continued from page 15

eyes beaming, he walked to the blackboard and we continued working on the left part, which, after several intermediate erasures, was full at the end of the discussion. The photo shows that most of what we wrote on that half of the blackboard was later erased and replaced by two scattering-like drawings, which may or may not represent Feynman's later thinking on the spin problem. But his writing on the two-particle-type probability densities and the corresponding measuring tool densities, as well as his equation for g on the top right, is mostly still there. I had not named the two types and used subscripts I and II in the exhibited formulas. Feynman said to just call them A and B. His use of this notation can be seen above the left "scattering" drawing.

It is inappropriate for me to explain the new model of spin types here (it has been further disseminated and is to be published), except to mention that Feynman's two lectures in 1980 and 1982 at Caltech on using negative probabilities to understand the EPR paradox inspired me to work on this well-known fundamental problem. He had allowed me quite a few privileged discussions over the years, beginning in 1966 with my taking his advanced quantum mechanics course at Caltech, and including several occasions since 1980 on which I sought to specifically understand the EPR problem. But this last occasion was the only such opportunity I had in his last two years.

Knowing that he had been struggling with cancer, I found it a great inspiration to see his usual healthy clear and quick mind and high spirits during the discussion. He probed the key notion of independent reciprocal probability densities profoundly. He was very pleased with and encouraging of the new idea. He asked me to write it up and see him again as soon as possible. And he promised to think more about it. I immediately shared this inspiration with several colleagues. But I did not know, and only learned retrospectively from his secretary, Helen Tuck, that he was quite ill even then.

In about two weeks I wrote the draft up and brought it to his office. He was ill and not in. I left it with Mrs. Tuck. He never saw it.

Feynman's last blackboards speak of his generosity to others and his unceasing quest for scientific truth. What the great teacher taught, we will carry on.

Charles Tse Chin Mo
R&D Associates
Marina del Rey, California

3/89

Lessons Feynman Taught Them—and US

Here is a footnote to David L. Goodstein's article "Richard P. Feynman, Teacher" (February, page 70). Goodstein mentions Feynman's informal course Physics X and notes that he cannot discuss its contents, as faculty were excluded.

I graduated from Caltech in 1962 with a BS in physics, and attended Physics X regularly for about two years. In a sense this was the best part of my undergraduate education.

In my time Physics X was conducted on Thursdays at 11:00 am because that was a reserved, class-free hour (in case a campus-wide meeting had to be called). Feynman began by asking whether anyone had an interesting physics problem. If someone did (and we spent lots of time trying to think up questions that could challenge him—I think that was one of his motivations) Feynman would work it out *before our eyes*. If no one had a good problem he always had one of his own. I have no idea whether Feynman cheated by thinking about his own questions before the lecture, or whether he attacked them as extemporaneously as he answered questions from the floor. I suspect the latter because he was always scrupulously honest.

Physics X let us students see how a great physicist thought. But imagine what breadth of knowledge and rapidity of analysis were necessary to put on such performances, week after week. I know of no other scientist crazy enough even to try such a thing, much less pull it off.

Two incidents, recalled here to the best of my ability, summarize the impact of Physics X on my career.

▷ Tommy Lauritsen, of lamented memory, taught a course in the physics of matter (atomic physics, solid state, plasmas, a little of everything). The exams were always closed book, an anomaly at Caltech in that era. Lauritsen believed in memorization, to the dismay of the students, including me. So when Robert Bacher (my adviser) asked how I liked the course, I complained about the memorization (which, according to student lore, was fit only for trolls—Caltech-ese for "nerds"—and organic chemists—but I repeat myself).

Bacher admonished me in the following terms: "A physicist has to know how big things are, so he has to know the physical constants by heart. He has to know, as naturally as breathing, the tools of his trade such as Maxwell's equations, the Schrödinger equation and so on. You have

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been attending Physics X. *Did you ever see Dick Feynman fumble for a formula or a physical constant?"*

In other words, Bacher, a sly devil, used my hero-worship of Feynman to shame me to my marrow. I might say that Lauritsen's course was the making of me as a physicist, but I would not have taken it seriously had Bacher not known how to hit me where I lived.

▷ One Physics X meeting, after the spectacular televised failure of the latest Vanguard rocket, someone from Dabney House (Caltech alumni will know what *that* means) asked why the Russians could apparently put up any number of satellites, while we could only fumble. Feynman, honest as usual, said, "I don't know"! But then he added that "in such a large audience surely someone must know something about this."

Since no one else wanted to speak, I eventually stuck up my hand and, drawing on my experience on the assembly line at Grumman Aircraft (plus some model aircraft, rocketry and electronics experiences in my checkered past), discoursed for perhaps five minutes on reliability, the level of quality control in airframe construction, what happens when metal dust gets into either electronics (in those days, vacuum tubes and relays) or turbine bearings, contrasting a rocket design with no safety margin (the Vanguards) with the overbuilt Russian rockets and so on. This answered the question to everyone's satisfaction, and Feynman, after thanking me, went on to discuss something more interesting—either Mach's principle or how an H-bomb works; I forget which.

The aftermath was also interesting: At the end of the hour, he beckoned me to come down to the podium. I couldn't believe he meant *me*. He said, "What do you say we go for a walk around the campus and chat?" Now it must be understood that I was a growing boy, skinny at that time (hard as it is to believe now), and famishing for lunch. I was also looking forward to double ice cream sundaes that Blacker House had won from Rickets in some athletic contest. So you will see how he mesmerized me: I agreed without a murmur!

We walked twice around the periphery of Caltech, and in that hour or so, after some polite inquiries as to my name, studies and the like, Feynman sucked my brain dry (that is the only way I can describe the process) of every fact it contained about how aircraft are built, including how the construction is managed, how mistakes get corrected, the actual processes of construction and so forth, in detail that I had no notion I possessed. At the end of the debriefing, I was *really* hungry and my brain was numb. Somehow I got through the afternoon's labs without quite electrocuting myself, but I literally could not think clearly for at least a day afterward. The only experience I can compare it to is staring too long at the Sun—the details fade away, but the dazzle spots remain.

On an entirely different subject, I find it remarkable that the Feynman issue—containing articles that could be called "Feynman, Teacher," "Feynman, Artist," "Feynman, Computer Jock" and so on—had no article "Feynman, Writer" (although several of the articles made mention of his prose). His extremely readable autobiographical books are hardly anomalies. Feynman's technical prose was the best in the business—spare, clear and to the point. I still use the little paper by Feynman and G. Speisman (*Physical Review* **94**, 500, 1954) as an example for my students of what technical writing can aspire to. Like any master, he made it seem easy. But anyone who has tried knows how much effort this kind of effortlessness requires.

Julian V. Noble
University of Virginia
3/89 *Charlottesville, Virginia*

Graduating from the University of Illinois in 1960, I wanted to do my graduate work at Caltech because Richard Feynman and Murray Gell-Mann were there. They did not disappoint me! A recent visit to Caltech stirred up some memories of Feynman.

One day during afternoon tea in Bridge Laboratory at Caltech, Feynman was talking to a group of graduate students. He asked us what research we were working on, and after several responses he said: "Doesn't it bother you knowing that others are working on the same problems using the same approaches? I can't work on something unless I believe that I am doing it in a different way from everybody else."

In February of 1962 I went to see Feynman in his office in Bridge to schedule my PhD qualifying exam. (In those days the only exam was a two-to-three-hour oral exam covering everything.) I was accompanied by a former PhD student of Feynman's (one of the few) who was now working at the Rand Corporation, where I had met him the previous summer. He was a cocky, independent type who clearly looked to Feynman as a model of how to be in the world. Feynman was in a good mood. He agreed to be on my committee, but said that his wife was expecting their (first) child, so there was a chance that he might miss my exam. (He didn't; their son was born several weeks before the exam.) While I was in his office, Feynman jokingly gave me a quick problem: to integrate the Gaussian $\exp(-ax^2)$. I could do it.

During my qualifying exam Feynman asked several unusual questions. One that I recall was "How would you express the basic new idea in quantum field theory to a group of kindergarten students?" I didn't give the answer he was looking for—the possibility of particle creation and annihilation. He also asked me something about clouds and radiation from the Sun, which involved the Stefan–Boltzmann σT^4 law.

Feynman scared off most potential PhD students. One fellow in my class supposedly left Caltech after a conversation with Feynman. It took someone confident, tough and loud to stand up to him. And you had to bring him the idea for your research; he didn't have a collection of problems to hand out to students. I didn't get the impression of someone with the desire to be a mentor or adviser.

The word among the faculty and postdocs was that the trick was to get Feynman *interested* in a problem. Then he would figure out a way to solve it. He was known as a tenacious, ingenious problem solver and calculator.

He said that he never read the physics literature. He had little patience to do it. He got others to give him the basic ideas and facts, and then he would work the rest out for himself if it was interesting.

The impression he gave was of someone whose attitude was either "Tell me something that will intrigue me" or "I will entertain you." There was not much sense of his wanting normal interaction and contact. Yet he clearly needed others around him. All of his books began as oral projects.

He loved an audience. It brought out his Brooklyn accent and his brashness; he swaggered; he grew louder. He wanted to entertain, amuse, arouse. His lecture style was informal, conversational and physical. He talked with his whole body; his hands were very expressive and graceful.

In those days, I remember, he dressed typically in pretty good slacks (often the beltless type), with a white shirt open at the collar, and gray Hush Puppies on his feet. I picture him walking and talking loudly, taking big steps, with his feet turned outward.

He could really be obnoxious at a seminar. He sat in the front row with his feet up on the table and was perfectly willing to insult the speaker if he was saying something wrong, uninteresting or trivial.

He hated pomposity, empty rhetoric, "phony" problems and phony people. He loved all parts of science; he was not a snob who believed that some areas are more worthy of respect than others.

One day at lunch in Chandler dining hall he described how he recently had found several $100 bills in a manila folder with some research work in it. He realized that this dated from one of his trips to Las Vegas, where he typically sat in the lounges of the big hotels working on physics for many hours. To hide some of the cash he had with him, he would tuck it into his papers.

I found him fascinating and exciting, but he had a less appealing side too. At times he seemed inconsiderate, crude and even unkind. I felt his attitude toward philosophy, psychology and other humanistic areas was based largely on ignorance. He could demonstrate the brash arrogance of a spoiled child. Perhaps his extraordinary success in physics meant that he never had to learn to compromise, really take others into account, or reevaluate his beliefs; he could be insensitive or unaware of how sensitive others can be, or—more likely—just unwilling to let that affect his behavior.

We admired his brilliance and especially his originality, his spontaneity, his directness, his wildness, his independence. His (implied) message was: Be yourself, be adventurous, enjoy yourself in your work (and elsewhere in your life), don't trust something until you have thought it through for yourself, and don't be too philosophical or "serious."

Although I had seen him only a few times since the 1960s, I was surprisingly touched by the news of his death. He was such a heroic presence, so much fun to be around, such a unique character. He appealed to the free, smart, excitable, noisy kid inside each of us—and when he died, there was no one to take his place.

HARVEY K. SHEPARD
University of New Hampshire
4/89 *Durham, New Hampshire*

Bravo on your February special issue on Richard Feynman. His passing was a loss to many, and it was my loss never to have met him. But through his later, autobiographical publications, we have been given a look at

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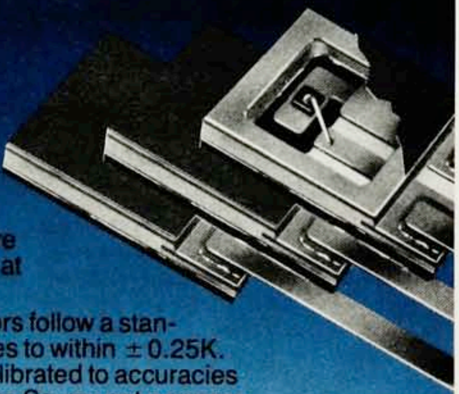

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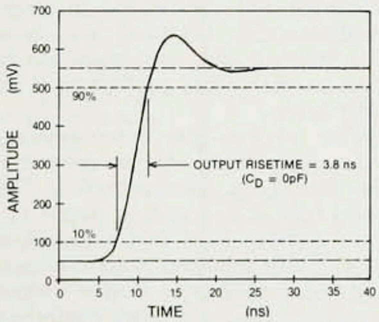

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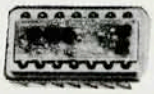

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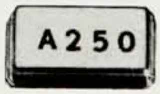

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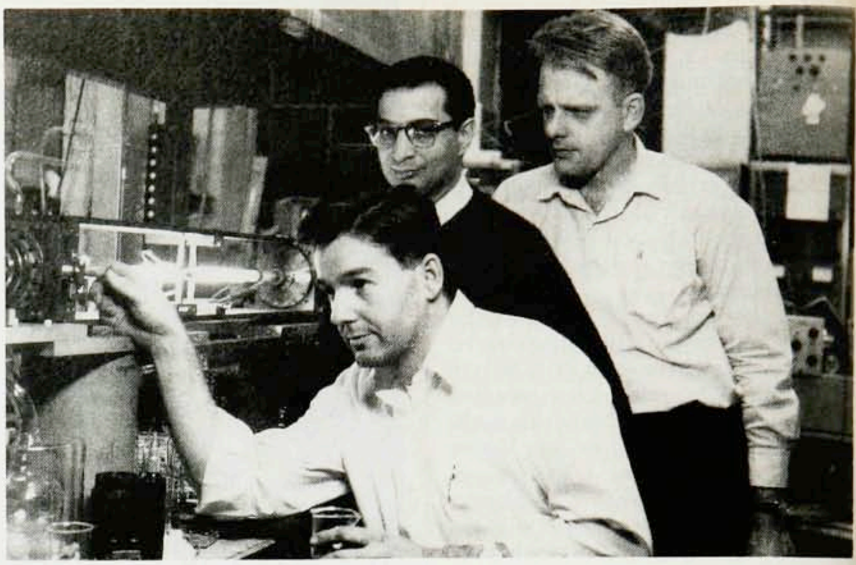
this loving, caring and witty person that goes beyond his physics.

His was a rare gift of being able to teach the most technical subject in a manner that not only could be understood by anyone but would motivate them to learn more. More people should have come to know about him during the Challenger commission hearings. His notes published in the February 1988 issue of PHYSICS TODAY (page 26) and in his book *What Do You Care What Other People Think?* were worth far more than the entire commission report. One can only hope that the report will have a positive effect on NASA management, although NASA's ills are not unique but are symptomatic of many large American companies supported by the government and the Department of Defense.

Thank you Richard Feynman. Your spirit and work will continue to inspire and live on beyond us all.

RICHARD A. COX
2/89 *Saratoga, California*

The First He:Ne Laser: An Amplification



There was one minor flaw in Joan Bromberg's carefully written article on the early development of the laser (October 1988, page 26): The date ("early in 1960") given for the photograph of the first helium–neon laser on page 33 is off by nearly a year. That particular laser was first assembled in the lab I shared with Ali Javan toward the end of the summer of 1960 and was tried in various forms throughout that fall. The first version involved a water-cooled microwave discharge coupled to an S-band magnetron—which turned out to melt the discharge tube. The second version had a quartz electrodeless rf discharge tube of the type that eventually worked. However, the high-reflectance mirror coatings flaked off the mirrors during bake-out in our vacuum system.

No one was interested in photographing the device until after it had oscillated (in its third version) on 13 December 1960. I have enclosed a copy of a Polaroid snapshot that was taken on that occasion, showing the laser in its original environment. From left to right are Donald R. Herriott, Javan and myself. The location was the lab in which we took all of the basic data reported in our first article.[1] In the background, behind the large bake-out oven, are three other experiments: ones in which Ali and I measured the He^*–Ne excitation transfer cross section and detected gain in the helium–neon system early in 1960; and a third that I had put together to measure lifetimes of the pertinent neon levels.[2] Don Herriott had designed the Fabry–Perot structure and is shown adjusting one of the micrometer screws with one hand while holding a beaker of potable liquid in the other—a substance that we all shared to celebrate the event. The Bell Labs publicity department evidently felt the original environment was too cluttered, and the laser was moved to the more pristine environment of Herriott's lab during January 1961 for a series of artificially posed photographs—one of which was reproduced in Bromberg's article. While it was there, Don Herriott took some more careful data on the optical properties of the laser beam.[3]

The original gas laser survived three more trips: At the request of the Bell Labs administration, Don and I took it in a station wagon to a hotel room in New York City, where we actually managed to get it aligned and oscillating on a rather rickety wooden table for a press conference held on 1 February 1961. The mirrors had to be aligned within approximately 1 arcsec; people used to high-gain confocal lasers have no idea what was involved in getting a plane-parallel system to oscillate with approximately 1% gain per pass. That laser survived the return trip to Herriott's lab and then one more back to Ali's lab, where one of the Kovar-to-glass seals was broken.

The second successful gas laser oscillated in April 1961 and consisted of a more stable four-rod structure that I designed to study mode pulling and spectral hole-burning effects.[4] It is currently in my lab at Yale and still oscillates. The third was an all-quartz design by Herriott, which ultimately became an exhibit at the Smithsonian.

Almost a year went by before anyone outside of Bell Labs succeeded in making an oscillating cw helium–neon laser. As I recall, that was accomplished by Roy Paananen at Raytheon and not by any of the people who later collected royalties on our invention.[5]

References

1. A. Javan, W. R. Bennett Jr, D. R. Herriott, Phys. Rev. Lett. **6**, 106 (1961).
2. W. R. Bennett Jr, in *Advances in Quantum Electronics*, J. R. Singer, ed., Columbia U. P., New York (1961), p. 28.
3. D. R. Herriott, in *Advances in Quantum Electronics*, J. R. Singer, ed., Columbia U. P., New York (1961), p. 44.
4. W. R. Bennett Jr, Phys. Rev. **126**, 580 (1962).
5. W. R. Bennett Jr, A. Javan, US Patent 3149290, 15 September 1964. A. Javan, W. R. Bennett Jr, D. R. Herriott, US Patent 3614653, 19 October 1971.

WILLIAM R. BENNETT JR
Yale University
10/88 *New Haven, Connecticut*

Corrections

March, page 21—The quotation from Alex Malozemoff should have referred to "the large *critical* current densities obtained in thin films."

March, page 64—Stephen R. Leone is still employed by the National Institute of Standards and Technology (formerly the National Bureau of Standards). He works in the division that is part of the Joint Institute for Laboratory Astrophysics at the University of Colorado, Boulder. He is also an adjoint professor in the university's chemistry department and a lecturer in the physics department. ■