

Invention of the Injection Laser at IBM

MARSHALL I. NATHAN, FELLOW, IEEE

(Invited Paper)

Abstract—The events leading up to the observation of stimulated emission from GaAs p-n junctions at the IBM T. J. Watson Research Laboratory in 1962 are recounted, and the subsequent occurrences which culminated with the making of a GaAs laser are described.

IN THIS paper, the events that led up to the observation of stimulated emission of radiation in GaAs and the subsequent making of an injection laser at IBM will be described. To some extent, this has already been done in several papers [1]–[4]. These articles will be drawn upon heavily, in particular [2], in which I wrote a short section on this subject. My intention is to give a personal account of what influenced me and what my colleagues and I did in making the semiconductor laser.

The early 1960's were an exciting time for people working in solid-state physics, and especially semiconductor physics research. The invention of the transistor had fostered the belief that there would be many solid-state devices which could further revolutionize the computer and communications industries. Indeed, we had seen the invention of the cryotron [5] and the tunnel diode [6], and were looking for more. In 1958, Schawlow and Townes [7] published a seminal paper describing the problems and the techniques for possibly overcoming the difficulties involved in the extension of maser action from the microwave region of the electromagnetic spectrum into the infrared and visible frequency range. In 1960, Maiman [8] made the first laser using the energy levels of the Cr^{+3} ion in Al_2O_3 . (This is a three-level laser, where the final state is the ground state of the system, which is occupied in equilibrium.) Soon afterwards, Sorokin and Stevenson [9] used the energy levels of U^{+3} and Sm^{+2} in CaF_2 to make four-level lasers, which have a lower threshold power for lasing because the final state is unoccupied. These were the first and second lasers to be realized after Maiman's accomplishment, and they assured the world that the ruby laser was not just a remarkable coincidence but hopefully the first of many.

There was much talk in the early 1960's about making a laser in a semiconductor. At the time, people who I heard discuss it or was told at the time had discussed it at scientific conferences were Pierre Aigran and Ben Lax. There were others of whom I was not aware at the time.

Manuscript received November 10, 1986.

The author is with the IBM Thomas J. Watson Research Center, Yorktown Heights, NY 10598.

IEEE Log Number 8714078.

I did not take these suggestions very seriously. There was one person at our laboratory who did take the possibility of a semiconductor laser seriously and that person was Rolf Landauer, our department director at the time. He induced Gordon Lasher to think about the problem and to do some calculations. Gordon [10] pointed out that diffraction loss would be a major loss mechanism. Independently, Bill Dumke [11] undertook the study of the problem. He realized that free-carrier absorption was important. Using his vast background in the optical properties of semiconductors, he calculated the optical gain in direct gap materials like GaAs and several other III-V compounds which would be sufficient to overcome this loss even though the gain was insufficient in indirect gap materials, such as Si and Ge. This was a very critical and important point because it turned our thinking and that of others away from the indirect materials which were technologically more advanced and would have been easier to deal with, to the direct materials. During the summer of 1961, informal conversations took place between Rolf and Bob Keyes, who was my manager at the time, and representatives of U.S. Army Electronics Research and Development Laboratory at Fort Mammoth, N.J., about injection lasers. Shortly afterward, IBM submitted a proposal for a contract written by Bob with a contribution from Gordon which was closely related to the latter's paper in the *IBM Journal of Research and Development*. This proposal had the goal of delivering an injection laser within one year, and it was funded by the Army. Its existence gives an indication of the serious thoughts of Rolf and others at the lab about injection lasers at the time. I was never asked to work on the contract. However, I would have refused to do so because at the time I regarded my scientific freedom as very important. To be constrained by being required to work on a contract that someone else had written would have been intolerable.

Rolf's influence during this period cannot be overstated. He was continually inducing people to think about semiconductor lasers. I did not take the whole business very seriously. Without his influence, many people including myself probably would have done nothing on lasers. About this time, the atmosphere in the laboratory was generally conducive to working on semiconductor lasers. In addition to Rolf's attitude, Peter Sorokin's success with CaF_2 gave credibility to the idea that lasers were a real thing, and we could make them. The contract with Fort Mammoth showed directly the interest of the management in injection lasers. At Yorktown, an effort on

GaAs had been started three years before for the purpose of making high-speed bipolar transistors. This effort was primarily a materials and physics effort—the devices and circuits had not gotten very far yet. However, Rick Dill had made some working bipolar GaAs transistors. There were several people working on GaAs crystal growth—Norm Ainslie, Sam Blum, Kurt Weiser, and Jerry Woodall. Transport and optical studies were being done by Dave Pettit, Warren Reece, Bill Turner, and Joe Woods. Gerry Burns and I were doing spectroscopy measurements of GaAs and insulator crystals. We were getting interesting results. For me, talk of a semiconductor laser was a distraction, even though an interesting one.

In January of 1962, Rolf invited Sumner Mayburg, his one-time Harvard roommate, to the laboratory to give a talk about electroluminescence from GaAs p-n junctions. Mayburg observed that most of the luminescence was in a sharp line near the energy gap. He made a claim that the luminescence had a quantum efficiency of one. His evidence for this was rather circumstantial. He observed that the light was linear with current after having been superlinear at low current, and he suggested that this meant that the nonradiative processes had saturated. He also said that at 77 K it was possible to see the diode despite the fact that it was radiating at 8400 Å. Mayburg's results seemed reasonable but less than totally convincing. Shortly afterward, he presented his work in a postdeadline paper at the March 1962 American Physical Society Meeting in Baltimore, MD. There, his results seemed to meet with a similar reaction. The fact of being able to see the diode was greeted with total skepticism. People said that it was probably the high-energy tail of the 8400 Å light. A few months later when we were making GaAs diodes it was easy to show with optical filters that Mayburg was in fact seeing the 8400 Å light.

Mayburg's results with some added prodding from Rolf made me think about our GaAs luminescence samples. In some of the samples from Ainslie, there were emission lines which were 100 times sharper than Mayburg's. It seemed as if they might be good candidates for lasing. I discussed the results with Peter Sorokin. I understood almost nothing about lasers, and he did not know much about semiconductors, so it was a difficult but interesting conversation. We decided that it would be worthwhile to look for lasing using Peter's apparatus. He had a helium dewar and pulsed flash lamp with a millisecond rise time which he had used for U^{+3} and Sm^{+2} in CaF_2 . We had a sample of high purity GaAs cut into the shape of a rectangular parallelepiped $2 \times 0.2 \times 0.2$ cm³ which we polished and etched lightly to remove the damage to the surface. The sample dimensions were comparable to those Peter had used for CaF_2 . The sample appeared to be too large since the gain in GaAs was expected to be much larger than in the ionic lasers, but no one seemed to know how to polish a small sample. We pulsed the sample with the flash lamp and observed the GaAs luminescence, but the wavelength shifted in time to longer wavelength and the intensity died out during the pulse as the sample was heated by the light from the flash lamp. The time con-

stants in the system were just too slow to see lasing or super radiance even if it had been observable given the nature of the sample and its geometry.

Other groups had difficulties similar to ours in conceiving how a semiconductor laser might work. Thomas and Hopfield [12] reported problems and prospects for overcoming them in making an optically pumped laser in ZnS. They concluded it was very difficult because of materials problems and the large optical absorption constant.

The next impetus to the semiconductor laser came in late June 1962. Besides Mayburg, other groups were working on light emission from GaAs p-n junctions, namely, Pankove and Massoulie at RCA [13], Nasledov and co-workers in the Soviet Union [14], and Keyes and Quist at Lincoln Laboratory [15]. In late June 1962, Keyes and Quist [15] reported at the IEEE Device Research Conference that recombination radiation from GaAs p-n junctions was 100 percent efficient. They measured the light output directly with a thermopile. It was hard to understand how the efficiency could really be 100 percent because of reabsorption. However, even if the results were off by a factor of 10 or 100, it was still the most efficient electroluminescence that had ever been measured. It gave credence to Mayburg's results and his claims about them.

No one from our group in Yorktown was at that conference. Our lack of attendance did not speak well of our sophistication in semiconductor devices. At the time, we were a relatively new laboratory. It did not matter that none of us were there, however. The *New York Times* reported the story the next day. Rolf brought the *Times* clipping to a semiconductor group meeting. He was very excited about the results. During a conversation we had several years later, he showed me a copy of a memorandum that he had written to IBM's upper management around this time in which he flatly predicted that an injection laser would be made within the next year.

Rolf asked Dick Rutz to start making GaAs p-n junctions. Dick and his people had been working for sometime on tunnel diodes in materials which included GaAs. Therefore, they had much of the technology on hand and were only too willing to comply. John Marinace prepared the wafers and diffused zinc into them. Rick Dill and Dick Rutz prepared the diodes and mounted them on headers. In the subsequent weeks, I took an interest in measuring the diodes. I measured the spectrum and verified that the visible light was 8400 Å light. It was similar to the photoluminescence that Burns and I had measured from p-type GaAs but different from n-type. Thus, it appeared the junction luminescence was from the p-side of the junction [16]. I measured the efficiency with a thermopile and found that it was only about 10 percent even with some very favorable assumptions about the true angular distribution of the radiation. Throughout all of this, Rolf maintained an interest and usually came to talk to me weekly. He wanted to hear only about the diodes. Whenever I started to talk about GaAs photoluminescence or worse, insulator photoluminescence, his eyes would glaze over or he might say something like "You're still working on that are you?" It was a bit heavy handed and was

somewhat of a gamble. I could have reacted negatively. However, I am very glad that he did it because my response was a positive one and I began to identify with making a laser.

We knew we were in a competitive situation. We knew that Lincoln Laboratory was also working on the problem of making a semiconductor laser. Ben Lax, an important figure at Lincoln Labs, was a public proponent of the laser; and Keyes and Quist's work suggested a promising direction. Other laboratories such as RCA and Sylvania also seemed likely to be working toward a laser.

At this time, I had no idea how to make an injection laser and no one else in the laboratory seemed to either. The problem was that we thought we had to have a resonant cavity, and we couldn't envision how to make one. An approach that circumvented this problem was taken by Peter Sorokin, Rick Dill, and Dick Rutz. They decided to try to use the GaAs p-n junctions to optically pump a rare-earth doped CaF_2 laser. The fact that rare-earth ion lasers have a low threshold since they are four-level lasers made this approach sound favorable. However, the threshold was not sufficiently low to overcome the low external quantum efficiency of the GaAs p-n junction. Peter, Rick, and Dick were unsuccessful in their attempt.

One day in August of 1962, Gordon Lasher and I discussed the possibility of detecting laser action or stimulated emission without having the light emitter in a resonant cavity. I asked Gordon how we would know that stimulated emission was occurring. Would there be threshold with an increase in light output and directionality of the light? The main question was what to look for. I kept pressing Gordon. Finally, he suggested that measured light output versus current would look superlinear. Perhaps there would be a weak threshold despite the fact that there was no cavity. Within the next few days, I measured the light output of several diodes at 77 K. My previous work had shown that the spectrum and quantum efficiency did not change between 4.2 and 77 K. The light was absolutely linear with current up to the range of 10^5 A/cm². I was disappointed but not surprised—nothing could be that easy. Shortly, I would be proven wrong.

I told Gordon of the result and also mentioned it to Rolf and Bill Dumke. I went back to photoluminescence studies having done my part. A few days later Bill came to see me in my lab. He had calculated that the electromagnetic modes in the spontaneous emission line must be highly excited given the high current densities, high internal efficiency and multiple internal reflections [16]. He liked the idea of looking for stimulated emission without cavity. If the gain is high enough, we should see it, but light output was the wrong quantity to have measured. He suggested that I look for spectral line narrowing. This is a characteristic of lasing, and he thought it would be a much more sensitive indication of the phenomenon. This, of course, was not a difficult experiment. However, I was not very enthusiastic because Gerry Burns and I were getting some interesting results, it seemed as if stimulated emission was not likely to be observable with these experiments. Nevertheless, I decided to try the experiment.

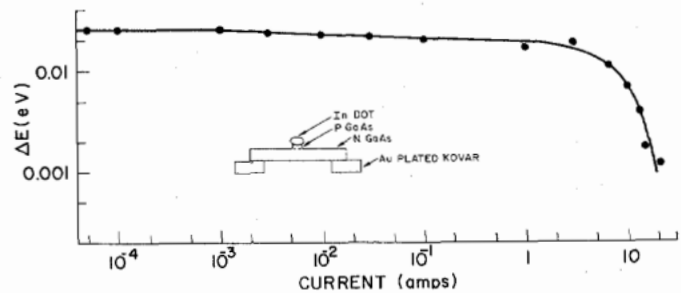


Fig. 1. Spectral linewidth versus current from a GaAs p-n junction at 77 K. Junction area was 1×10^{-4} cm².

I set the diode up in front of the spectrometer and used as a current source an old mercury delay line pulse generator that had previously been used for hot electron experiments. The pulse length was about one half a microsecond. As the current increased, the electroluminescence line began to shift to longer wavelengths because of heating. The pulse length had to be reduced. At about 3×10^3 A/cm², there was a small amount of spectral line narrowing—from about 120 Å to about 90 Å. I was incredulous! Then it occurred to me that it is probably some sort of statistical effect. By rights, the line width should have started out at $3/2 kT$ or 56 Å at 77 K if there were no band tailing. I decided arbitrarily to adopt a measured line width of kT or 38 Å as the criterion for a meaningful result and pushed on. As the current increased further, the line width decreased faster until it reached 30 Å (see Fig. 1). Fig. 2 shows the page from my notebook which shows the data for the first observation of line narrowing. As you can see, keeping good records is not one of my strong points. At this point, I stopped. I went over to Bill's office to show him the results. We were both ecstatic. We decided to go tell Rolf. However, it was almost 6:00 pm on Friday, and he had left for the day. We figured it was important enough to disturb him at home so we called and told him.

The next few days were a blur. Over the weekend, Gerry Burns and I went to the lab to confirm the result and to see how narrow a line we could get. We looked at several diodes. All of them showed line narrowing, but none of them narrowed to less than 30 Å. In the original diode, we were able to see that the line continued to narrow. At the highest current, it appeared as if there were two or more lines whose line width was limited by the resolution of the spectrometer—about 2 Å.

We wrote up the results as fast as we could [18] and decided to send the paper to *Applied Physics Letters* because we were concerned about possible problems in the refereeing procedure for *Physical Review Letters*. After it was dispatched to the journal, I took stock of the situation. We had shown that GaAs was a laser material, but we had not made it into a laser with the nice and potentially useful properties that lasers usually have, namely, a collimated, coherent beam of light. What is more, we did not really even know how to do that, although we were beginning to get some ideas. We had competition from other laboratories. Now it felt as if they were breath-

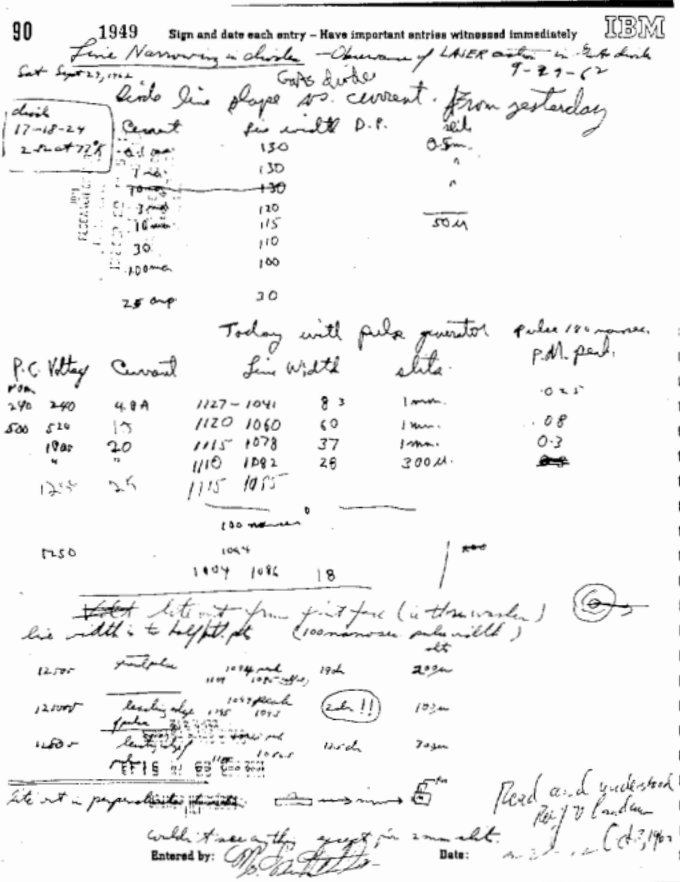


Fig. 2. Page from notebook showing data for spectral line narrowing.

ing down our necks, especially since it seemed to have been so easy for us. The atmosphere in the lab was electric for me. There seemed to be so many unanswered questions and so many experiments to do. There was only Gerry and I to do measurements. The fabrication area did not seem to be a problem. They could turn out "laser" diodes faster than we could measure them. Al Michel and Ed Walker, when asked by Bob Keyes to work on semiconductor lasers, agreed and set out to measure the near-field patterns.

It seemed to me that the management was being too conservative. They should have assigned as many people as possible to work on semiconductor lasers, especially at that particular time. There was a short period of about a month when we had the thing to ourselves, hopefully. If we did not have it to ourselves it was all the more reason for more people to work on it to gain a lead in an important area. I was in a strong position since I had been the first person to observe the phenomenon. I decided to circumvent the management and try to get some other people to work on the phenomenon. Bob Keyes and Rolf may not have been unhappy with what I was going to do, but they could not do it themselves because of commitments to other managers. So we had a role reversal: I became the advocate of the laser. This strategy was by no means selfless and altruistic, since I felt that I could easily be a collaborator with the people to whom I suggested experi-

ments. As it turned out, my strategy was beneficial not only to myself and the people to whom I talked, but also to the laboratory in general since we got many more results than we otherwise would have.

Our lab was a relatively small one at the time, and I was familiar with most of the work and equipment of the people in the semiconductor area. I went around the laboratory and talked to several of them suggesting possible experiments for them to do. In some cases, I just talked with them and let them know that participation in experiments would be welcomed. This was sufficient for some people, as in the case of Alan Fowler. Alan, incidentally, took up the study of optical coupling of lasers and did the first work on optical logic [19]. Other people such as Webster Howard and Frank Fang started to measure the spectrum as functions of temperature. Bob Laff began to measure the far-field patterns. The situation was a bit disorganized, but it seemed to be OK to me. I was able to keep tabs on what was going on, fairly well. Some managers thought too many people were working on the laser and that we were neglecting other areas. However, they were reassured when told that it was only for a month or two and that people would then go back to their original projects. My concept was that people would work on laser problems for a few months so that we could find a real laser and skim the cream, and put us ahead.

The atmosphere was charged. Almost daily there were new results. Within a week, Rick Dill and Dick Rutz came up with the method of cleaving to make an optical cavity [20]. This method turned out to be extremely important. To this day, it is the way most lasers are made. At the time, it gave us a quick and easy way to make lasers. From then on, all our lasers were made by cleaving. We were able to make "real" lasers. We saw modes in all of them. Before long, we saw the directionality effects by making the lasers long and narrow rectangular parallel-piped [21]. However, we continued to cleave all four sides. It was not until after we became aware of the General Electric work [22] that we started to roughen two of the sides.

During the month of October, Howard and Fang observed CW operation [23] at 2 K, and Michel and Walker showed directly that the light came from the p-side of the junction and observed the near-field mode pattern [24]. In addition, Gerry Burns and I observed operation of the laser at room temperature [25]. Rueben Title studied the paramagnetic resonance of acceptors in GaAs [26]. This last piece of work is not directly related to lasing, but it seems worth mentioning because it is an additional indication of the shift of interest in the laboratory toward anything related to semiconductor lasers. Lasher and Dumke calculated the threshold condition [10], [16]. As November 1, 1962, the publication date of our paper approached, we became more apprehensive that someone would publish or announce a laser before our paper came out. The International Electron Device Meeting was in Washington, DC, and the group from Lincoln Laboratories had a paper scheduled for presentation there. The title was innocuous

enough, but we thought they might announce a laser during that talk, so we submitted a post-deadline paper to the conference on the location of the active region in GaAs light emitting diodes. I traveled to Washington prepared to give two talks with two abstracts in my briefcase—one, the paper we had submitted and the other, the paper on the laser. Fortunately, it turned out to be a false alarm, and I gave the first talk. However, about three or four days before November 1, I received a paper on the semiconductor laser from Lincoln Labs to referee [27]. It was both good news and bad news. They had made a laser, but they were behind us. A day or two later, we heard a rumor that there was another group. We learned indirectly on October 30 that Bob Hall and his co-workers were publishing a report of a GaAs laser in *Physical Review Letters*. Of course, we were very disappointed. We decided to submit all the results we had up to that point to the *IBM Journal of Research and Development*. We wanted it to be clear that our work was independent of the work at General Electric.

It is fairly common for discoveries to be made simultaneously and independently in more than one laboratory. The time was ripe for the discovery of the semiconductor laser. The discovery of the laser, the talk by Aigran and Lax, the calculations by Dumke, the work of Mayburg, and Keyes and Quist set the stage. The technology to make the laser existed at several laboratories. The discovery of injection lasers was thought to be important. In fact, in this case, the discovery of the semiconductor laser was made at *three* laboratories. It very well might have even been more.

The disappointment of not being alone in the discovery of the semiconductor laser eventually faded. What remains is the memory of the fantastic experience of participating in the discovery of that laser and the thrill of having made a contribution to it.

REFERENCES

- [1] G. Burns and M. I. Nathan, "p-n junction lasers," *Proc. IEEE*, vol. 52, pp. 770-794, 1964.
- [2] R. N. Hall, "Injection lasers," *IEEE Trans. Electron. Devices*, vol. ED-23, pp. 700-704, 1976.
- [3] R. W. Keyes and M. I. Nathan "Semiconductors at IBM: Physics, novel devices and materials science," *IBM J. Res. Develop.*, vol. 25, pp. 779-792, 1981.
- [4] P. P. Sorokin, "Contributions of IBM toward the development of laser sources," *IEEE J. Quantum Electron.*, vol. QE-20, pp. 585-591, 1984.
- [5] D. Buck, "The cryotron—A superconduction computer component," *Proc. IRE*, vol. 44, pp. 482-493, 1956.
- [6] L. Esaki, "New phenomenon in p-n junction," *Phys. Rev.*, vol. 109, pp. 603-604, 1958.
- [7] A. L. Shawlow and C. H. Townes, "Infrared and optical masers," *Phys. Rev.*, vol. 112, pp. 1940-1949, 1958.
- [8] T. H. Maiman, "Stimulated optical radiation in ruby masers," *Nature*, vol. 187, pp. 493-494, 1950.
- [9] P. P. Sorokin and M. J. Stevenson, "Stimulated infrared emission from trivalent uranium," *Phys. Rev. Lett.*, vol. 5, pp. 557-559, 1960; "Solid state optical maser using divalent samarium in calcium fluoride," *IBM J. Res. Develop.*, vol. 5, pp. 56-58, 1961.
- [10] G. J. Lasher, "Threshold relations and diffraction loss for injection lasers," *IBM J. Res. Develop.*, vol. 7, pp. 58-61, 1963.
- [11] W. P. Dumke, "Interband transitions and maser action," *Phys. Rev.*, vol. 127, pp. 1559-1563, 1962.

- [12] D. G. Thomas and J. J. Hopfield, "Fluorescence in CdS and its possible use for an optical maser," *J. Appl. Phys.*, vol. 33, pp. 3243-3249, 1962.
- [13] J. I. Pankove and M. Massoulié, "Injection luminescence from gallium arsenide," *Bull. Amer. Phys. Soc.*, vol. 7, p. 88, 1962.
- [14] D. N. Nasledov, A. A. Rogachev, S. M. Rivkin, and B. V. Tsarenkov, "Recombination radiation of gallium arsenide," *Sov. Phys. Solid State*, vol. 4, pp. 782-784, 1962.
- [15] R. J. Keyes and T. M. Quist, "Recombination radiation emitted by gallium arsenide," *Proc. IRE*, vol. 50, pp. 1822-1823, (correspondence), 1962.
- [16] M. I. Nathan and G. Burns, "Recombination radiation in GaAs by optical and electrical injection," *Appl. Phys. Lett.*, vol. 1, pp. 89-90, 1962.
- [17] W. P. Dumke, "Electromagnetic mode population in light-emitting junctions," *IBM J. Res. Develop.*, vol. 7, pp. 66-67, 1962.
- [18] M. I. Nathan, W. P. Dumke, G. Burns, F. H. Dill, Jr., and G. Lasher, "Stimulated emission of radiation from GaAs p-n junctions," *Appl. Phys. Lett.*, vol. 1, pp. 62-64, 1962.
- [19] A. B. Fowler, "Quenching of gallium arsenide injection laser," *Appl. Phys. Lett.*, vol. 3, 1-3, 1963.
- [20] F. H. Dill, Jr. and R. F. Rutz, "Method of fabrication of crystalline shapes," U.S. Patent 3247576, filed Oct. 30, 1962, issued Apr. 26, 1966.
- [21] G. Burns, R. A. Laff, S. E. Blum, F. H. Dill, and M. I. Nathan, "Directionality effects of GaAs light emitting diodes: Part I," *IBM J. Res. Develop.*, vol. 7, pp. 62-63, 1963; R. A. Laff, W. P. Dumke, F. H. Dill, and G. Burns, "Directionality effects of GaAs light emitting diodes: Part II," *IBM J. Res. Develop.*, vol. 7, pp. 63-67, 1963.
- [22] R. N. Hall, G. E. Fenner, J. D. Kingsley, T. J. Salty, and R. O. Carlson, "Coherent light emission from GaAs junctions," *Phys. Rev. Lett.*, vol. 9, pp. 366-368, 1962.
- [23] W. E. Howard, F. F. Fang, F. H. Dill, Jr., and M. I. Nathan, "CW operation of a GaAs injection laser," *IBM J. Res. Develop.*, vol. 7, pp. 74-75, 1962.
- [24] A. E. Michel, E. J. Walker, and M. I. Nathan, "Determination of the active region and light emitting diodes," *IBM J. Res. Develop.*, vol. 7, pp. 70-71, 1962.
- [25] G. Burns and M. I. Nathan, "Room temperature stimulated emission," *IBM J. Res. Develop.*, vol. 7, pp. 68-69, 1962.
- [26] R. S. Title, "Paramagnetic resonance of the shallow acceptor Zn and Cd in GaAs," *IBM J. Res. Develop.*, vol. 7, pp. 68-69, 1962.
- [27] T. M. Quist, R. H. Rediker, R. J. Keyes, W. E. Krag, B. Lax, A. L. McWhorter, and H. J. Zeiger, "Semiconductor maser of GaAs," *Appl. Phys. Lett.*, vol. 1, pp. 91-92, 1962.



Marshall I. Nathan (SM'64-F'71) was born in Lakewood, NJ. He received the B.S. degree from the Massachusetts Institute of Technology, Cambridge, in 1954, and the M.A. and Ph.D. degrees in physics and applied physics from Harvard University, Cambridge, in 1956 and 1958, respectively.

In 1958, he joined the IBM Research Division where he has done research in semiconductor physics, hot-electron phenomena, luminescence, and injection lasers. In addition, he has held several managerial positions there.

Dr. Nathan is a Fellow of the American Physical Society and received the 1980 IEEE David Sarnoff Award.