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.

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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.

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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.

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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