receive this acknowledgment from the scientific and engineering communities.

Ralph F. Wuerker
(rwuerker@roadrunner.com)
Westlake Village, California

The account of ruby laser activities at Bell Labs in 1960 was fascinating. During the same period, the group working under Charles Townes at Columbia University's Radiation Lab (CRL) was pursuing a different, and ultimately failed, track to the first laser.

Herman Cummins and I were the graduate students assigned to make the IR continuous-wave potassium laser and, later, the cesium vapor laser outlined in the 1958 paper Townes wrote with Arthur Schawlow.[1] Townes brought in Oliver Heavens, a thin-film expert from Royal Holloway College in the UK, to help us produce our own reflecting films. We began in 1958, but we had difficulty pumping potassium with a high-pressure mercury lamp at 404.7 nm, so we turned to cesium vapor pumped with a helium discharge tube at 388.8 nm. We used an elliptical reflecting cylinder and tried sapphire tubing and sapphire windows to avoid cesium darkening in glass.

By June 1960 we had measured gain but no laser oscillation. Heavens attended the June spectroscopy meeting in Rochester, New York, and gave an overly optimistic progress report of the cesium work. We later learned that Malcolm Stitch from Hughes Research Laboratories was at the meeting, and thus the Hughes group heard that the Columbia group was "about to succeed." That probably helped stampede Maiman into the now famous press conference. But the cesium laser never did work at Columbia.

I learned of the Maiman ruby laser through a telephone call from Walter Sullivan, then science editor of the *New York Times*. He often called Townes, who that day was in Washington, DC, so I took the call. Depressed, I thought, "There goes my PhD thesis." Townes called me the next day, and I was given an open account to spend what it took to make a ruby laser. Bill Rose at CRL had a pink ruby boule for use as a maser amplifier in his radio telescope. We cut and polished a slice for my laser and evaporated silver reflective films on the rod ends. The lab bought the flashtubes, storage capacitors, and power supply. My laser worked in about two months, just before Labor Day.

We published two papers' worth of work, enough for a thesis.[2,3] Then Sven Hartmann, Norm Kurnit, and I did the first experiment that observed photon echoes in ruby.[4]

In retrospect, Townes made the correct decision to abandon the cesium laser project.

References

1. A. L. Schawlow, C. H. Townes, *Phys. Rev.* **112**, 1940 (1958).
2. I. D. Abella, H. Z. Cummins, *J. Appl. Phys.* **32**, 1177 (1961).
3. I. D. Abella, *Phys. Rev. Lett.* **9**, 453 (1962).
4. N. A. Kurnit, I. D. Abella, S. R. Hartmann, *Phys. Rev. Lett.* **13**, 567 (1964).

Isaac D. Abella
(asea@uchicago.edu)
University of Chicago
Chicago, Illinois

It was disturbing and disappointing to read "Bell Labs and the Ruby Laser." Although the article adequately describes the authors' views of the events of 1960, it can be interpreted as casting a shadow over Theodore Maiman's seminal achievement. That is unfortunate, since it amounts to stirring up the old controversy. The events of 1960 have been discussed extensively, and all the key issues and questions have been addressed and, in all essentials, settled. I certainly do not wish to engage in further polemics and revisit all the old arguments. Of course, there can be no objections to the authors' desire to share their views, as Maiman did in his book, *The Laser Odyssey* (Laser Press, 2000). However, I think that presenting a one-sided report during this anniversary year when we are celebrating the remarkable achievements of 1960 (including those of the Bell Labs laser team) is inappropriate, tactless, and not in especially good taste. The sooner we put all this behind us, the better.

Viktor Evtuhov
(viktor.e@verizon.net)
Pacific Palisades, California

The feature article "Bell Labs and the Ruby Laser" is marvelous. As a brand new assistant professor at the time, I learned of the events described via *Physical Review Letters* and the physics grapevine. It was an exciting time. And the article demonstrates how messy the progress of physics can be. Physics is done by human beings, not automatons.

I cannot resist noting one unmentioned aspect of the times. In all the laboratory pictures, the physicists at work were wearing dress shirts and neckties. That might have been the Bell Labs dress code, but I don't think so. It's a superficial indicator of how times have changed.

Donald N. Langenberg
(dnl@usmd.edu)
University System of Maryland
Baltimore

The paper "Who Invented the Laser: An Analysis of the Early Patents," by Robert Myers and Richard Dixon,[1] is a thorough scholarly assessment of laser history. It will be of considerable interest to readers of "Bell Labs and the Ruby Laser."

Reference

1. R. A. Myers, R. W. Dixon, *Hist. Stud. Phys. Biol. Sci.* **34**, 115 (2003).

William Joyce
(wbj@mailaps.org)
Millington, New Jersey

Nelson, Collins, and Kaiser reply: Apart from the enjoyment of three old men looking back at their youthful efforts of 50 years ago, our purpose in writing "Bell Labs and the Ruby Laser" was to describe how our efforts came about and were carried out, so that we might bring "a modicum of peace" to the controversy that has gone on far too long. We wished to make it clear that we gave Theodore Maiman credit for the key ideas of the ruby laser—and we listed them in our article—by citing official, printed Bell Labs statements of 1960 and a later history of Bell Labs.

Jeff Hecht was initially offended by an errant phrase in our article's first paragraph: "led to the creation of the first ruby laser." As stated in the editorial note above, that phrase was not in our submitted manuscript, and we apologize for our poor proofreading of the editor's reworking of it. We disavow the phrase and are pleased that Hecht is willing to put it behind all of us.

However, he still does not want to give us the credit for the first publication of attaining threshold with the accompanying pencil beam and relaxation oscillations because he does not appreciate that scientific credit is based on publication in peer-reviewed journals. Instead he wishes to base judgment on unpublished observations, personal conversations, and much delayed claims. That approach forces us to be more specific in quoting the record than we were in our search for "a modicum of peace." In his 2000 book,[1] Maiman states in a footnote on page 150, "Before going into publication of this manuscript, I hooked up the original laser, which I still retain. Of course, as always, I used a non rod-like 'stubby ruby' (about 9 × 18 mm). That laser still works. It meets the Townes criteria of a red spot on the wall. It meets the Nelson specification of 'spiking behavior.' *And, even meets the Maiman demand of effortlessly boring a hole in a Gillette razor blade!*" Those claims are in sharp

continued on page 59

continued from page 10

contrast to his 1961 report[2] that the crystals described in his two 1960 papers "exhibited R_1 line narrowing of only 4 or 5 times, a faster but smooth time decay of the output (compared to the fluorescence), an output beam angle of about 1 rad, and no clear-cut evidence of a threshold excitation." Those two statements are in direct contradiction. Should we believe the 1961 contemporaneous statement, participated in by four coauthors and published in a respected, refereed journal, or his own claim 40 years later in his self-published book? Hecht seems to subscribe to the latter.

Hecht's criticism that "the Bell group should have heeded [Amnon] Yariv's report" is apparently based on the mistaken belief that everyone at Bell Labs knew what every other researcher did and said. Bell Labs had close to 20 000 employees at several different locations. Yariv was at the Holmdel location, we were at the Murray Hill lab. Our management chains were different, and joined only with the vice president of research, four levels up. None of us had ever heard of Yariv's remarks until Hecht cited Yariv's 2000 memoir in his letter.

Hecht repeats Maiman's claim that in a phone call of August 1960 he told Collins he had a pencil beam. Collins is firm in his memory of no such claim by Maiman, and neither of us have any memory of Collins telling us that; it is something we would not forget.

We are pleased that Hecht has offered a figure as his "proof" of Maiman's early observation of "spiking" (that is, relaxation oscillations) and threshold because to the eye of a scientist the figure proves the contrary of each. The undated photo with no time scale shown (probably 0.5 or 1 ms per division) purports to show spiking. But notice that the raggedness begins before the abrupt rise that shows the start of stimulated emission. Notice further that the raggedness does not stop abruptly but continues as the decay falls below the level where the stimulated emission began. The raggedness is obviously noise of some kind, not relaxation oscillations. Further, the figure shows an exponential decay, not an abrupt drop that would occur if threshold had been exceeded. To see how threshold dramatically begins and ends (a rise and fall of a factor of 1000) and how relaxation oscillations appear, see figure 1 of our 1960 letter.[3]

Ralph Wuerker's remark that Bell Labs researchers were thinking of continuous operation of a laser apparently is based on Yariv's memoir. That doesn't jibe with our memories. Our thinking was directed at finding a four-level system for lasing. Maiman's most creative contribution was to believe he could adequately empty the ground state of a three-level system, like pink ruby, to make a laser.

The Myers–Dixon analysis of maser and laser patents, mentioned by William Joyce, is a fine exposition that we recommend, particularly to readers of Nick Taylor's book.[4] And Donald Langenberg's comments remind us that there was no dress code whatsoever for Bell Labs researchers, even for publicity shots. But dressing well in that era was regarded as a politeness to the people around us.

References

1. T. Maiman, *The Laser Odyssey*, Laser Press, Blaine, WA (2000).
2. T. H. Maiman, R. H. Hoskins, I. J. D'Haenens, C. K. Asawa, V. Evtuhov, *Phys. Rev.* **123**, 1151 (1961).
3. R. J. Collins, D. F. Nelson, A. L. Schawlow, W. Bond, C. G. B. Garrett, *Phys. Rev. Lett.* **5**, 303 (1960).
4. N. Taylor, *Laser: The Inventor, the Nobel Laureate, and the Thirty-Year Patent War*, iUniverse, Lincoln, NE (2007).

Donald F. Nelson
(dfnelson@charter.net)
Worcester, Massachusetts
Robert Collins
Bethesda, Maryland
Wolfgang Kaiser
Munich, Germany

Remembrances of Dirac

In his fascinating article (PHYSICS TODAY, November 2009, page 46), Graham Farmelo mentions Paul Dirac's training as an engineer and the effect it had on his thinking. But the article barely touches on one of Dirac's little-known practical accomplishments. Farmelo mentions that during World War II Dirac spent "part of the time developing an idea he had conceived for separating isotopes using an apparatus with no moving parts." In fact, Dirac developed the fundamental theory of separation processes that underlies the design and analysis of today's uranium enrichment plants, which provide fuel for almost all of the world's nuclear power plants.

Dirac introduced the central concepts of separative work, which measures the effort needed to effect a prescribed separation. It is based on assigning to each isotopic concentration x a value $V(x) = (2x - 1)\ln(x/(1-x))$, the derivation of which involved a bit of Dirac magic. The results can be counterintuitive for the uninitiated. For example, it takes much less separative work to extract bomb-grade uranium (say, 90% ^{235}U) from a batch of low enriched uranium, such as that typically used for power reactor fuel (say, 5% ^{235}U), than it does to extract that amount of low enriched uranium from natural uranium (0.7% ^{235}U). Physicists accustomed to associating Dirac with an ethereal approach to quantum electrodynamics may find it difficult to imagine him setting the theoretical foundation for an important industrial process. But he did.

Victor Gilinsky
(victor@gilinsky.com)
Santa Monica, California

Graham Farmelo cites Paul Dirac's 1939 speculations on the role of quantum jumps in primordial cosmology as an example of the man's deep insights into physics. However, I think that is not a good example; Dirac probably was aware of the writings of Georges Lemaître, the first physical cosmologist, who by then had gained significant prominence. Most likely the credit for that insight should go to Lemaître, who published an amazing 1931 letter titled "The Beginning of the World from the Point of View of Quantum Theory."[1] In it he wrote, "Clearly the initial quantum could not conceal in itself the whole course of evolution; but, according to the principle of indeterminacy, that is not necessary." It is remarkable how close Lemaître's concept was to the contemporary notion of cosmology, in which the structure of the universe today arose from initial quantum fluctuations of a perfectly homogeneous, primordial vacuum.

Reference

1. G. Lemaître, *Nature* **127**, 706 (1931).

Ted Jacobson
(jacobson@umd.edu)
University of Maryland, College Park

I enjoyed Graham Farmelo's recent article on Paul Dirac—and his book, *The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom* (Basic Books, 2009; reviewed in PHYSICS TODAY, December 2009, page 52). Jeremy Bernstein has also recently written an interesting piece about Dirac.[1]

My experience is that Dirac was concise rather than reticent, and always