Injection Lasers

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(Invited Paper)

INTRODUCTION

IT often happens that announcements of new or related discoveries are made almost simultaneously by two or more groups of workers. The first reports describing the ruby and He–Ne lasers appeared in quick succession during 1960. Again, two years later, a pair of publications announcing the discovery of the injection laser appeared on the same date and were followed a month later by a similar announcement from a third laboratory. In each case, it would appear that the general body of scientific knowledge had advanced to the point where ideas could be fit together in a new way or where fresh avenues of investigation appeared promising. The opportunity for new exploration was thus perceived almost simultaneously by a number of different investigators.

The discovery of the semiconductor laser affords a good illustration of the way an idea takes form and develops toward a successful conclusion. In this paper, I will try to describe how these events took place in our laboratory, as we viewed them at the time.

BACKGROUND

I recall having been asked on several occasions before the summer of 1962 whether I thought a semiconductor laser might be possible. My response was negative. I had been vaguely aware of some of the suggestions that had been offered for achieving coherent radiation using semiconductors, but for several reasons, none of them struck me as offering even a remote possibility for success. The lasers known at that time required long optical paths and highly reflecting resonators to achieve sufficient amplification. This seemed incompatible with the strong free-carrier absorption that is characteristic of semiconductors. Stimulated emission generally involved transitions between very narrow energy levels, whereas optical transitions in semiconductors tended to be much broader, particularly if they involved conduction or valence band states. The most serious difficulty, however, was that radiative recombination in semiconductors had always been very inefficient. There are too many nonradiative processes, and these would prevent the radiation from building up to the necessary intensity. Besides, I was already engrossed in several other investigations and did not want to abandon them for a project that appeared to offer so little likelihood of success.

On the other hand, these questions did prompt me to look into the meaning of such concepts as ''population inversions'' and to find out how they would be expressed in semiconductor terminology. During this period, Dr. Maurice Bernard (CNET, France) was an annual visitor to our laboratory, and I found discussions with him to be particularly helpful in presenting the essential concepts in terms that were familiar to me [1].

The situation changed abruptly during the summer of 1962. On July 9, high-efficiency radiation in GaAs was reported in two papers which were presented by Keyes and Quist of M.I.T. Lincoln Laboratory and by Pankove of RCA at the Solid State Device Research Conference which was held at Durham, NH [2]. Both papers described junctions which produced band-edge luminescence with close to 100 percent quantum efficiency at a radiated power density on the order of 1 kW/cm^2.

I was greatly impressed by these results. They showed that at least one of the major obstacles that had stood in the way of making a semiconductor laser—the radiative efficiency—had been overcome. It seemed like a good idea to take a careful look at the other factors to see whether a laser structure could be designed which might offer some chance of successful operation.

PRELIMINARY IDEAS

During the next few days after this meeting, I tried to work out some of the design considerations. Bernard's analysis showed that the junction would have to be given a sufficient forward bias to separate the electron and hole quasi-Fermi levels by more than the energy gap of the semiconductor. This meant that both sides of the junction would have to be degenerately doped, with the transition region being less than a few diffusion lengths in thickness. At these high concentrations, I knew that the lifetime would become very short and therefore the active region would be very thin, probably no more than a micron and possibly a good deal less. With such a thin active region, there would be little opportunity for light traveling perpendicular to the junction to be amplified, so it became clear that it would have to propagate approximately in the plane of the junction. In this configuration, the Fabry–Perot mirrors that would be needed to provide optical feedback could be formed by making two of the side faces of the junction flat and parallel to each other. My first notebook sketch illustrating these idea is shown in Fig. 1.

With this much settled, the general form of the proposed laser became clear, and I started discussing it with some of the other members of our semiconductor group.

Manuscript received October 15, 1985.

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IEEE Log Number 8714193.

0018-9197/87/0600-0674$01.00 © 1987 IEEE

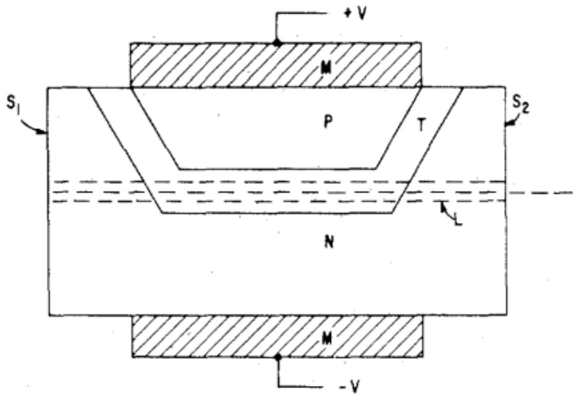

Fig. 1. Initial thoughts about how to make a junction laser. We soon realized that it would be better to use a flat junction which would intersect the polished faces S_1 and S_2.

This met with considerable interest, and I had no difficulty finding associates who wanted to join the venture and would be able to divert time from their current activities in order to participate.

The next move was to obtain a go-ahead from my manager, Roy Apker. I told him that it did not look like a major project, that four or five of us working half time could probably find out if the idea was good for anything in a few months. We believed it to be something of a long shot, with the chance of success being around one in five, but even if we never produced any coherent light, we were bound to learn a good deal about the high-efficiency junction luminescence that had been reported. That in itself ought to be enough justification to have a try at it, and of course, if we did succeed in creating a laser, there would be plenty of scientific excitement and all the rest. There was no need for a selling job; Roy liked the idea and gave it his blessing on the spot.

Getting Started

It was evident that we had to work fast. Every other semiconductor lab in the country knew about this GaAs luminescence and at least two of them, Lincoln Laboratory and RCA, had a head start of several months. We had to hope that either they did not regard making a try for a laser as being worth the gamble or that we had some good ideas to start with and could move fast enough to get there first. We decided on a two-level effort. It was fairly easy to come up with an initial design that was optimized with respect to the parameters that seemed important (as well as we could guess them) and which would also be easy to construct. We would start with this design, using it to check out fabrication and testing procedures in order to find out what our problems were going to be as quickly as possible. Meanwhile, in the expectation that this initial design would not succeed, some of us would try to figure out what might be going on inside those junctions so that we could come up with an improved design that would stand a better chance of working.

It is important to emphasize that we could see several reasons why an injection laser might *not* be possible. Foremost was the knowledge that the active region, where degenerate concentrations of electrons and holes would have to coexist, would be very thin. Nevertheless, it would have to provide enough gain to maintain a wave that would fringe out a considerable distance into the lossy n-type and p-type regions on either side. We did not know how lossy these regions would be, how deeply the light wave would extend into them, or how thick the active layer would be. We did not even know how much gain to expect in this layer since there was no way of guessing how many electrons and holes we might be able to inject into it or how much their effective temperatures might be increased by the injection process itself.

There was also the problem of the fuzzy band edges. We knew from tunnel diode work that states near the band edges of heavily doped semiconductors would be smeared out over a considerable range of energy, and it appeared that this would substantially decrease the effective densities of states and affect the optical transition probabilities in the spectral region that we were most concerned about. We had no idea how serious these effects might be. Another concern was that the optical properties on the n-type and the p-type sides of the junction would be different, and that this should cause the wave to curve towards the side with the higher refractive index. This might have to be taken into account by constructing the junction with a matching curvature and by tilting the mirrors on the two opposing faces by the appropriate angle. Unfortunately, we had no knowledge of how large the difference in refractive index might be. Neither did we know what the net impurity gradient would be in our junctions. Keyes and Quist had used zinc diffusion to create their junctions, but we knew zinc to be a fast diffuser and it might produce junctions that would be too gradual for our purpose. Would we have to use a different acceptor and could it be made to yield junctions with the same high efficiency? We did not know. We did not have answers to any of these questions. What we did know was that they could be faced later; the immediate need was to get underway with the initial design as quickly as possible.

We rounded up all the wafers of strongly n-type GaAs we could lay our hands on and began diffusing zinc into them at various temperatures. After diffusion, we cut them into strips about a half millimeter wide (which we hoped would provide enough optical path length) and cemented them to plates so that we could lap and polish their edges in order to form the resonator faces. The strips were then sandblasted apart into tiny rectangular pellets and mounted on headers for testing. These fabrication steps were carried out by Ted Soltys who had worked with me for many years and had experience building all sorts of semiconductor devices. Fortunately, this experience included a good deal of practice making GaAs tunnel diodes, so a lot of this background was directly applicable to the laser program.

Gunther Fenner took on the job of testing the diodes. We knew that it would be necessary to cool the diodes and send as much current through them as possible without burning them up, so he built a pulser that would deliver 50 A pulses and mounted the diodes in an unsilvered liquid nitrogen dewar so he could see what was happening.

It seems strange now, but at that time, one of our big uncertainties was to know what to look for as evidence that the diode was lasing. Clearly, we could expect some kind of change in the luminescence when stimulated emission set in, so the first test Gunther tried was to measure the light output using an infrared phototube and compare it to the diode current during the pulse. We had some doubts that this test would show anything, however, since if the luminescence efficiency was already 100 percent, it could not be expected to go any higher just because the transitions were being stimulated. It seemed more likely that the increase in recombination rate above threshold would cause a distinct change in the current–voltage characteristic. We watched for such changes, but never did see anything this way. We also knew that there ought to be a narrowing of the spectral distribution above threshold so we measured the spectra of a few of the diodes, but since this was relatively time consuming, we decided to depend mainly upon some of the other tests. Besides, there were so many uncertainties about the optical properties of these heavily doped junctions that we were not at all sure that the narrowing would even be observable.

The test that would clearly indicate something unusual going on was the observation of the far-field radiation pattern. We believed that this ought to change as the laser went above threshold, so Gunther placed an infrared image tube outside the dewar to see if he could observe any changes in the light distribution as he increased the pulse amplitude. It was not quite that easy, however. The first pulser produced pulses about once every few seconds, and it proved difficult to make visual comparisons of images that were separated by such a long time interval. Sometimes a nitrogen bubble would get in the way during the pulse or some ice would deposit on the face of the diode and the pattern *would* change, and it took several more pulses before we would realize what had happened.

The rest of our group consisted of Jack Kingsley and Dick Carlson. Jack was our laser expert. He was familiar with optics and the way other kinds of lasers behaved, and he was a big help as we tried to figure out what might be going inside these junctions. Dick's job was to study the zinc diffusion process to make sure that it was under control and the junctions were flat and free of imperfections. We needed to know how the impurity profiles depended upon the diffusion conditions so we could have a way of determing the optimum thickness of the active region.

Once the initial design was settled and the assembly and testing procedures were worked out, my participation in the project became more like that of a back seat driver. Ted and Gunther knew what to do and were going about it full speed ahead, and I did not want to do anything that would slow things down so I turned my attention to making sure that our ''production line'' did not run into any snags. We needed better supplies of n-type GaAs and I wanted to watch for changes that might make the assembly go more smoothly. I also began putting together an interferometer which we would later use to measure the parallelism of the Fabry–Perot faces, since I was doubtful that the polishing procedures we were using were doing a proper job. During this period, I also began working on an analysis which would describe the propagation of the light wave along the junction region. This was to provide the basis for the more advanced laser design which would be needed after we had determined that our initial approach would not work. It turned out that this analysis would not be needed.

By the time the project was a month old, Ted had made his eighth diode and Gunther was starting to test them with his pulser. Some turned out to be dead shorts and others with poor contacts just disintegrated under the heavy current pulses that he applied, but a few showed what appeared to be reasonably strong spontaneous light emission. We made some improvement in the asembly procedures and Gunther shortened his pulses from a few milliseconds to a few microseconds, and the yields of promising diodes gradually improved.

At about this time, we received word that Nasledov had published a paper which made reference to the observation of coherent light from a GaAs junction [3]. Before long, we obtained a translation of this paper and were relieved to learn that it described only a slight amount of spectral narrowing and that he had considered the possiblity that it might have been due to stimulated emission, but had rejected this explanation in favor of another which he regarded as more likely. Furthermore, he had made no provision for any kind of optical feedback so it seemed evident to us that his structure could not have generated any coherent radiation.

EUREKA

Our big news came on a Sunday morning after our project had been underway for only two months. Gunther had been spending the weekend testing one of Ted's latest diodes L-52 because of something unusual that he had noticed the previous Friday. As long as the pulse amplitude was below 6 A, the phototube response had the same shape as the current pulse, and the two increased together in amplitude. However, as the pulse was increased above 12 A, the light ouput began to rise much more rapidly. That looked interesting, so Gunther rigged up the image tube to see if it would help explain what was going on. Below 12 A, he only saw a diffuse glow on the screen. However, at higher currents, a new and unexpected pattern appeared, a bright horizontal line that could hardly have been produced by a spontaneous radiation source. That evidence was exciting enough that he called Roy Apker at home to come in for a demonstration.

To this day, that horizontal line pattern remains a mystery. We saw it once more on another diode, but none of the explanations that we invented to account for it made any real sense. However, on the following day, Gunther did obtain some far-field patterns from L-69 which were unquestionably the result of coherent light emission. They showed strong interference lines that were consistent with what one could expect from a coherent source the size of the junction edge.

Next followed several frantic weeks as we studied more diodes and gathered as much data as possible in preparation for a publication which would announce our results. Jack Kingsley was in his element during this period as he and Gunther set about improving the experimental measurements and trying to figure out what they meant. This was not obvious, since the pulses were still long enough that the diodes would heat up and cause the spectra to shift around during each pulse. Besides, we were not sure which were normal diodes and which were generating strange results because of some defect in their construction. During this period, we also had to spend some time preparing documentation for a patent application which needed to be filed before our manuscript could be submitted.

An awkward incident occurred during this period which I still remember all too vividly. Dr. Bernard had dropped in for one of his annual visits, and I had to carry on a discussion of various topics relating to semiconductor lasers without being able to tell him about our results. How I wanted to take him into the adjoining room where I could show him a laser in operation!

Out paper appeared in the November 1 issue of *Physical Review Letters* [4]. The same mail had some unexpected news for us, too, the announcement that a group at IBM had also obtained coherent light emission from GaAs junctions [5]. This initial IBM publication described a structure which did not provide for optical mode selection, but their results showed pronounced spectral narrowing and was clear evidence that stimulated emission had taken place. A little later, a reprint arrived describing a semiconductor laser that had been built at Lincoln Laboratory [6].

Evidently, the time was right for the discovery of the injection laser. The near simultaneity of these three announcements suggests that they had all been sparked by the same event, the discovery of high-efficiency recombination radiation which had been announced a few months earlier. That announcement did not suggest how one might go about making an injection laser, but it did initiate several lines of investigation which converged from different directions toward the same final result.

DISCUSSION

There is a wonderful thrill in coming upon a new discovery, and it is worth giving thought to the various factors that helped to bring our laser project to this timely and successful conclusion.

There is no doubt in my mind that good luck played and important part. I might not have attended that particular session at the conference in Durham. It was fortunate that my semiconductor experience happened to coincide with those fields that would be needed for the conception and development of the laser. Through this work, I had become familiar with radiative recombination and the optical properties of semiconductors and had conducted studies of tunnel diodes which made me feel at home with degenerate semiconductors. This work included considerable experience with GaAs junctions. I am also sure that the fact that I had been an amateur telescope maker in my younger days helped to nourish the idea of polishing the resonator faces of our little laser pellets.

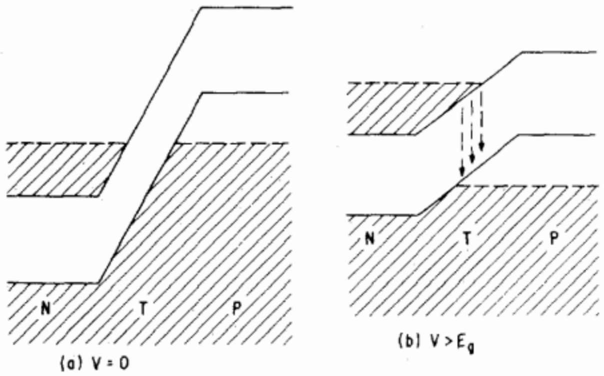

Fig. 2. By applying a forward bias to a degenerate junction, the electron and hole populations can be made to overlap within the transition region without exceeding the threshold for injection across the junction. This produces a population inversion within the transition region *T*.

Something else had helped make me receptive to the thought of making an injection laser. In working with tunnel diodes, I had realized that the degnerate electron and hole populations could be caused to intermingle in the junction region by the application of a forward bias voltage even though it was slightly less than that required for minority carrier injection [7], as illustrated in Fig. 2. I had looked upon this as one of my more novel ideas, but had been disappointed that no good had ever come of it. Now, however, I realized that those intermingled carriers represented the population inversion that would be needed to make a semiconductor laser work, and I thought that at last my little idea had found its destiny. It never did, but it was one more factor that encouraged me to give serious consideration to the semiconductor laser.

Another important factor was that in those days, the working climate in our laboratory was highly conducive to that kind of investigation. The contract load was relatively modest and there were not many project-oriented programs so it was fairly easy to rearrange priorities in order to pursue a promising new idea. We moved rapidly because we had the enthusiasm that comes from knowing that we were working on our own idea and that it was a good one, and we were encouraged to work on it as hard as we wanted to.

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Robert N. Hall (SM'53–F'57–LF'87) received the Ph.D. degree from the California Institute of Technology, Pasadena, in 1948.

He joined the General Electric Research Laboratory, where he began his work in semiconductor device physics. Initially he investigated methods for purifying germanium and developed the fractional crystallization process. This work led him to the concept of the p-i-n rectifier, and to the alloy process as a means of fabricating such junctions. From analysis of the temperature dependence of the current in p-i-n junctions he identified recombination via deep-level impurities (Hall–Shockley–Read Recombination) as the explanation for their measured characteristics and for the dependence of the minority carrier lifetime upon doping and injection level. He participated in the early development of tunnel diodes and conducted studies of phonon-assisted tunneling at liquid helium temperatures. In 1962 he led the group of researchers who first achieved coherent radiation from semiconductor lasers. He next studied the preparation and properties of extremely pure germanium needed for making high-resolution gamma-ray spectrometers. His research in this field led to the replacement of lithium-drifted detectors by hyper-pure germanium detectors throughout the industry. He is currently investigating precipitate defects which form during processing of silicon integrated circuits. He has written numerous technical papers and has been awarded 43 U.S. patents. He has served on various professional committees and editorial boards.

Dr. Hall is a Fellow of the American Physical Society and a member of the Electrochemical Society. In 1963, he received the IEEE David Sarnoff Award in Electronics and in 1970 was elected a Coolidge Fellow of General Electric Corporate Research and Development. In 1976 he received the IEEE Jack A. Morton Award for "meritorious achievement in the field of solid-state devices" from the IEEE and was elected to membership in the Bohmische Physical Society in recognition of his development of high purity germanium for nuclear spectroscopy. The following year he received the Electrochemical Society Award in Solid State Science and Technology. He was elected to the National Academy of Engineering in 1977 and to the National Academy of Science in 1978. During 1978–1979 he served as a member of the American Physical Society study group on Solar Photovoltaics.