

Ideas and Stumbling Blocks in Quantum Electronics

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Abstract—Quantum electronics, including in particular the maser and the laser, represents a marriage between quantum physics and electrical engineering which was probably longer delayed than it might have been because the two were not sufficiently acquainted. The mutual discovery of one field by the other is discussed, as well as the misunderstandings and false starts. Specific examples are used to make more real the thinking of the early years in this field and the struggles with ideas which, as with most now-understood sciences or technologies, seem much simpler in retrospect.

IT is sometimes said that there is no single component idea involved in the construction of masers or lasers which had not been known for at least 20 years before the advent of these devices. Of course, a discovery which might have occurred earlier is not uncommon in science or engineering. Nevertheless, the case of quantum electronics is striking enough that it may be useful to review the development of ideas prior to the time this new field became visible, and the stumbling blocks which may have delayed its creation. I believe whatever unnecessary delay occurred was in part because quantum electronics lies between two fields, physics and electrical engineering. In spite of the closeness of these two fields, the necessary quantum mechanical ideas were generally not known or appreciated by electrical engineers, while physicists who understood well the needed aspects of quantum mechanics were often not adequately acquainted with pertinent ideas of electrical engineering. Furthermore, physicists were somewhat diverted by an emphasis in the world of physics on the photon properties of light rather than its coherent aspects. It is still surprising that the basic combinations of ideas required for quantum electronics were not more completely envisaged somewhat earlier than they were. Nevertheless, it is understandable that the real growth of this field came shortly after the burst of activity in radio and microwave spectroscopy immediately after World War II since this brought many physicists into the borderland area between quantum mechanics and electrical engineering.

I know that most of my electrical engineering friends, while well acquainted with absorption of radiation by atoms and molecules, were surprised to learn that excited molecules could give up energy coherently to an electromagnetic wave. With that knowledge in mind, they might at least have imagined utilizing such effects for amplification even if they were not expert with the specific arrangements required. Many physicists knew of stimulated emission, but few connected it with useful amplification. That amplification by stimulated emis-

sion had been well understood by many individuals is impressively demonstrated by a number of early records—from Richard Tolman's publication in 1924 discussing amplification by an inversion of population, to serious consideration of an experiment to demonstrate stimulated radio emission by John Trischka at Columbia University several years before invention of the maser. In all, I know of eight apparently independent discussions prior to the maser invention of how stimulated emission and nonequilibrium populations can increase the intensity of a wave. There may be others.

Why were the many discussions of stimulated emission not followed up to produce some actual demonstration or a useful device? In some cases such effects appeared to be only of theoretical interest, providing a neat and consistent explanation for characteristics of radiation and of absorption. Furthermore, the simple weakening of absorption with increasing population in an upper state (e.g., for $h\nu > kT$) always seemed to me an adequate demonstration of stimulated emission. The few experiments on stimulated emission attempted in the optical region made demonstration of any large effects seem difficult, and were not followed up by other experimenters. Probably the failure to couple the idea of feedback with weak stimulated emission helped make such effects seem inevitably small. In the case of Trischka, and in my own thinking before a feedback oscillator was envisaged, a demonstration of amplification in the microwave region due to inversion seemed rather difficult and not important enough to be worth the required time. In the case of inversion of nuclear spins, which was obtained some years before maser action, the low frequencies and nuclear magnetic dipole moments involved made most stimulated emission effects rather small, and presumably for that reason successful inversion of populations did not direct attention toward useful amplification.

In my own case, it was primarily a strong desire to obtain oscillators at shorter wavelengths than those otherwise available that induced me to initiate experimental work on the maser. This is why the first system designed on paper was for 1/2 mm wavelengths, in the far infrared. My students and I had previously tried many techniques—magnetron harmonics, coherent Cerenkov radiation, and others to obtain short wavelengths, and while most of them worked after a fashion, none gave the promise which masers did for good spectroscopic sources at short wavelength. Initially, I did not fully realize the maser's potential as a low noise amplifier or as a precision clock, though these two applications added considerable interest rather soon after work on a maser was started with James Gordon and Herbert Zeiger.

As indicated above, my belief is that for many of the physicists who understood stimulated emission, isolating such ef-

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fects seemed somewhat difficult, and the necessary experiments were not very important because to these same physicists stimulated emission was already rather well understood. The idea of feedback and large numbers of quanta in single modes which might have suggested practical applications and given additional value to such experiments had not occurred. In addition, there were some misunderstandings and confusions which played a role in delaying quantum electronics. About 1945 I had myself written an internal memorandum at the Bell Telephone Labs explaining that molecules and atoms could be used to generate short microwaves, but that intensities could only be low because they would be limited by the second law of thermodynamics—use of a nonequilibrium distribution and population inversion had not yet occurred to me.

Emphasis on the photon aspect of light deflected some physicists from coherent amplification. It turned out that before the maser was operational, John Von Neumann had suggested exciting electrons in a solid by neutron bombardment and thereby obtaining a powerful cascade of photon emission. Coherence was not mentioned, and I believe no one ever attempted such an experiment. J.H.D. Jensen told me that in the 1930's he had thought about stimulated emission from an inverted population as a cascade of independent photons, like a cosmic ray shower. He lost any great interest in the idea after an experiment which seemed to produce such effects turned out to be explained otherwise. In thinking about light itself, rather than microwaves, it may be that many electrical engineers would have not been any more concerned with coherence effects than were these physicists. However, both engineers and physicists were naturally led to consider coherence when dealing with the radio or microwave region, and I believe this is why initial ideas and development of the field were so dependent on those with experience in radio and microwave spectroscopy.

Some of the confusion about coherence seemed a little strange even in the early days, and will seem remarkable at this time, but was real. Consider the early experiment of A. T. Forrester. Shortly after World War II, he began an experiment to irradiate a photoelectric surface with two Zeeman components of an optical line in order to mix the two frequencies. The idea was to have the two frequency components separated just enough to produce a varying photoelectric current at a microwave frequency low enough to detect by available electronics. Forrester's interest, I understood, was to demonstrate the effect of mixing two discrete optical lines, an effect which should have been detectable by the recently developed high-frequency electronics. The experiment was not easy at the time, and apparently a number of physicists believed it to be conceptually wrong. There seemed to be confusion over the spatial extent of the possible coherence, and questions whether the independent particles of different frequencies could cooperate in emitting electrons from a surface and thus give a beat. My belief then and now is that a bright electrical engineer would have figured out that the experiment would work. Nevertheless, the basic idea was challenged by a publication in the *Physical Review* in 1948 and by enough scientists that after the experiment was under way I was asked by the sponsoring agency to review it and advise whether the idea was

faulty. The experiment was only difficult, not erroneously planned, and Forrester's published result later dispelled any doubts.

Perhaps a somewhat more subtle example of physicists' bent at the time toward thinking in terms of individual particles involved the frequency spread of a maser oscillator. From the point of view of stimulated emission produced by an oscillating field established in a resonant cavity, it is not hard to understand that the radiation produced by a maser oscillator could indeed have a very narrow frequency band, independent of the width of response of individual excited molecules. Any real width has to be due to either the small amount of additional spontaneous emission or to thermal radiation present, as first worked out by James Gordon. However, there was the uncertainty principle relating time and energy, a basic law for physicists. With the lifetime t of molecules in the cavity limited (for the beam-type maser) by the time of transit, how could there be a frequency width much smaller than $1/t$? An electrical engineer accustomed to the almost monochromatic oscillation produced by an electron tube with positive feedback would perhaps not have given the problem a second thought. However, before oscillation was achieved I never succeeded in convincing two of my Columbia University colleagues, even after long discussion, that the frequency width could be very narrow. One insisted on betting me a bottle of Scotch that it would not. After successful oscillation, I remember interesting discussions on this point with Niels Bohr and with Von Neumann. Each immediately questioned how such a narrow frequency could be allowed by the uncertainty principle. I was never sure that Bohr's immediate acceptance of my explanation based on a collection of molecules rather than a single one was because he was convinced, or was due simply to his kindness to a young scientist. My discussion with Von Neumann had a more special twist and occurred at a social occasion. When I told him about our maser oscillator, he doubted that the uncertainty principle allowed our observation of such a narrow frequency width to be real. After disappearing in the crowd for about 15 minutes, he came back to tell me he now understood the situation; my argument was correct. That Von Neumann took even that long to understand impressed me. He then went on to urge that I try for stimulated emission effects in the infrared region by exciting electrons in semiconductors. I was puzzled by his strong insistence on this, because at that time the use of semiconductors for stimulated emission amplifiers seemed much more difficult than other possible methods. It was only much later I learned that he had already independently proposed such a system to produce a powerful avalanche of photons by stimulated emission, and must have been avoiding the more usual response of saying he had invented the idea before he heard of our work.

I must note that, although a number of good physicists did not find the coherent aspects of masers straightforward, for most of those in the field of radio and microwave spectroscopy, they were fairly obvious. That was true, for example, of my colleagues I. I. Rabi, Polycarp Kusch, and Willis Lamb, and of course Arthur Schawlow with whom I later collaborated on the laser. Electrical engineers, while not so knowledgeable about quantum properties, also found coherence properties

very natural and seemed to have the right instincts about many aspects of quantum electronics from parallels in ordinary amplifiers and circuits. As an illustration, I give an example of how an electrical engineer helped me at one point.

The frequency stability of a maser oscillator was an important question, and I had worked out an expression for it which showed, I thought, that the cavity pulling would give an error proportional to the square of the ratios of quality factor Q for the spectral line to that of the maser cavity. On explaining this to an electrical engineer at Stanford (whose name unfortunately I cannot recall), he remarked that such a result was peculiar since frequency pulling of one resonance by another in circuits was proportional to the first power of this ratio. While denying any detailed knowledge of the quantum mechanical properties, he doubted my result was correct and he was right. A little further examination when I returned home showed that I had neglected reactive terms of the quantum mechanical oscillator and cavity, which then gave just the result he expected.

As of today, the more engineering ideas of coherence, feedback, and nonlinear frequency mixing have become so intermingled with the more physical ideas of discrete states and quantum mechanical processes in the minds of both electrical engineers and physical scientists that some of the above confusions will probably be hard to believe. That is why the few specific examples above may help remind us how things were.

In addition to conceptual stumbling blocks which affected the course of quantum electronics, in the early days there was also a limited appreciation of the potential of this field, and this too may deserve illustration. Of course, I do not pretend to have foreseen the field's full potential myself, though I was obviously more impressed by it than many others.

Consistent with my usual practice of working with graduate students, development of the maser proceeded at the rate of a normal graduate student thesis project, being completed by Jim Gordon approximately three years after the idea was conceived. Our laboratories at Columbia University were completely open in the usual way of academic institutions, and many knowledgeable people visited the maser experiment. However, no one seems to have thought it interesting enough to reproduce or to try to compete with us. As far as I know, there were no concurrent efforts to obtain a molecular or atomic oscillator except the work of Basov and Prokhorov in the Soviet Union, and in this case I am not familiar with just what was done in these earliest years.

Completion of the maser oscillator was exciting to some, but evoked no more than mild interest on the part of other of my friends and did not immediately generate any great flurry of work. For some time it appears that the potential of quantum electronics was unappreciated by many of those not already in the field. In part this result could have been because the first maser itself may have been judged both limited and specialized. But also, for understandable reasons, scientists busy with their own research are not necessarily quick to see the potential of new events in other fields. Whatever the reason, when the performance of masers as frequency standards and then as amplifiers became more evident and as new varieties such as many solid-state systems were proposed, interest in masers grew. By

1960, publications on new varieties of masers became so common that, presumably under the assumption that the excitement must be over, the Letters section of *The Physical Review* made public a policy not to accept any more letters on new masers.

The delay of about six years between masers in the microwave region and lasers, or masers at shorter wavelengths, was no doubt also due in part to some conceptual stumbling blocks. One of these was imperfect recognition of the possibility of obtaining a high Q and of emphasizing a single mode in a structure which is very large compared to a wavelength, like the Fabry-Perot, even though Fabry-Perot resonators were well known. This problem and some others made it difficult to recognize ways that masers operating at infrared or optical wavelengths could perhaps be as easy or easier, rather than harder, than those in the microwave region. I shall not here try to explore the missing conceptual links, but rather turn to a different aspect, an apparent lack of appreciation of the potential of optical masers prior to late 1957. A number of individuals certainly recognized that maser techniques might be extended to much shorter wavelengths. I believe it was in 1956 that Bill Otting, Head of Physics for the Air Force Office of Scientific Research, asked me if his office could support me or someone else I might suggest in work towards an infrared maser oscillator. It is difficult to remember how many other more casual conversions there might have been on the subject or to know how many scientists may have considered this, but there were apparently no substantial efforts to explore maser oscillators at wavelengths much shorter than the microwave region before 1957. I know why I myself delayed this long—I was busy with and excited by microwave applications of the maser and saw only rather brute-force methods of moving to much shorter waves before that time. I was waiting for a "neater" idea to occur. About others I have no direct evidence, but believe a lack of appreciation of the potential of lasers and a closely connected effect due to the state of development of optics and optical oscillators both played a role in the time-delay between microwave and optical oscillators.

Optical spectroscopy had its heyday for physicists in the 1920's and 1930's; by 1940 most physicists considered it a mature field of solid importance but from which no remarkable breakthroughs were likely to emerge. There was, I believe, an attitude of déjà vu about optics. After World War II optics and optical spectroscopy did have a substantial renaissance, especially in the hands of French physicists, but was still not an area to which many turned for forefront physics. As I see it, lasers might well have been invented during this 1925-1940 period, although they would have been more difficult for lack of certain techniques which were further developed in later decades. These include a miscellany of things such as good optical coatings, flash tubes, and improved varieties of infrared materials and detectors.

As the possibility of high quality, single or almost single-mode lasers came into everyone's view, interest and intensity of attention to this field increased sharply. Nevertheless, much of its now quite evident potential was initially appreciated only by enthusiasts and some not even by them. There was almost immediate interest in the optical maser proposals of Schawlow and myself, but the beauty of the device may

have been more attractive to most scientists than its potential applications. A favorite quip which many will remember was "the laser is a solution looking for a problem." While an enthusiast myself, and aware of the potential for high precision measurements, monochromaticity, directivity, and the high concentration of energy that optical masers would provide, I missed many potent aspects. The area of medical applications is one that did not occur to me initially as promising. In retrospect, I can imagine recognizing the beauty of operating directly through the pupil without other insults to the eye, but since I had never heard of a detached retina such an idea would have been another "solution looking for a problem." My own scientific interests were primarily in the direction of new forms of spectroscopy and precision measurement, and hence I needed only modest power. While it was evident that optical masers could be expected to produce powers of at least a number of watts, I did not initially think of very short pulsed operation at a power level of many kilowatts, as was produced by Maiman's ruby laser.

In looking back over why the field of quantum electronics took as long as it did in getting started and why even then the buildup was initially not more rapid, I necessarily mention some of the stumbling blocks, misconceptions, and fumbles. The development of any science by humans has its similar mistakes and illogicalities. Recalling these can keep us humble and make us aware there may be other exciting events not yet visible around the corner. However, focusing on problems of the past omits or deemphasizes the remarkable insights and inventions made by a large number of colleagues who have

contributed to this field, and the vigor with which industry pursued and developed it. I can resist discussing these impressive aspects of the field only because I know others will treat them appropriately.



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From 1937 to 1939 he was an Assistant in Physics at the California Institute of Technology. For the next eight years he was a Member of the Technical Staff at Bell Laboratories. In 1948 he joined Columbia University, first as an Associate Professor of Physics from 1948 to 1950, then as a Professor of Physics from 1950 to 1961 and also Executive Director of the Columbia Radiation Laboratory during 1950-1952 and Chairman of the Department of Physics during 1952-1955. He was with the Institute for Defense Analyses as Vice President and Director of Research during 1959-1961. From 1961 to 1967 he was with the Massachusetts Institute of Technology, as Provost and Professor of Physics during 1961-1966, and Institute Professor during 1966-1967. In 1967 he became University Professor of Physics at the University of California, Berkeley.

Dr. Townes was awarded the Nobel Prize in 1964. He was also the recipient of the Niels Bohr International Gold Medal in 1979. In 1983 he was enshrined in the Engineering and Science Hall of Fame. He was on the Board of Editors of the *Review of Scientific Instruments* from 1950 to 1952; the *Physical Review*, 1951-1953; the *Journal of Molecular Spectroscopy*, 1957-1960; the *Columbia University Forum*, 1957-1959; the *Journal on Missile Defense Research*, 1963-1965; and the *Proceedings, National Academy of Sciences*, 1978-1984.