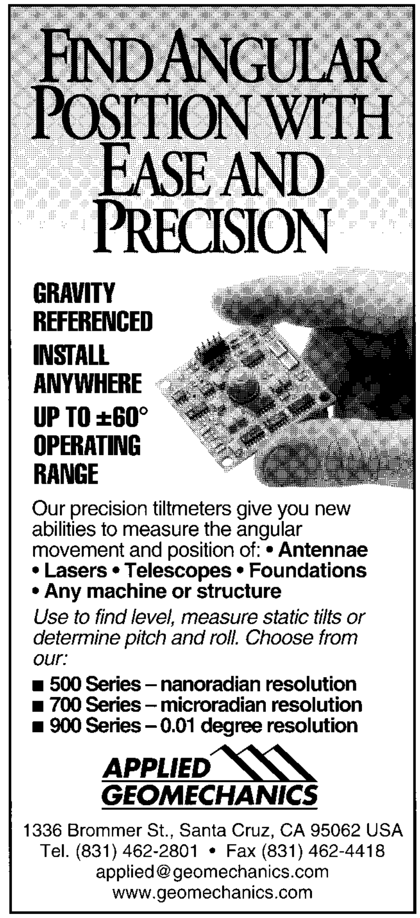
Correction

November 2002, page 87—The Institute of Physics announced its award winners for the year 2003, not 2002 as reported. ■



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