of modern physics, including quantum electronics. (Moreover, by extending the photon concept to the interaction with electrons even in bound states, Einstein later predicted both the maser and laser phenomena.)

Reference

1. An excellent English-language account is given in E. Hecht, A. Zajac, *Optics*, Addison-Wesley, Reading, Mass. (1974), p. 444.

Max J. Lazarus
(m.lazarus@lancaster.ac.uk)
University of Lancaster
Lancaster, England

I would like to add a positive note to Allan Franklin's article and especially the photo of him (page 33) on his way to a place in Washington State named Electron. There is a place in Ontario, about 30 miles north of Toronto, called Proton.

James M. Daniels
(daniels@pupgg.princeton.edu)
University of Toronto
Toronto, Ontario, Canada

FRANKLIN REPLIES: George Trilling and Max Lazarus are correct, of course. The "discovery" of the electron was not a single event, but involved the work of many scientists. One could make a good case for Zeeman, Lorentz or Kaufmann as either discoverers or codiscoverers, along with J. J. Thomson. I was unaware of the work of Hallwachs, Elster and Geitel that Lazarus refers to, and they should also be added to the list.

The intent of my article was to construct a possible historical argument for the existence of the electron and not to give a complete history of its discovery. A much more complete account was given by Robert Rynasiewicz of Johns Hopkins University in a fascinating talk at the American Association for the Advancement of Science meeting held in Seattle in February 1997.

Allan Franklin
(allan.franklin@colorado.edu)
University of Colorado at Boulder

Caricature of Meitner Countered by Drawing on Historical Record

Carl Benedicks's highly unflattering caricature of Lise Meitner and his insulting notation, which are reproduced in *A Nobel Tale of Postwar Injustice* (PHYSICS TODAY, September 1997, page 26), highlight a point not mentioned in the article about why she was not awarded a Nobel Prize in the mid-1940s: To what extent did her being a woman affect the decision? I am surprised the subject isn't touched on.

Irene Newhouse
(newhoir@mail.auburn.edu)
Springfield, Ohio

CRAWFORD, SIME AND WALKER REPLY: We regret that, when the article was published, our original explanation of Carl Benedicks's caricature was inadvertently omitted from the caption. Indeed, his note—Swedish for "Mr? Mrs? Miss Lise Meitner"—indicates that he regarded Meitner as sexless and abnormal. In contrast to his sketches of the two men, his depiction of her is a gross distortion, again indicating his revulsion against the presence of a woman (and possibly also a Jew) in the Royal Swedish Academy of Sciences.

But to what extent did gender bias influence the Nobel decisions against Meitner? In our article, we focused on the Nobel documents in which gender bias and antisemitism do not explicitly appear. Nevertheless, the following brief review of Meitner's experience in Sweden may suggest some possible answers.

In the 1930s, Meitner was in the top echelon of nuclear physicists worldwide, nominated for a Nobel Prize some 15 times, in chemistry and in physics, for her work both with Otto Hahn and independent of him. She was not unknown when she arrived in Sweden in 1938.

She accepted the position in Manne Siegbahn's institute because she knew that experimental nuclear physics was just beginning in Sweden (Siegbahn had only recently switched from x-ray spectroscopy to nuclear physics), and she hoped to contribute to its development. Instead, she was excluded on at least two fronts: as a woman, as a foreigner and (given what we now know about the antisemitism of the Swedish elite) perhaps also as a Jew. In Siegbahn's institute, she was given a room but no students, no assistants, no equipment, not even the keys to the building; she was neither invited to join Siegbahn's group nor given the resources to form her own.

One telling indication of Meitner's outsider status in Sweden was that although she had been a pioneer of beta spectroscopy, when Siegbahn's son Kai began work in the field (for which he later got a Nobel Prize), Meitner was never consulted. When she complained, she was regarded as difficult.

Would a man of Meitner's stature have been so marginalized? We cannot definitively answer that question, but we are certainly entitled to ask it. In 1957, Meitner wrote to her friend James Franck that in Sweden "just being a woman is a semi-crime."

Although Meitner had good friends and colleagues among Swedish physicists, her poor relationship with the influential Siegbahn and his disciples (such as Erik Hulthén) undoubtedly destroyed her chances for a Nobel Prize. After the war, Siegbahn may have viewed Meitner as a competitor for funds and prestige, but if their relationship had been better all along, they could have been colleagues and not competitors (and Swedish nuclear physics might not have lagged so far behind during the war). In any event, at the time, Meitner's Swedish friends were convinced that she had been pulled down by Siegbahn for "dark reasons of prestige" and that she was a victim of "royal Swedish jealousy" (to quote from their letters). Ironically, then, it appears that Meitner's close contact with the Swedish Nobel establishment diminished rather than increased her chances of getting a Nobel Prize.

Elisabeth Crawford
(e.crawford@gersulp.u-strasbg.fr)
National Center for Scientific Research (CNRS), Institute of the History of Science, Louis Pasteur University
Strasbourg, France
Ruth Lewin Sime
(rodsime@csus.edu)
Sacramento City College
Sacramento, California
Mark Walker
(walkerm@union.edu)
Union College
Schenectady, New York

Nobelists Played Roles in Implementation of 'Fountain' Experiment

In our May 1996 letter to PHYSICS TODAY (page 89), we traced the history of the atomic clock with emphasis on Jerrold Zacharias's "fountain" experiment. The awarding of the 1997 Nobel Prize in Physics to Steve Chu, Bill Phillips and Claude Cohen-Tannoudji prompts us to revisit the story and offer this brief addendum.

In our account, we described the gap of three decades that occurred between Zacharias's abandonment of the experiment and the successful implementation in 1989 by a group from Stanford University and IBM using laser-cooled atoms.[1] As we noted, the original experiment had been de-

continued on page 97

signed to use Norman Ramsey's separated oscillating field method in a vertical atomic beam magnetic resonance apparatus. Gravity would slow and stop atoms that started upward with velocities at the low end of a thermal distribution, and the increased interaction time would yield a very narrow resonance. One Ramsey RF transition was to be on the ascending atoms, the second on the descending ones.

The successful work carried out by the Stanford–IBM group was in fact a variation on this experiment, as it relied instead on two RF pulses on these two sets of atoms while they were in the RF cavity. The realization of the Zacharias fountain in its original incarnation with two separated continuous-wave excitations, also with cooled atoms, was finally achieved two years later, in 1991, by researchers from the Laboratoire Primaire du Temps et Fréquences, Laboratoire Kastler-Brossel and Laboratoire Aimé Cotton in France and the National Institute of Standards and Technology in the US.[2]

References

1. M. A. Kasevich, E. Riis, S. Chu, R. G. DeVoe, Phys. Rev. Lett. **63**, 612 (1989).
2. A. Clairon, C. Salomon, S. Guellati, W. D. Phillips, Europhys. Lett. **16**, 165 (1991).

Robert A. Naumann
(robert.a.naumann@dartmouth.edu)
Dartmouth College
Hanover, New Hampshire
H. Henry Stroke
(henry.stroke@cern.ch)
New York University
New York, New York
CERN
Geneva, Switzerland

Peer Instruction Can Work, Memorization Needs to Be Improved

I would like to follow up on Robert Jones's letter to the editor (September 1997, page 103) commenting on peer instruction, memorization and related issues.

His criticism of using peer tutoring as a means of achieving student understanding is accurate as far as it goes. Peer mistutoring is well documented in the educational literature. However, the literature overwhelming shows that lectures, demonstrations and cookbook labs cannot dispel misconceptions in an extremely large number of gifted students. It also shows that many misconceptions are so resistant to change that doubling the number of lectures, demonstrations and problem solving activities usually has little positive effect. Demonstrations, in particular, can be counterproductive, especially when students claim to see something different from what the instructor sees. Misconceptions can blind them to actual outcomes. Of course, all teaching involves some degree of risk that students will pick up misconceptions. In some cases, unfortunately, misconceptions are reinforced by conventional instruction.

I disagree with Jones's reservations about Eric Mazur's *Peer Instruction: A User's Manual*. The book, one of the most significant texts on teaching physics, has some very specific instructions on how to use this technique. One of Mazur's points is that the technique works best when about 50% of the students initially get the correct answer to a question before discussion with their peers. It does not work well when a small percentage initially get the answer, and it is useless if a large percentage get the answer. Essentially, guided peer instruction works, while unguided peer instruction may not. The use of peer instruction has been well documented in the literature, and it works much better than other techniques in dispelling misconceptions.

Basically, Mazur has successfully adapted the idea of peer tutoring to the large lecture hall. By providing a comprehensive manual on how to use this method, he has given physics teachers a tool that could make a significant difference to physics education, in that it is likely to increase students' understanding of and enthusiasm for physics.

Jones's concern about students' lack of memorization skills is pointed and accurate, especially at the high school level. Students are well trained to memorize material for the next test, and then forget it immediately. Factors contributing to this sorry situation include use of short (two-week) units with little review in subsequent units, lack of cumulative final exams at the end of the school year and an overall decrease in emphasis on drill and practice in the lower grades. In addition, high school students tend to treat learning in an adult manner by simply looking up what they need to know and as they need to use it. Unfortunately, this attitude creates a low knowledge base that hampers students later on. Also, they are taught that formulas are merely information to be memorized rather than concepts to be mastered. Clearly, this situation needs to be improved. One easily implemented change would be to require that physics teachers make it clear from the first day of class that their students need to both memorize certain facts and also acquire an understanding of the basic concepts that underlie those facts.

One final point: In my experience, hardly any physics instructors read the educational literature, and those who do, alas, tend to disbelieve the research results. I think that many have their own preconceived notions about education, and they find it difficult to change them. In this sense, they have much in common with physics students taking introductory courses.

John M. Clement
(clement@hal-pc.org)
Bellaire, Texas

Breakthroughs Recalled on Transistor Precursors in Germany, France

There is not much one can add to the story of the brilliant performance of John Bardeen, William Shockley and Walter Brattain that led to the development of the transistor and the subsequent birth of the information age. As chronicled in the December 1997 issue of PHYSICS TODAY (see Ian Ross's article, page 34, and Michael Riordan and Lillian Hoddeson's article, page 42) and elsewhere,[1] their broad and sophisticated research was initiated in 1945 at Bell Laboratories under Mervin Kelly, and it culminated in the most spectacular breakthrough in the newly established area of solid-state physics.

It is also instructive, I think, to take a brief look at certain precursor efforts—namely, the European development of the crystal rectifier in connection with the development of radar during World War II. The story of the crystal rectifier reflects the fact that basic technical advances require a certain period of gestation and that breakthroughs occur when the technical effort is driven sufficiently by a particular need—in this case the demand for radar receivers in the ultrahigh-frequency range (centimeter wavelengths). In the later war years, German and Allied researchers engaged in an intense race to become the first to achieve higher-frequency operation of airborne radar sets. As Heraclitus said, "War is the father of all things."

I worked at Telefunken's research laboratories in Germany throughout