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Quantum Electronics, and Surprise in Development of Technology

The Problem of Research Planning

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The evident importance and the considerable expense of scientific research stimulate frequent efforts to assess its contributions to our society, and to optimize its planning. Such efforts are usually undertaken on the premise that we can and should make decisions about the support of scientific research on the basis of what we foresee as its tangible contributions to the nation. While hard-nosed assessment of the contributions of research is clearly appropriate and worthwhile, I am convinced that devotion to this premise is often self-defeating, as will be illustrated here by the obstinate and sometimes bruising facts of past experience.

If we forget the cultural values of knowledge, and evaluate science only by the touchstone of "practical" results, we may at first seem to have a straightforward guide for planning research. We know well that basic research develops many of the new ideas and new information from which technology is derived. Hence, it is easy to conclude that we need primarily to consider what types of technology are wanted for the future, and sponsor those forms of basic science which will contribute the background of information needed for them. There is indeed some truth in this reasoning; it applies particularly to those aspects of technology and of science which we now understand reasonably well, and where we are looking primarily for upgrading of our present abilities or the maturation of developments which are now predictable. But our ability to foresee the practical effects of science is too imperfect. For periods of time as long as a decade or more, or for the really new ideas and startling developments which are not now foreseen, the above approach is unhappily limiting and misleading. Furthermore, it gives a very unrealistic view of the environment needed for high-quality scientific research and of the complex interplay between science and technology, which includes the stimulation of basic science by applied science as well as the reverse.

How, in fact, can we plan for the new idea and the startlingly new, but now unrecognized, technology? Certainly we cannot show that a particular line of basic research will lead to new technological developments if we don't yet even know the nature of these developments. Nor is it possible to satisfy a persistent doubter that present basic research, even though it may be uncovering new knowledge and new ideas, will lead to important though unknown developments for human welfare. Perhaps the best way to examine such questions with some objectivity is the historical method, use of experience.

A general conclusion which seems to me to emerge from a historical approach—the examination of a number of research case histories—is that mankind consistently errs in the direction of lack of foresight and imagination. We continually underestimate the power of science and technology in the long term. Eminently knowledgeable planners and scientists, in attempting responsibly to make realistic appraisals of research, and facing what is at the time uncertain or unknown, all too frequently fall short in foresight and imagination. The element of surprise is a consistent ingredient in technological development, and one we have great difficulty in dealing with on any normal planning basis. Let me now proceed to discuss a particular example with which I happen to be well acquainted—quantum electronics. This is done in some detail, because very specific examples rather than generalities are probably necessary to overcome our natural tendency toward complacency.

Origin of Quantum Electronics

Quantum electronics became a field of physics and of engineering with development of the devices known as the maser and the laser. They use a new type of amplification, the stimulated emission of electromagnetic waves from atoms or molecules. The two devices are of the same general class; in fact, the laser was originally called an optical maser, although the name maser is sometimes restricted to molecular amplification in the radio or microwave range because it was derived as an acronym for *m*icrowave *a*mplification by *s*timulated *e*mission of *r*adiation. The word laser simply means *l*ight *a*mplification by *s*timulated *e*mission of *r*adiation, an application of the same idea to light waves. The parent device involved an amplification technique so radically different that it could not grow out of previous electronics in any orderly way; in fact, its birth in the early 1950's seems to have almost required prior development of the field of basic research known as microwave spectro-

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scopy. How can I justify such a bald statement? Because the idea for maser amplification originated independently in three different laboratories of microwave spectroscopy, and from research rather universally eschewed in applied laboratories. Each of these three origins had a slightly different timing, and differed appreciably in its completeness and practicality. However, all three came from physicists occupied with basic, university-type research on the microwave spectroscopy of gases.

Technology as a Source of Basic Science

It is almost equally significant that microwave spectroscopy itself grew out of wartime technology. This, as well as a good deal of closely related radio-frequency spectroscopy, originated with physicists who had acquired experience in electronics during World War II. In particular, microwave spectroscopy—the study of the interaction between microwaves and gaseous molecules—came about because microwave oscillators and technology were well enough developed during the war to allow this new branch of physics to be fruitful. Thus, a field of basic research was made possible by technology, and the first work in microwave spectroscopy in this country was largely carried out in industrial laboratories. Four independent groups of scientists in the United States, at the Bell Telephone Laboratories, at Westinghouse, at the RCA Laboratories, and at Columbia University initiated more or less independently the study of gases by means of microwaves immediately after the war, and pursued it with some vigor because of its evident importance to physics. The historical importance of technology to its origin is quite clear when one finds that the only university group of these four had been heavily involved in microwave technology during the war and initiated its work to solve an important radar problem. A little later than these four laboratories, the General Electric Company and several universities began further work in the field.

Migration of Microwave Spectroscopy to the Universities

No doubt in the industrial laboratories there was some hope that the new field of physics would have a worthwhile contact with commercial applications. In the case of the Bell Telephone Laboratories, I had myself written a memorandum with some care to convince research management that this could be the case. However, after several years this type of work died out in the four industrial laboratories where it had an early start and moved to the universities entirely. There it attracted a good number of excellent students, as well as experienced professors, because of the insight it afforded into molecular and atomic behavior. Reasons for growth of the field in universities may seem natural enough. Reasons for its decay in industry are equally important, and illustrate rather clearly our dilemma in the planning of research.

Evidently the four large industrial laboratories, although deeply involved with electronics, did not feel at the time that research on the microwave spectroscopy of gases had much importance for their work. I do not know the detailed reasoning of management at Westinghouse and RCA, but after the small teams of research workers which had been quite successful at these laboratories left or lost interest, research in the field was not rebuilt. At the General Electric Company, the research scientist in this field was transferred by management decision to another field considered more pertinent to the company's business. In the case of the Bell Telephone Laboratories, there was a management decision that, while one senior scientist could be appropriately supported, the work was not important enough to the electronics and communications industry to warrant adding a second one. Yet it was out of just this field that 2 or 3 years later a completely new technique of amplification was born which now occupies hundreds of scientists and engineers in the same laboratories. Clearly, misjudgment of its potential was not a simple human fault of any one company or individual; it was a pervasive characteristic of the system.

Sociology of the Maser Invention

Microwave spectroscopy in the universities utilized some of the new electronics techniques of the time, and was able to examine delicately and powerfully the various types of interactions between electromagnetic waves and molecules in ways which were different from those of normal spectroscopy. My own work, by then at Columbia University, flourished in an environment where a considerable amount of related radio frequency spectroscopy was being carried out, and supported by a rather farsighted Armed Services contract. The resulting development of ideas, in close association with electronics, led in 1951 to invention of the maser at Columbia, and shortly after to other proposals for use of stimulated emission for practical amplification—one at the Lebedev Institute in the Soviet Union and another at the University of Maryland. It is worth noting that basic research in the Soviet Union was at that time primarily concentrated in laboratories of the Soviet Academy, some of whose scientists taught in universities, and that this closest equivalent to our university research laboratories was the setting for the invention there.

By 1954, collaboration with J. Gordon and H. Zeiger produced the first successful oscillator with the new amplifying principle. While a few applied scientists were enthusiastic, overall it evoked only very mild industrial interest. I cannot claim that foresight of the academic community concerning the maser was remarkably greater than that of industrial organizations. But what was important was one of the crucial strengths of academic institutions, that an individual professor by and large makes his own decisions as to what is worthwhile and what might work. This, I believe, generally allows a scientific diversity and utilization of individual insights or enthusiasms in the academic world that are difficult to match in more closely planned and ordered industrial organizations. The latter are especially adapted for a concerted attack on a well-recognized goal. But the diverse and novel ideas for strikingly new approaches to problems are more normally current in communities where vigorous basic research flourishes. Coherent amplification by stimulated emission of radiation, and the idea of gradual quantum transitions rather than quantum jumps, for example, were reasonably well-recognized processes in some academic circles. Applied scientists were at the time characteristically surprised by them. Furthermore, even though there are now many varieties of masers, for some reason the two most complete original suggestions for practical maser systems, from Columbia University and from the Lebedev Institute, both involved molecular beams and Stark effects, techniques and ideas which were of some currency in academic circles but scarcely ever considered in industrial laboratories. But certain ideas of electronic engineering were important too, for example, in providing an understanding of regeneration and of the utilization of coherent amplification. It was the mixture of electronics and molecular spectroscopy inherent in the field of microwave spectroscopy which set appropriate conditions for invention of the maser.

The new type of amplification immediately produced an interesting oscillator, but not so immediately a very usable amplifier. My visit with scientific colleagues at the Ecole Normale Supérieure in Paris generated what seemed to me the first clear view of a practical amplifier by the use of paramagnetic solid materials, because there I was associated with other physicists studying paramagnetic materials and became aware of some of their properties which were otherwise unknown to me. A somewhat similar idea grew up independently from Professor Strandberg, a microwave spectroscopist at M.I.T. He passed on an interest to Professor Bloembergen of Harvard, who had been studying paramagnetic properties for some time, and who provided the variant of the maser which is now its most practical form for amplifiers. By this time industrial laboratories had become more alert to the new possibilities, and it was Feher, Scovil, and Seidel at the Bell Telephone Laboratories who first built a workable amplifier with paramagnetic materials. From this point on, the nation's applied laboratories pursued maser amplifiers for the microwave region with vigor and success.

The Laser

By 1957, I was eager to try to push the new technique on into the shorter wavelength regions, since it was clear that molecules and atoms had the capability of amplifying wavelengths very much shorter than anything previously done by vacuum tubes. I discovered that my friend Arthur Schawlow, then at the Bell Telephone Laboratories, had also been thinking along somewhat similar lines, and so we immediately pooled our thoughts. It was he who initiated our consideration of a Fabry-Perot resonator for selection of modes of the very short electromagnetic waves in the optical region. This very likely had something to do with the fact that Schawlow had first been trained as a spectroscopist and had done his thesis with a Fabry-Perot, another important technique current primarily among university spectroscopists. From this collaboration came the first fully developed ideas for lasers.

The new device was so far out of the normal tradition that its value for applied work was not immediately obvious to everyone. Bell's patent department at first refused to patent our amplifier or oscillator for optical frequencies because, it was explained, optical waves had never been of any importance to communications and hence the invention had little bearing on Bell System interests. But the potentialities were soon sufficiently clear that a number of laboratories in both universities and industry became strongly interested in the optical maser, later called a laser. In particular, management at the Bell Telephone Laboratories not so much later gave it considerable priority. The first actual operating system, the ruby laser, was produced by Maiman at the Hughes Aircraft Company; this was followed shortly by a second type based on an idea of Javan at the Bell Telephone Laboratories, and then a third one made by Sorokin and Stevenson at IBM. Clearly, the nation's powerful industrial laboratories had begun their push to develop the field. Subsequently, quantum electronics has blossomed to its present level of about $200 million of business per year, with an expectation of about $1 billion per year by 1970 or 1971.

The successive ideas for improvement and extension of the new type of amplification to the point which I have described came primarily from the realm of basic research. Some of them were rather new, some of them older ideas which had been current in laboratories of basic research. Their sources were almost exclusively scientists trained in microwave and radio frequency spectroscopy. In fact, all but one of those I have mentioned or alluded to above had extensive experience in this field. The demand for such personnel in industrial and governmental laboratories by the early 1960's was, of course, intense.

Practical Uses

What has come out of this development? A total variety of applications too long to list. Since the new technique allows amplification and control of electromagnetic radiation in the infrared, optical, and ultraviolet regions approximately equivalent to what electronics has provided in the radio region, one needs only to think of the utility of light and of electronics to see that a marriage of these two fields would have possible applications in almost any sophisticated technology. I shall give a few examples.

Maser-type amplification comes very close to providing the ideally sensitive amplifier, which can successfully amplify one quantum of radiation. For microwaves, the new amplifier actually provided a sensitivity about one hundred times better than what had previously been available. While by now there are some other types of improved amplifiers, the maser amplifier remains and will likely remain for all time our most sensitive detector of microwaves. Its use is particularly important in allowing efficient transoceanic commercial communications through satellites, scientific measurements of new sensitivity, and in making practical space communications throughout the solar system.

The constancy of atomic properties and the lack of noise fluctuations also makes a maser oscillator the world's most precise clock. A maser based on hydrogen is so constant that if kept going for 300,000 years, its expected error would be only about 1 second.

Since light waves can be amplified by the new techniques, they can provide light of almost indefinitely high intensity. Already lasers produce light many millions of times more intense than what was previously available. Laser beams can be accurately controlled and focused to drill holes in refractory materials such as diamond, to partially evaporate and thus precisely adjust electronic circuit elements, or to do delicate surgery. As a surgical tool, the laser is particularly useful in the performance of operations inside the eye without any external incision.

The laser allows our most accurate measurement of distance. In the laboratory, it has detected changes of distance as small as 1/100,000 the diameter of an atom. The coherence of laser light allows interferometric measurements to a precision of a fraction of the wavelength of light up to distances of many miles. This is already being used for detection of earthquake phenomena, and for very precise machining. The directivity of laser beams makes them convenient tools for civil engineering; they have been introduced for the boring of tunnels, the dredging of channels, and the grading of roads.

In photography, the new intensities of light available have allowed much higher-speed photography than was previously possible. But still more spectacular is use of the laser as the basis for a new type of photography called holography. Laser light projected through a photographic film with holographic techniques, gives a real three-dimensional image with a wealth of detail and a remarkable depth of focus.

Other uses of laser beams include radar, guidance for the blind, information processing, and information storage and retrieval. In the future there may be wireless power transmission, large-screen color television, and cheaper communications.

The Research Planner's Problem and the Drive for Practicality

Consider now the problem of a research planner setting out 20 years ago to develop any one of these technological improvements—a more sensitive amplifier, a more accurate clock, new drilling techniques, a new surgical instrument for the eye, more accurate measurement of distance, three-dimensional photography, and so on. Would he have had the wit or courage to initiate for any of these purposes an extensive basic study of the interaction between microwaves and molecules? The answer is clearly No. For a more sensitive amplifier he would have gone to the amplifier experts who, after considerable effort, might have doubled the sensitivity of amplifiers rather than multiplied it by a hundred. For a more accurate clock, he probably would have hired those experienced in the field of timing; for more intense light, he would have sought out and supported a completely different set of scientists or engineers who could hardly have hoped to have achieved an increase in intensity by the factor of a million or more given by the laser. For more accurate measurements or for better photography, he would have tried other improvements of known techniques and very likely have achieved moderate success, but no breakthrough by orders of magnitude. It was the drive for new information and understanding, and the atmosphere of basic research which seems clearly to have been needed for the real payoff.

There is at least a superficial similarity between the search for new technology and the pursuit of happiness, each of which is sometimes best approached by indirection. We know some straightforward, but limited, ways to achieve happiness. A better house to live in, or even just an ice cream cone now and then will help. But generally the direct and continuous pursuit of happiness itself is much less successful in achieving the big result than dedication to worthwhile human values and enterprises, without such overt thought of self-satisfaction. Similarly, while direct and planned development of technology is clearly useful and should not be neglected, efforts confined entirely to this approach will be badly limited. Success can be enormously increased by the stimulation and the discoveries which come from an interested dedication to knowledge and discovery themselves.

Americans are intensely practical, and it is difficult to accept the idea that a result is not best achieved by systematic planning, keeping one's eye on the ball, and good hard work. But we have all too frequently had the experience that in judging the practical value of specific scientific research, and in certain cases even of engineering development, those who would seem to be most knowledgeable and responsible are not able to foresee the most imaginative and important steps. History shows this in many more cases than in quantum electronics. In fact, surprise in the development of technology is our regular fare.

Surprise and Nuclear Energy

Some of the interesting story of the development of nuclear energy is quite familiar. Einstein's deduction of the equivalence of mass and energy should have given some inkling of the possibilities even early in this century. During the first part of the 1930's, the exciting field of nuclear physics opened up and produced a small flurry of speculation about the possibility of nuclear energy. But the *Herald Tribune* of 1933 carried an assessment of these possibilities under the headline "Lord Rutherford Scoffs at Theory of Harnessing Energy in Laboratories." Rutherford could perhaps fairly be called the greatest experimental physicist of the day and the father of nuclear physics. He had just spoken in Great Britain about the splitting of the atomic nucleus in the same hall where a generation earlier Lord Kelvin, a great physicist of his day, had pronounced the atom indestructible. Rutherford commented, "The energy produced by breaking down of the atom is a very poor kind of thing. Anyone who expects a source of power from the transformation of these atoms is talking moonshine." Professor Rabi of Columbia University, interviewed at the same time, confirmed Rutherford's calculations and hence, apparently, his general conclusions. Professor La Mer, also of Columbia University, was quoted as saying, "I am pleased to see Lord Rutherford call a halt to some of the wild, unbridled speculation in this field." There were indeed some other opinions. Of those interviewed by the *Tribune*, Professors Sheldon of New York University and E. O. Lawrence of the University of California still held out some hope. However, the generation of nuclear energy was not for a few years taken very seriously by the scientific community and was hardly an issue in the support of the study of nuclear physics. In fact, there was considerable concern among physicists, planners, and in industrial circles that too much of physics was swinging toward the nuclear field and that there was too much attention given to this esoteric, relatively useless, aspect of physics. The General Electric Company, deeply involved in power generation, made an overt management decision during this time that the promise of atomic power was not worth its initiating any nuclear research.

Only 5 years after Rutherford's pronouncement, the unlooked-for phenomenon of fission was discovered, and suddenly the whole world of physics saw the possibilities of nuclear energy in a completely different light. Success could not be assured, but there were now straightforward ways of attempting to obtain nuclear energy. The basic knowledge and knowledgeable personnel were fortunately available because of the previous years of intellectual curiosity centered in the universities; this background and help from Europe's intellectuals were crucial to the United States and its allies.

Other Case Histories

The transistor, another outstanding technological triumph, is by contrast quite a different case, and represents one of success in research planning. M. Kelly of the Bell Telephone Laboratories did foresee that solid-state physics was important in a variety of ways to operations of the Bell System, and formed and encouraged a group of physicists interested in basic exploration of this field. At least initially, this was not done with any direct thought of transistor-type amplification. But it was Kelly's plan of basic physical research on solids, in contact with engineering interests and considerably in advance of most other industrial laboratories of the time, which led to the transistor and its many descendents.

An interesting example of our difficulty with foresight and imagination in a more engineering domain, and where the basic physical phenomena were rather well known, is the case of the airplane. Lord Rayleigh, one of the greatest physicists of the 19th century and certainly familiar with appropriate fields of physics, commented in 1896, "I have not the smallest molecule of faith in aerial navigation other than ballooning." This was followed by severe congressional criticism over the "waste" of government money on Langley's attempts to build a heavier-than-air machine, and was just 7 years before the Wright brothers successfully "navigated" over the sands of Kitty Hawk. One can trace an interesting and intense argument for some time thereafter over whether or not the airplane would ever amount to much. Eventually, human need for a flying machine and the characteristically surprising power of technology won handsomely again.

Which Way Genuine Realism?

The above shows us some of the cases where hard realism wasn't real and dreams were. One might well wonder how we can possibly hope to judge the value of specific basic research for the future of technology, and hence on what we can base our plans. My belief is that knowledgeable and responsible people, in trying to judge carefully and not run too much risk of being wrong, have almost inevitably been too shortsighted. Furthermore, planners, in trying to be realistic and faced with tough budgetary decisions, all too frequently find themselves convinced only about what can be demonstrated, and hence their programs are unhappily limited. Science fiction and human need seem to have frequently been more reliable guides to predicting long-range technological developments than sober scientific statesmen. The progress of technology to a point further than we can see clearly—and this means hardly more than a decade—is always surprising and almost invariably greater than we think.

How can we best foster discovery and useful invention? I certainly would not want to play down the importance of planned research and development toward the shorter-term goals which can be foreseen. For this, organized teams and keeping one's eye on the ball can be very effective, and in some cases are almost essential. On the other hand, an atmosphere where utility is paramount is likely to confine thinking in particular channels, and is too prone to smother and draw attention away from what will produce many of the happy technological surprises and radically new ideas. I can suggest three useful guides.

1) There should be an environment of evident devotion to knowledge and discovery themselves, as well as to practical results.

2) To take best advantage of man's curiosity and his potential for discovery, we must give clear attention to supporting the clever, productive, and dedicated researcher in his own insights about what is interesting or fruitful.

3) If the nation is to ensure itself against missing the most exciting surprises, it must ensure support for those fields, even the nonutilitarian ones, where new understanding (not just new detailed knowledge) is most rapidly developing.

I have purposely concentrated attention on the material results of science, but must at least pause to recognize that this involves the frequent mistake of omitting almost completely other important and perfectly real aspects of science and knowledge—their cultural values. Man's view of his universe and of himself which results from scientific research has a significance considerably beyond what is considered "practical" in the narrow sense. Discovery and understanding give breadth of view and inspiration, the satisfaction of man's innate wonder and intellectual drive, and a sense of creative achievement toward some of his most universal goals and most lasting monuments. As something of a parallel to the limitation of being concerned only with the tangible results of science, consider how far short we would be in explaining the importance of music to mankind in a discussion confined to its practical and economic results. However, basic scientific research does, of course, have a profound effect on man's material productivity and wellbeing, and this can be appropriately discussed as long as we remember that there are also other values at stake.

The Short and the Long Run

We have done well in basic research and the generation of new ideas during the last two decades. However, I am genuinely concerned about what seems to me a trend in the United States toward emphasis on the shorter-range goals and overconcentration of attention on utility to an extent which may well limit our technological productivity and leadership in the future. Having emphasized man's limitation in predicting the outcome of research, I do not want at this point to try predictions myself, other than to affirm the continuity of history and the constancy of man's nature. However, it is clear that among the many fields where we face decision now are high-energy physics and space exploration. Both are exciting, but expensive. Very little utility can really be predicted for high-energy physics, and little for much of space exploration. Yet we must examine them from both cultural and utilitarian points of view, and with such things on our conscience as the myopic tendencies of the past, our proclivity for taking the lack of foreseeable utility for lack of its real existence, and the ease with which we have disproved the possibility of what only a few years later becomes actuality in this ebullient world of science. And if in these fields or others we are found shortsighted, too lacking in daring, or too indifferent to forward-looking dreams, the pace of science and the impact of technology are now sufficient that our limitations will be obvious not only in the nation's future and the eventual judgment of history, but also to us personally, and in our lifetime.