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TECHNOLOGY AND ECONOMIC GROWTH

HEARINGS

BEFORE THE

SUBCOMMITTEE ON ECONOMIC GROWTH

OF THE

JOINT ECONOMIC COMMITTEE

CONGRESS OF THE UNITED STATES

NINETY-FOURTH CONGRESS

FIRST SESSION

JULY 15 AND 16, 1975

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TECHNOLOGY AND ECONOMIC GROWTH

TUESDAY, JULY 15, 1975

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON ECONOMIC GROWTH
OF THE JOINT ECONOMIC COMMITTEE,
Washington, D.C.

The subcommittee met, pursuant to notice, at 10:05 a.m., in room 1318, Dirksen Senate Office Building, Hon. Lloyd M. Bentsen, Jr. (chairman of the subcommittee), presiding.

Present: Senators Bentsen and Javits.

Also present: William A. Cox, William R. Buechner, Courtenay M. Slater, Lucy A. Falcone, and Robert D. Hamrin, professional staff members; Michael J. Runde, administrative assistant; and M. Catherine Miller, minority economist.

OPENING STATEMENT OF CHAIRMAN BENTSEN

Chairman BENTSEN. The hearing will come to order.

Gentlemen, we open these hearings on technology and economic growth at a particularly auspicious time. This morning, the United States and the Soviet Union are beginning an historic experiment in international scientific cooperation, with the launching of the Apollo-Soyuz flight. But as vital as space exploration is, our hearings today and tomorrow will concentrate on a much more down-to-earth, but no less important matter—the field of civilian technological innovation as it relates to economic growth, to jobs, to American business and industry.

By technological innovation, I mean the development of new products and new production processes that improve productivity, conserve on our resources, provide jobs, and which translate into a higher quality of life and longer lifespans.

The extraordinary progress which the American economy has made during the last century has in large part been due to the innovativeness of American industry, and the willingness of American industry to ucts and new production processes that improve productivity, con-recently, we have witnessed tremendous technological progress in the Western European countries and Japan that confronts many of our own industries with strong foreign competition, both here at home and in our foreign markets.

How we respond now to this new competition can determine the strength and progress of the American economy for years to come. We can either move to stimulate our domestic growth and our international competitiveness by vigorous Federal support for research and development in the basic sciences and their commercial applications, or we can withdraw into ourselves, throw up protective trade barriers around our threatened industries, and sap the competitiveness and the vigor from our own economy.

The American economy has always been a dynamic one, thriving on the ingenuity of inventors who come up with solutions to pressing problems, and businessmen who sometimes make them work. I am confident that we will choose the correct path, and stay this way.

But it is going to take some strong impetus for it, I believe, from the Federal Government.

As of today, the United States has no apparent national policy for encouraging basic research, or for stimulating the kind of technological innovation that is needed to build and maintain a healthy growing economy. Federal funding of research and development, which makes up more than half of the national total, grew by 5.6 percent in real terms between 1961 and 1967, but since then has actually declined by 3 percent per year. In 1967, the Federal Government spent $14.4 billion on research and development, compared with $11.3 billion in 1967 dollars this year. That is $14.4 billion down to $11.3 billion this year. That was a 21.5-percent decline, and the 1975 amount is smaller in real terms than for any year since 1963.

We have dismantled our cold war research apparatus, and have failed to replace it with one directed toward our new national needs—economic growth, environmental soundness, export competitiveness, and social welfare.

Gentlemen, we need a clear picture from you as to where we should go. We are very pleased to have men of such distinction before us this morning, who have prepared excellent statements in this regard. If you would introduce yourselves for the record, please—if you would proceed.

Mr. HORNIG. I am Donald Hornig. I am president of Brown University, and formerly science adviser to President Johnson.

Mr. WIESNER. I am Jerome Wiesner, president of the Massachusetts Institute of Technology, and former science adviser to Presidents Kennedy and Johnson.

Mr. DAVID. I am Edward David, executive vice president of Gould, Inc., and former science adviser to President Nixon.

Chairman BENTSEN. If you would proceed, Mr. Hornig.

STATEMENT OF DONALD HORNIG, PRESIDENT, BROWN UNIVERSITY

Mr. HORNIG. Mr. Chairman, it is a very great pleasure to be invited to testify before this distinguished subcommittee. I think you have very aptly summed up the problem which faces us, and I will be pleased to add my observations.

The subject of technological progress and how to achieve it in the first place, and how to translate it into economic progress, is a critical one for the country; and I am pleased that the subcommittee is investigating it in such depth. It is a subtle and difficult matter on which many offhand opinions have been expressed, to which little coherent or adequate analysis has been devoted. It seems to me that economists have only recently become interested in the dynamics of technological progress, scientific progress and technological inputs to industry. For too many years they have regarded technology as something which sat on the shelf, and which, like capital, could be applied at will. It was there. On the other hand, scientists also have assumed that if they built up the material in the journals, and that if the supply of knowledge grew, that somehow, someone somewhere would translate this to public purposes.

Chairman BENTSEN. Do we have a prepared statement from you, Mr. Hornig?

Mr. HORNIG. I will not have a prepared statement.

Chairman BENTSEN. I was just trying to follow your testimony, and I could not find your statement.

Mr. HORNIG. Unhappily, I have had a very difficult situation at my university, where we are having serious financial problems. My efforts there have precluded my putting together the testimony I had hoped to bring in today. And so, I am speaking from notes.

The American problem, it seems to me, is compounded by several things. One is that we are still, in most areas, in the technological forefront. This is not a problem of technological disaster. It is that American leadership has been based on being ahead, and the rapid progress of our competition threatens that leadership. Our problem is how to stay ahead technologically, rather than how to become technologically competent.

Secondly, we have been concerned about new ranges or social problems, while the world political climate and availability of raw materials is changing. This necessarily raises the question of how we adapt and change our technology to meet the new social requirements and new world strategic requirements without at the same time losing our competitive position. The heart of technological progress is continuing change to make available new skills. This has been a subject of deep concern to me for many years, particularly when I was science adviser to President Johnson. At that time, we spent a great deal of effort trying to understand how we could make the fruits of Government research expenditure more available to civilian industry; how to stimulate technological progress in areas that were outside of the space-atomic energy forefront of Government's efforts. I am thinking of industries such as housing, where the productivity has not increased perceptively in a long period of time, and which is still a major national need.

After I left the Government, I was vice president of a high technological company, Eastman-Kodak, which established and maintained its competitive position through research and development. I saw the same problems from a somewhat more commercial but equally interesting point of view. During the last 5 years, while I have been at Brown University, I have been away from the details of these problems, and I am not in a position to discuss detailed budgets or detailed economic estimates. However, I have stayed in continuous contact with industrial concerns, and from a more spectator role have continued to be interested in these problems.

My present role is concerned with education, and it occurs to me that this whole problem of technological progress is not fundamentally different, except that it is on a national scale, from the problem of education of individuals. Research is the acquisition of knowledge and skills and fitting this knowledge and skills into some sort of intellectual framework. Technological progress is the translation of the knowledge and skills into useful form; that is, into industrial processes. Economic progress is the utilization of these processes to meet human needs efficiently. This is quite parallel to what one tries to do in educating an individual.

So, as with the student, the first task of the society—in this case, industry—is to know, and that of course is what science means, knowledge. The job of basic research is to develop the knowledge base from which all technology, all of our problem-solving activities, can proceed. And in the rapidly-moving, high-technology industries, basic knowledge forms a limit to what we can do. There is no industry, whether it be computers or the health industry and new drugs, that can transcend the pool of the available knowledge at any given time.

The second task is to put knowledge to work. One of the great debates, when we talk about technological advance, is whether advance proceeds by the pull of human needs—whether we should guide our activities through a careful formulation of what is needed—or through the so-called technology push—the urge to find something to do with something new. I would assert that, at the basic research end, it is not very meaningful to talk about demand pull, largely because, fundamentally, new things cannot be done on order. For basic research, the goal has to be understanding and utilization, which is usually fairly far off in the future. It is for just this reason that basic research should be the fundamental responsibility of the Federal Government, and involve, in a major way, the universities of this country. Basic research cannot be tightly coupled to immediate economic payoffs, and hence there is very little incentiv for any but the very largest industrial concerns to carry it on. And even then, there are restrictions to engage in basic research. The fruits are very widespread and may equally benefit competitors.

And so, basic research has prospered, and must prosper, from a partnership of the Government and universities.

There is a second reason for carrying on basic research. The people trained in the course of doing basic research are the best way I know of of conveying new knowledge and new techniques into industry, and bringing them to the attention of those engaged in more immediate practical tasks. To a surprising degree, despite all elaborate information systems, the important elements of know-how move through our system from universities and into industry, and back again, via people.

Since I have mentioned people, I might mention that people are also one of the reasons advanced industries support basic research. The knowledge produced in basic research cannot be reserved to a particular industry. But by doing it, a pool of people can be maintained who are in contact with frontiers of the area, be it chemicals, drugs, or computers, and this can be maintained in the country's internal resource.

In my view, and as you have already mentioned, present Federal support of basic research is not adequate in two different ways. First, the absolute level of support is inadequate to meeting the whole range of new problems that confront us; and second, we have more and more moved to the notion that we can focus our basic research on the problems immediately confronting us. I would assert that that is the short-range policy. When I was science adviser, I used to play the game of asking what an infinitely wise science adviser in the 1920's, if such had existed, could have done to expedite the progress the country made between that time and the 1960's. And the conclusion I came to was that

that infinitely wise man in the 1920's would have had to do just about all of the things that were ever done.

For example, one of the questions I asked was, how could he have speeded up the genetics revolution in biology? And I found that in order to have solved those problems, he would have had to develop a computer industry to make computers; he would have had to find out about X-rays; he would have had to sponsor the development of X-rays to study the structure of crystals; he would have had to develop much of modern physical and organic chemistry. In short, I cannot see even by hindsight how such prescience would have been possible. But this is the character of basic research.

Now, it is ironic to me that it took a war to make this country appreciate the fundamental role of basic research. I am referring to World War II, of course. And in the years since the 1940's, it was, in many ways, the Department of Defense that properly understood the important role of basic research; in part, because it could see the whole spectrum between basic research and eventual, and sometimes long-term, utilization. I would hope that we can be as wise now, when we have great civilian problems to face.

Of course, basic research is not enough. I think we all know that. Much more is needed to translate knowledge into practical processes. That is what we call development, and that in turn must be coupled to needs, to markets, to the pool of existing mechanical skills and labor skills, and so on. It is very much connected with the entrepreneurial attitude of industry. This is fundamentally a non-Federal problem. Countries which have tried to do their development from within the framework of government have too often ended up with uneconomic, uncompetitive positions.

Chairman BENTSEN. I could not quite hear what you said.

Mr. HORNIG. I said the examples one has in other countries of efforts by governments to make production and development and market decisions have not had a record of great success. More often than not, they have resulted in large expenditures but, noncompetitive, noneconomic industries.

Chairman BENTSEN. We are going to limit each witness on his opening statement to 15 minutes and then we will return to questions because of the number of witnesses. So, if you could summarize, please.

Mr. HORNIG. I will summarize, then. The emphasis of the Federal Government needs to be on basic and applied research with some emphasis on quality. The Federal Government ought to focus on a national goal setting so that private entrepreneurs can assess the framework in which they must operate.

It must facilitate the flow of information between the Government, universities, and industry, and it must above all, by its own institutional mechanisms, present some degree of foresight so that areas which are not of immediate economic importance but which may be strategic—for instance, coal—will have been worked on. And that at each time we will carry on technological development in alternate areas, alternate to those required by the immediate situation.

In short, I think the Government ought to provide guidance and framework and support to the American industrial scene.

Chairman BENTSEN. Thank you very much, Mr. Hornig.

I am particularly interested in your point about governments getting themselves committed to major technological projects. Now, was it not during your tenure as science adviser to President Johnson, that a major commitment was made in the way of technological advance and judgment on the market for the SST which turned out to be a very uneconomic venture? What do you think brought that about? Was it a political thing? Or was it related at all to the SST's market virtues? Did you have a bureaucratic momentum develop there? What brought it about?

Mr. HORNIG. I never supported the SST. It was plainly uneconomic. There was an economic argument that even an uneconomic SST would drive more economic planes off the market on long-haul routes simply by having much higher load factors as a result of much shorter travel-times. I will not get into that.

The forces that pushed the plane plainly were partly the national prestige argument and industrial pressure to do it because it was interesting. There was a strong technological pressure to do the next exciting thing that could be done. It was the next frontier for some people in the aircraft industry.

I think it was a confluence of all of these things, together with the fact that the British and French and Russians were already doing it, that provided very strong arguments to those who wanted it to proceed.

Mr. WIESNER. May I add to that?

It was just happenstance that he got blamed for it instead of me. I left just before the decisions were made, but the pressures were clearly there. All of our studies that were done for the Science Advisory Committee, the technological and economic studies done by the Council of Economic Advisers, indicated the shortcomings of the aircraft, both technical and economic shortcomings.

But all of these other issues, the prestige, the desire to maintain the monopoly of the U.S. aircraft industry, the very strong fear that if a foreign SST was developed and we did not have one, our industry would lose 50,000 or 60,000 jobs, I do not remember the numbers, that whole range of problems, plus just the emotional feeling that we had to maintain world leadership, were the arguments that were used ultimately, persuasively, to push it even though it was very clear, from the analyses, that it was going to be a marginal airplane in terms of range, economics, and so on.

All the economics, I would say, are hard to judge because you have to make a guess as to what premiums somebody is prepared to pay to save time. And our economists might make one assessment of the value of that time and some other economists would make another judgment about what 2 hours of flighttime is worth to a busy person.

And so, was an area of ambiguity there, the economics. There was clearly no area of ambiguity in the technical field, and it is clear, even in the case of the Concorde, which was an easier plane to build than the one we contemplated, we moved to Mach 3 which was quite a whole new technology. The Concorde was pushing the end of the old technology, that the aircraft would not have long transoceanic range, and it would be very marginal for most of the long-haul routes.

Chairman BENTSEN. Is there a propensity for Government spending—which is taken to forward research from the technological advancement stage to the hardware stage—to drive out private research?

Mr. HORNIG. I think this is a real problem. I think, in fact, it did happen in this country to some extent in the case of the aircraft industry and some parts of the electronic industry. The Government did take over in a good way which eventually produced important civilian benefits.

At the same time, however, it had the effect you mentioned, in that by syphoning off many of the best people into selected areas that it detracted from private research in many purely civilian areas during the 1960's.

Mr. WIESNER. But not in the same fields. One has to discuss this in some detail. I do not think you can make that judgment in the aviation industry because the aviation industry was so stimulated by Government intervention that we would not have the same aviation industry. It would be on a very different scale. You would not have modern airplanes, in my view, of the kind we have today if we had depended upon private investment.

So that the scale of the industry and what they could spend of their own money for commercial aircraft was, in fact, vastly enhanced by Government spending for military aircraft.

On the other hand, as Mr. Hornig says, the large scales, based on military programs, drove up the cost of doing research and had such a glamorous excitement to it that it drew people out of the less-glamorous, less well-financed field, the fields that had to be financed by industry alone. And with few exceptions, I think, one could demonstrate professionals who work in the field have made strong arguments that industry, as a whole, the so-called civilian sector of the industry, did suffer as a consequence of the very large expenditures for military R. & D.

I think that this is the thing that varied with time in the immediate post-World War II era. I think that Government intervention for technology probably stimulated the whole industrial enterprise. But later, as it grew and dominated the technological scene and pushed up the cost, it probably was deleterious to the other aspects of the civilian economy; power fuel, for example.

Chairman BENTSEN. Senator Javits.

Senator JAVITS. Well, my interest in the testimony can be related primarily to the unplanned nature of the American economy. And from what I see in the hearing evolves the questions of knowing what we want and for what priorities we are ready to allocate resources.

What is a better standard of living? Do we need it? What are we willing to spend for it, and what are we willing to give up in the area A in order to gain some advantage in area B?

I have just come from the Soviet Union with a group of Senators, and the Soviet Union, aside from military and space matters, is probably America as it was 35 to 40 years ago. Well, they made their choice. They became a superpower because they piled their resources into military preparation and space. And whatever comments you gentlemen wish to make on that, in short, is it not an essential concomitant of what you are recommending to us by way of Government policy that we have a longer range view—a 5- to 10-year swing—of where we are going? Therefore, do we need something other than these short-ranged generalities?

For example, we have the problem of endemic unemployment with no solution.

Mr. DAVID. Senator Javits, I share your feeling that a more orderly long-range approach would be desirable. However, in cases where we have seen such an effort to make collective decisions, setting technological directions, these efforts have been notably unacceptable. There are several reasons.

First of all, technological development and innovation take many years to come to fruition—10 years is a good round figure. Deciding that many years in advance where the society should be is not likely to be successful. The foresight of governments and collective groups is not all that it should be.

I would prefer to see these decisions as to where we go and how we allocate our resources be made on a decentralized basis so that the mistakes of one group can be compensated by the wisdom of other groups. And I believe that system stands a better chance of bringing us to a point which will be to the benefit of society as a whole than collective decisions of the kind that are made in the Gosplan.

Senator JAVITS. Well, nobody is recommending a Gosplan decision. I'm speaking of the bill Senator Humphrey and I introduced which calls for voluntary planning.

Mr. WEISNER. Well, I think there is a great danger that we will spend the morning on this subject because, in my judgment, it is the most important single subject facing us as a nation which I think the technological issues that you are raising is a subparagraph of. But, I do not think any of us are prepared to make strong recommendations.

I have myself been thinking a good deal about this range of problems. I have spent a lot of time trying to understand the Soviet Union, as you know, because of my interest in disarmament. I have visited China and I am convinced that they do not know how to stimulate their technology. The Russians, 50 years after their revolution, are still anxious to buy Western technology for their industrial uses.

Senator BENTSEN. I am sorry, I am going to have to call a recess. We have a vote and we have two back to back. I am going to call a 15-minute recess.

Forgive me for interrupting.

[A brief recess was taken.]

Chairman BENTSEN. Mr. David, will you proceed.

STATEMENT OF EDWARD E. DAVID, JR., EXECUTIVE VICE PRESIDENT, GOULD, INC.

Mr. DAVID. Mr. Chairman and members of the Joint Economic Committee, I appreciate the opportunity to give you my views on U.S. science and technology and the economy. My theme is that R. & D. contributions to healthy economic growth hinge upon a subtle integration of needs and possibiltiy. Needs are a product of human wants and desires. Possibilities for satisfying them are created by technological advance. We find healthy economic growth when these two factors come together gracefully—when human needs are satisfied by technological possibilities. This is the basis of technological innovation.

Following this theme, it is easily seen that the quantity of funds for R. & D. is not an issue in itself. In fact, it is possible to spend a great deal on technological development with little influence on economic growth now or in the future. Similarly, needs by themselves do not produce growth, no matter how insistent nor how large the potential market so defined. It is the matching of needs and possibilities that is behind economic growth based upon innovation. The aim of policy in this realm ought to be to create the essential conditions and remove the barriers so that this matching can occur. tI is these conditions and barriers which I will address in my testimony.

CONDITIONS FOR TECHNOLOGICAL INNOVATION

Probably the most important condition is the mutual support of science, technology, and the marketplace. I am certain that you have heard a great deal about synergy between science and technology. Science provides us with the kowledge to fashion technology. Examples are rife; nuclear physics and nuclear technology are a prominent pair.

Less well recognized are that new inventions spur new science. It took the invention of the transistor to create a boom in solid state physics which is today one of the two largest fields in physics. That has, in turn, created still newer technology—integrated circuits and light-emitting diodes, for example. But this mutual support goes further. Through commercialization of solid-state devices, products such as miniature radios and TV's, calculators and computers, and satellites for communication have been made to serve consumers. From these, science and technology have derived further support. This pattern of using the successful products of R. & D. to support development of the next generation extends back to Edison and before. It is not only a tradition, it is the soundest way to support R. & D. The chemical industry, the computer industry, the communication industry, and the automobile industry all use this principle. Notice, too, that great technical ventures—a new generation computer, a new communication system—are expensive and so require support from large-scale production for use by masses of the public.

This view of the innovation process is further supported by experience in the realm of venture capital. Briefly, independent invention and small innovative businesses typically find that they must have substantial infusions of capital in order to succeed. As you know, special venture capital firms have come to exist because of this situation. The undercapitalized venture typically does not have the base of production to support R. & D. on an adequate scale nor over the period of time when the venture is in a negative-cash-flow, no-profit position. Insufficient resources are an occasional hazard of R. & D.-based ventures.

BARRIERS TO INNOVATION

The flow-back nature of the innovation process has been too little appreciated by Federal policymakers. Its self-supporting aspect calls into question a number of policies. A principal one concerns antitrust. The antitrust laws are administered as nearly as I can find without attention to their effect on technological-based innovation. The current actions against A.T. & T. and IBM illustrate this well. These two companies have the large-scale production for massive use that is required for modern development backed by fundamental research. They have used these resources in the way I outlined earlier. Fragmentation of these companies would significantly reduce the innovative capacity of their industries. The only contrary argument I have heard is based upon the mistaken notion that a supposed increase in competition is a cure-all.

Antitrust is also a barrier to the formation of coalitions by fragmented industries to attack large-scale development objectives, or even to support industrywide fundamental research laboratorise. A reevaluation of antitrust policies with R. & D.-based innovation in mind is definitely in order, in my opinion. Changes are required if the conditions for innovation are to be improved.

Federal policies for technology transfer and for large demonstration projects also require review. It is a seductive idea that technology developed for one purpose, say, space exploration or national defense, should be useful for commercial purposes. Yet, expectations for this process have clearly outrun accomplishment. Sometime ago I published an article on my view of the reasons. Basically, the lack of transfer stems from the absence of a financial stake in the technology by commercially oriented industry. The absence of an existing user community and market to feed the innovation process is the critical negative factor.

The same is true in my view of many large-scale Federal demonstration projects. Perhaps the most infamous is the Morgantown personal rapid transit projects. Too many such projects assume that a technical demonstration is all that is required. Actually, in commercial industry only one of three developments for which feasibility has been shown is ever offered to the public, and of these only one in three is commercially successful. The missing links beyond feasibility demonstrations are such obvious necessities as capital, production, marketing, distribution, servicing, and repair. Only an experienced and committed enterprise has a high probability of succeeding in such an effort. In the case of Morgantown, little consideration was given to these factors in planning and executing the demonstration. Such projects are likely to sink without a bubble, carrying with them the well-intentioned millions of dollars expended. Federal demonstrations aimed at commercial markets should be closely coupled to enterprise which is committed to implementing them for large-scale public use.

Another element of this same problem lies in patents and proprietary rights. These have been eroded in recent years, and yet I believe them necessary to the enlightened support of R. & D. This subject points up what I consider to be a policy fault in the bill establishing ERDA. Under a provision in that bill, developments emerging from ERDA funding will become Federal property—unless the administrator specifically gives an exemption. While it sounds appealing to say that such developments accrue to the public, deeper consideration indicates that "what belongs to everyone belongs to no one." Such denial of private property rights is bound to make it less likely that energy developments will be brought to public use on a scale justified by the needs. Thus I believe that many ERDA developments are likely to lie fallow due to this fault in the legislation.

In my opinion, the Federal Government should allow patent rights to become the property of the contractor, particularly where he shares the cost of development, and he should also be allowed to retain his background rights. The Government can retain a royalty-free right-to-use the invention for its own purposes. Should the Government find it essential to own patents resulting from Government-funded work, it should have the option of exclusive or limited licensing. This would encourage private funding to bring the development into use.

The Department of Defense, the most experienced Federal agency in sponsored R. & D., has recognized the desirability of contractor-owned technology in its policy. Furthermore, it has taken the enlightened step of recognizing R. & D. on future products as a legitimate cost of doing business. This recognition takes the form of independent research and development—I.R. & D.—funding as a part of allowed overhead. This allowance should be broadened to all Federal agencies since it is a way of hitching the company's commercial interest to Government programs. I.R. & D. could become even more effective in encouraging commercialization of Federal developments if the current restriction on I.R. & D. to departmental or agency concerns were eliminated. Broadening of the I.R. & D. charter to the entire Federal Government and removal of parochial restrictions would be a significant step forward.

The Federal Government is today the largest single consumer and customer for industry. Through its procurement, the Government can create incentives for development of advanced products. Federal procurement played an important role in development of both transistorized IBM computers and Xerox copiers. Today such incentives through procurement might well be pointed at energy conserving products. I understand that the NBS through its experimental technical incentive program is investigating such possibilities. This committee might well concern itself with ETIP and the role of Government procurement in technological innovation.

There are numerous other barriers to innovation which are worth mentioning in some detail if time permitted. Among them are:

(1) Federal requirements for extensive testing of products before marketing.

(2) Monetary policy, particularly the influence of high interest rates on increasing the costs and delaying profitability and positive cash flow of development programs.

(3) Decreased Federal support for graduate student fellowships and traineeships in the sciences and engineering.

(4) Problems of support of long lead time research by industry, and the proper role of the Government.

(5) The paucity of scientists and engineers with industrial experience in the highest councils of government.

(6) The lack of emphasis in industry on improving the mundane technologies that contribute to mature businesses.

All of these subjects seem to me to be prime candidates for this subcommittee to examine in its concern with economic growth and R. & D.

CONCLUSION

Overall, I am concerned that U.S. technical leadership is being eroded. However, the problem in my opinion is one of quality not quantity. The United States as a whole will spend some $32–$35 billion in fiscal year 1976 on R. & D. That is ample, but the allocation of those funds and their effective use for benefiting the economy is in question. Problems of quality are much more challenging than problems of quantity. There is no simple, easy panacea.

However, a key to the situation lies in my earlier comment about the mutual supporting roles of science, technology, and public usage on a substantial scale. It is through that mutual support that research development, and the economy can aid each other powerfully. In my view, this is fundamental to R. & D. support on the grand scale of today. Only when R. & D. improve both the living standard and life's quality for the public can science and technology derive the support necessary for still further progress. Minimizing the barriers to that improvement should be, and I presume is, a major concern of this subcommittee. That concern is well placed because without commercialization and wide public use of its products, R. & D. may prosper for awhile by promising future benefits. But if those benefits fail to materialize, R. & D. will become top heavy and will collapse. Even government will not be able to sustain it. I hope that this subcommittee will aid in reducing the barriers to use of technological products for bettering life in the United States and over the world. Thank you.

Chairman BENTSEN. Let me get to one very specific and a very major example. There is a lot of talk about coal gasification, and it is not a new subject at all. Back in World War II of course the Germans were doing it. Now, we are talking about new processes and doing it on a magnitude far beyond what has even been done before. We are speaking of plans that cost in excess of $1 billion now. How are we going to get industry to build this? Are we talking about making a commitment to purchase. Are we talking about protection of price? Are we talking about a partial guarantee of investment?

Would you propose that type of action, rather than the Government going ahead with its own technological research on that?

Mr. DAVID. There are two separate issues. One is the question of whether technology exists and, if not, how to encourage the creation of that technology. In my opinion, the right way to do that, from a Government standpoint, would be to offer to buy gas from coal gasification plants at a given price to satisfy Government needs. If that were done, there would be enough spontaneous R. & D. aimed at satisfying that market to produce the technology and, if the price were right, enough capital to build the plants to produce that gas. It may turn out that in some cases the Government will have to provide capital guarantees. However, I would hope not. In the long run private capital should be able to meet our needs for coal gasification plants.

If the private sector has been able and continues to be able to meet the capital needs for power utilities, we ought to be able to meet these other needs as well.

Chairman BENTSEN. We have another problem involved, though, in talking about our power needs and the fast breeder reactors.

But within the present state of the art, when you talk about coal gasification plants of the magnitude we are speaking of, you may be moving into a new area that has a substantial amount of risk. Or you might run into a situation where the Middle East countries might push down the price of oil for a couple of years to break those alternative sources. You would find it would be wise in that kind of a situation to provide some guarantee of purchase or some partial guarantee of investment; but the Government would have to put up at least some of the capital.

Mr. DAVID. I would agree. When I say, "Make a market through Government procurement," I am including provisions to prevent the

Arabs or others from undercutting that market for some specified period of time. Either a guaranteed price-production contract, or a guarantee of investment would accomplish that.

Chairman BENTSEN. Now, you stated a flaw in the earlier act that allows the Government to take the title to technology resulting from Federal funding, except where exempted. Can you comment about what all belongs to no one. Why cannot the patents be sold so as to recover some of the taxpayer's investment?

Mr. DAVID. It is my understanding that Federal agencies are not empowered to sell patents, nor to sell exclusive licenses, nor to sell licenses at all. I would not object to the Government taking title if the patents could be marketed in an effective way. That would be an alternative to what I suggested.

Chairman BENTSEN. When you talk about the eroding away of patent and proprietary rights, how does that come about and why?

Mr. DAVID. Well, in part, it is due to provisions in the ERDA, and also in NASA's implementing legislation. There is a trend in legislation to deny proprietary rights to private owners. The courts have also eroded patent rights. One area of particular interest is computer software. The courts have recently decided that computer software cannot be patented or protected. I do not think this is wise.

So some changes in the law are required with the idea of providing proprietary rights, so that private funds for final development and marketing will be available.

Chairman BENTSEN. The 1972 report of the National Science Foundation said that as technology becomes more sophisticated, based more on theory and general principles, the results of research and even of development tend to be more general, less uniquely appropriable by the organization doing the research, and as a result investment in R. & D. become weaker. Is that a general accepted statement?

Mr. DAVID. It is generally accepted amongst scientist and engineers and thoughtful managers that there is a tendency for industry to underinvest in R. & D. for exactly that reason; namely, not being able to recover the benefits of R. & D. for the funding corporation. However, that is true only for fundamental research and is much less of a worry for development and product projects. And the latter is where almost 90 percent of the total funding is spent.

Chairman BENTSEN. Thank you very much, Mr. David, and, being mindful of your other commitment, you many be excused, if you like to, at this time.

Mr. DAVID. Thank you very much.

Chairman BENTSEN. Mr. Hornig, we have seen the sign of a decline in Federal funding for untargeted, basic research—that is, for research dealing with basic knowledge, but not related to immediate policy goals. To what extent has your university been able to supplement that and make up for the void?

Mr. HORNIG. Essentially, not at all. Universities have been pressed on many fronts at once. In the last 2 years, inflation, declining real resources, have increased the cost of doing business as has the increasing load of Federal regulations. Other costs in universities have increased so that there have been no university resources to take the place of these Federal funds.

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Chairman BENTSEN. Well, I suppose you support the idea that we should be trying to give more funding for the universities from the Federal Government.

Mr. HORNIG. I would have argued that real effort should have been maintained at least constant, and that in an expanding industrial society, in any society which wants to expand qualitatively as well as quantitatively, the real effort should have been experiencing a modest growth.

Chairman BENTSEN. How effective is the coordination of basic research between individuals in different universities? Do you have any suggestions as to how it might be better coordinated and get a better exchange of information?

Mr. HORNIG. In my view, the scientific information flow system in this country is a remarkably effective one. The coordination takes place largely through the medium of a very good publication system and a high mobility of people. I would say that the coordination is not one of our major problems.

Chairman BENTSEN. Well, you have got a spat of R. & D. research programs that have been directed toward specific like mass transit, waste disposal, fire protection, and crime protection. To what extent have those yielded concrete improvements?

Now, Mr. David was talking about Morgantown. Do you have some examples showing that we have had some successes?

Mr. HORNIG. I cannot think of any remarkably successful examples. On a much larger scale, surely it is true that our nuclear energy program, which was very Government-financed, has resulted in economic nuclear powerplants. I think I would supplement the statement mostly by saying that our record in trying to produce civilian products as a result of the Federal effort has been marginal, at best. I think there is one conspicuous example of success, and that is agriculture. In agriculture, the Federal system engaged in an entire range of research and development, and I think agriculture should always be included among our very most successful high technology industries.

Chairman BENTSEN. And, yet, Mr. Hornig, back in 1955, we were spending 10.7 percent of the agriculture budget on research and development. We now find ourselves spending about 2½ percent of the agriculture budget on research and development. We have reduced it to less than one-quarter, percentage-wise, of what it was before.

Mr. HORNIG. I have not followed the matter in detail, but, offhand, that would sound unwise.

Chairman BENTSEN. Well, Mr. Hornig, thank you very much for your testimony this morning. I appreciate your attendance.

We have Professor Gilpin with us. Would you come forward please and present your testimony?

STATEMENT OF ROBERT GILPIN, PROFESSOR, WOODROW WILSON SCHOOL OF PUBLIC AFFAIRS, PRINCETON UNIVERSITY

Mr. GILPIN. Thank you very much for the opportunity to be here. Because the hour is late and my statement is long, I thought I would just hit the high points of my testimony as I have submitted a longer statement.

In the first part of my testimony, I point out that 10 years ago the major concern of people with respect to science and technology was the technology gap between the United States and the rest of the world, particularly Europe. Increasingly, however, in this country there is concern, and rightly so, over the relative decline of American science and technology. I say relative decline because, as Mr. Hornig has pointed out, American technology and science are still strong. Yet, relative to what our position was in the past, relative to what is happening in certain other countries, and relative to our national needs, I think there is reason to be concerned over the situation we have in science and technology today.

In my statement I suggested there is a role for the Government to play, but that the role of the Government should be dictated by the nature of the R. & D. enterprise. As one looks at that enterprise, there is a spectrum of activities in basic research through innovation and product development. I believe the Government's role is certainly at the end of supporting the basic research, and through experimental development; the Government's role is not in the area of commercial technological development and substituting its judgment for that of the entrepreneur and the market.

Chairman BENTSEN. Mr. Gilpin, some of the people in the back cannot hear what you are saying. Could you speak into the microphone, please?

Mr. GILPIN. I apologize.

The role of the Government, then, is one of supporting basic research, and there our efforts have declined. The other major area is through experimental development, so as to create basic technological capabilities in our society.

Now, if the Government is to play a role, the most important factor in determining the Government's role should be the nature of the R. & D. enterprise itself. Why do technological innovations come about? As I point out in the statement, in the past there has been an argument between economists and scientists on this issue, whether technology is coming from a technology push, or whether they come from demand pull. It is really both of these things—we need the market for success; but we also need the necessary scientific and technological capability.

Now, the Government can play its role in three important ways, I believe, in order to stimulate a high rate of technological innovation in our society. In the first place, the Government's role is that of supporting basic research and supporting what I call "on the shelf" technology. Twice we have found ourselves in a very difficult position because we as a Nation failed to develop "on the shelf" technology. The first was created by Sputnik when we found ourselves very far behind in space technology; and, of course, the second time is now when we find ourselves deficient in the basic technologies with respect to energy development, conservation, production—all of the spectrum of technologies that are required both for energy development and conservation and so forth. I think we are going to find ourselves in a third messy situation in the future when we pull out of this recession and find ourselves in an increasingly intensified international competitive situation, and we will find that we are going to have a difficult situation with respect to domestic civilian technological situations. So, the first role of the Government is that of a basic research, and so forth.

Second, the Government's comparative advantage is bringing technology up to the point of commercial development where market considerations then become of permanent importance, and the coupling of this new technology and the market is best done by private industry and not by the Government.

The Government is not a very efficient entrepreneur. As Mr. Hornig suggested a minute ago, he could think of no major spectacular examples of government entrepreneurship in the area of technology. The one case he mentioned, that of reactor development, is an interesting example for in that area the Government's role, with the exception of breeder reactors, has not been playing the role of entrepreneur. What the Government did is develop basic capabilities in the field of reactors, that is, developing different types of reactor possibilities; but the Government left it up to private enterprise to decide, on the basis of commercial criteria and markets, which were the most promising technologies to develop. And, it was precisely the example of the reactor demonstration program that I would say we should look to in what Government's role should be, and not to the breeder reactor where the Government has played the entrepreneurial role, and I am afraid it is not playing it very well.

The third area in which the Government can play an important role is that with respect to the market, and this is what Mr. David was emphasizing in his remarks. How can the Government through its budgetary policies, through its purchasing policies and other indirect means influence the market so that private enterprise will respond in terms of new innovations?

Now, in my recommendations I suggest that one of the things that is lacking in the Federal Government today is a capacity for evaluation of the effect of government policies on different industrial sectors, whether we are talking about housing, aviation, health, whatever, so that we have analyses of the ways government regulatory policies affect the incentives to innovate and the direction of technological innovation in various industrial sectors.

This is done very seldom at the level of the bureaus, and it is not done at all at the highest levels of Government. In other words, what is required is some sort of microeconomic capacity at the highest levels of Government to encourage studies with respect to how the Government and all of its policies impinge upon the private sector and its capacity to innovate. So, these are the areas, the three areas where I think the Government has a role to play.

First: basic research:

Second: In developing "on the shelf" technology, that is, basic capabilities so that we do not find ourselves in the situation where we found ourselves at the time of Sputnik or the energy crisis, and we may find ourselves in the future;

Third: Greater attention to the impact of Government policies upon the various sectors of the economy.

Beyond that I think, again going back to a point mentioned by Mr. Hornig in the field of agriculture—yes, agriculture is one of the important areas—and we have done very well; but I think one of the problems with agricultural research is not just the quantity of funds devoted to it, but very little of that money has gone outside the basic agriculture establishment; more of it should go into the universities and institutions of higher education outside the experimental colleges to do more fundamental research. This is the argument that all government bureaus should be supporting more basic research, especially research in universities and schools of engineering so as to broaden the base of support for science and technology in our country.

The last point is really something that is more of a sense of concern rather than any sort of recommendation, and that is that there is a danger as we become concerned over our technological situation, as we seek quick fixes to our energy situation, that we will tend to subsidize inefficient industries. We will tend to move in the direction of Great Britain in pouring vast amounts of money into industries which just do not meet the market test. So, it is really on this warning of what I think we should not do that I would like to close my statement. Thank you.

Chairman BENTSEN. Thank you, Mr. Gilpin, your prepared statement will be printed in the hearing record at this point.

[The prepared statement of Mr. Gilpin follows:]

PREPARED STATEMENT OF ROBERT GILPIN

Technological innovation in the civilian industrial sector of our economy is at a critical point. Partially due to policies pursued (or, rather, not pursued) by government and industry, and partially due to developments beyond our control, America's once unchallenged scientific and technological superiority has greatly deteriorated over the past decade. Ten years ago a major theme of discussions in this country and abroad was the technological gap between the United States and other industrialized countries; this gap still exists, though decreasingly so, in certain high technology industries: aerospace, computers, and, to a much lesser extent, atomic energy. But in the increasingly important technologies relevant for economic growth, international trade competition, and social welfare, the past decade has witnessed an historic reversal.

Ten years ago the United States enjoyed a large surplus in its international trade. In 1971, our trading situation had deteriorated to the point where we had the first trade deficit since 1873. This deficit was caused in part by the closing of the technological gap; the subsequent improvement of our trading position has required a large devaluation of the dollar to restore American competitiveness. The price of course which we have paid for the closing of the technological gap and for restoring a trade surplus through devaluation has been a lowering of our standard of living. More importantly, further deterioration of our technological position and further dollar devaluations will mean an even lower American standard of living.

A major factor behind this reversal in our trading position is the fact that the rate of American industrial productivity growth has declined relative to past rates of growth in this country and relative to our foreign competitors. Moreover, although this is very difficult to verify and is largely a matter of subjective impression, as our technology has diffused more rapidly abroad to foreign competitors, we have not adequately innovated new products for world markets. As a nation which has competed successfully in world markets, through a high rate of productivity growth and the introduction of new products, this decline is disturbing indeed.

Lastly, the past decade has witnessed a secular decline in our rate of economic growth; we have had one of the lowest growth rates among the world's industrial economies. Many distinguished economists fear that the underlying structural problems of our economy are such that even as we pull out of the present recession, we will be burdened by a growth rate insufficient to absorb our labor force.

Obviously the causes of these pressing economci problems are many. The relative decline of our technological position is but one of several explanatory factors, though I would argue it is an exceptionally important one. Obviously, too, the solution of these problems is a difficult and complex task involving much more than an improvement in our technological position. Yet, the upgrading of America's industrial and civilian related technological base is a necessary—and I fear, neglected—aspect of the challenge which faces us. As a consequence, there is a pressing need to initiate the necessary policies and create the policy mechanism to stimulate the technological innovations and industrial productivity required to help meet international economic competition, stimulate economic growth, and solve our domestic problems.

A national policy to foster and encourage a higher rate of technological innovation in the civilian industrial sector can be effective only insofar as it is consistent with and is based on what is known about the nature and process of innovation. Unfortunately, although economists have come to recognize the importance of innovation for economic growth, the economics of innovation is only in its infancy. There is very little reliable knowledge concerning the factors which tend either to facilitate or to retard technological innovation. What is known is highly tentative. In general, the findings of economists and others tell us what pitfalls to avoid rather than what should be done. Above all, what must be appreciated is that the primary factors which determine the tendency of a firm to innovate and for an innovation to be successful lie inside the firm and outside the reach of government policy. For this reason, the government's primary responsibility is to increase the technological opportunities and economic incentives to innovate (or to adopt more progressive technology).

THE NATURE OF THE R. AND D. ENTERPRISE

The R and D enterprise consists of three types of activities. The first is *basic research* which leads to the generation of fundamental knowledge about nature. The locus of such research is usually the university and a relatively few government or industrial laboratories. The second is *applied research and explanatory development*, relating to specific applications. Carried out principally in engineering schools as well as government and industrial laboratories, this type of R and D can entail activities ranging from the testing of new processes of prototype and pilot plant development. And, thirdly, there is the *commercial innovation* of new processes and products. Such activities are usually conducted in industrial laboratories where economic and market criteria are the major determinant of R and D activities.

As one proceeds along this spectrum from basic research to commercial development, the relative importance of critical factors changes. At the basic research end of the spectrum costs are lowest and uncertainty is highest. According to a rule of thumb, the cost ratio of basic research, applied research, and commercial development is 1 to 10 to 100. Conversely, scientific and technological uncertainties are reduced (or at least should be) as one approaches commercial development. Moreover, as one goes from basic research to applied research to commercial development, the relevant criteria of program planning shift from scientific merit to technical feasibility to market demand. Whereas basic research in universities and government laboratories tends to be wide-ranging and determined by "scientific" merit, industrial research and technological innovation are more focussed and keyed to markets.

The differences among the various types of R and D should be primary considerations in the development of a national policy toward R and D. They should determine the appropriate role of the various sectors of the R and D enterprise (university, government, and industry) and the locus of different types of decisions. Unfortunately, too frequently the comparative advantage of each sector has been neglected in the fashioning of national policy for R and D. With these considerations in mind let us look at the critical aspects of innovative process in industry and their implications for government policy.

THE COUPLING OF TECHNOLOGICAL INNOVATION AND MARKET DEMAND

For a long time, scientists and technologists, on the one hand, and economists, on the other, have argued over which is more important for successful innovation: the *supply* of new science and technology or the *demand* for new products and processes. At first, the best of the argument appeared to be on the side of the scientists and technologists. According to this view, innovations arise outside the economic process; they are exogenous factors which come about due to the advance of science and technology. The supply of new knowledge and technological opportunities are said to be the main determinant of rapid and successful innovation.

This view that innovation was outside the economic process and could not be explained by economic factors was challenged by the very detailed and painstaking researchers of Jacob Schmookler. Schmookler demonstrated that the primary ractor in successful innovation was market demand: The process of innovation was endogenous to economics and could be explained by economic factors. In effect, what Schmookler proved was the old saw that "necessity is the mother on invention."

There is support for both sides of this argument. On the one hand, scientific and technological advance open up new and unperceived possibilities. New technologies can create, if you will, their market. Certainly this has been the case with such radical innovations as the computer, the laser, and nuclear power. On the other hand, many innovations such as many in the area of machine tools even today take place without the benefit of new science or technical knowledge. More importantly, many products such as synthetic rubber or pollution-control devices were called forth by economic or social needs. It was market demand which resulted in the new technologies.

The truth of the matter appears to be that successful innovation involves increasingly a *coupling* or *matching* of new science and technology with market demand. New knowledge and economic need, to use Schmookler's analogy, are like the blades of a pair of scissors. They must be brought together or coupled by far-sighted and resourceful entrepreneurs of successful innovation is to result. Or, as another economist put it, "necessity may be the mother of invention, but procreation still requires a partner."

The importance of the coupling phenomenon for successful industrial innovation has at least three significant implications for government policy. First, although the government agencies may develop superior capabilities in scientific research and technological development, they are less apt to have a sense of market needs and potential. Secondly, the government's comparative advantage lies in advancing science and technology up to the point of commercial development where market considerations become of cardinal importance. And, thirdly, the government can play an important role in certain types of innovation through its influence over the nature of the market. But it should leave to private industry the responsibility for coupling national requirements with available technological possibilities. The government can influence both the pace and direction of industrial innovation most effectively through its influence on industrial, consumer, and public service demands.

The emphasis of both direct and indirect government intervention in the economy, therefore, should be to transform the market in ways which will encourage industry to innovate products of better quality and greater social utility. The government, however, should not substitute its judgment for that of industry concerning how these demands are to be met. But it should create the incentives and disincentives which will encourage industries to be more innovative in the use of their R and D resources. Thus, in the area of energy, the government should not decide whether this or that particular technology should be commercially developed but it should set standards and create incentives which encourage the efforts of industry in one direction or another. This is to say, through its regulatory and other policies, the government could do much more to couple user demand and technological possibilities. I will return to this subject in the latter part of my testimony.

GOVERNMENT FUNDING OF R. AND D.

In addition to its indirect role of influencing markets, the government has a direct role to play in the funding of scientific research and technological development. Unfortunately, in real terms government support for basic science has declined in recent years and in contrast to our industrial competitors, the United States has gravely underinvested in civilian technological development.

The basic argument for government financing of R and D is that certain market imperfections exist which result in a non-optimum level of private resources devoted to overall R and D or to specific economic sectors. In other words, for various reasons, there is an under-investment of private (university, industry, or agriculture) resources in R and D. These reasons usually involve the structure of the industry itself or a divergence between private and social interests. Among these reasons the more important ones are the following:

(1) The Public Nature of Knowledge

By its very nature, basic and certain types of applied research involve a very high degree of uncertainty both with respect to its results and its utility. Moreover, as the results of basic research and most applied research are made public, a firm cannot capture the results of its investment. Business corporations therefore have little incentive to invest heavily in basic and even applied research.

Aside from a few high technology corporations, a firm's primary purpose in conducting scientific research is to monitor basic research conducted in university and government laboratories. For this reason, most basic science is carried out by universities and is largely financed by the government.

While few would deny this responsibility of the government to fund basic research in science and technology, the tendency is to under-invest in basic science and technology. The desire for short-term and immediate payoff tends to predominate over the long-term need to increase the pool of knowledge.

(2) *Structural Aspects of Industry*

A second set of reasons for under-investment in both basic and applied research as well as in experimental development relates to the structural characteristics of industry. Oligopolistic industries, for example, may concentrate their resources on short-term improvements in existing products rather than in more risky and market-disturbing long-term innovations. Other firms due to technical, managerial, or organizational limitations may fail to appreciate the potential benefits of R and D. In certain critical industries such as housing, agriculture, and machine tools, the size of the firm or operation is too small and the industry is too fragmented to support an adequate research effort. Lastly, technical and market uncertainties may inhibit firms from investing in longer-term, radical innovations. The incentives in industry are biased in favor of short-term goals rather than the development of radical innovations.

(3) *Social and Political Needs*

A third category of reasons for government financing of R and D relate to society's social and political needs which cannot be met by the market mechanism or, at least, by the market mechanism unaided and/or influenced by government policies. In addition to military-related technologies, the government may finance R and D in high technology or politically sensitive areas for security of supply reasons. Atomic energy, aerospace, and electronics have largely been supported by the government due to their critical importance for a modern industrial system. With the launching of Project Indenpendence security of supply considerations have now been extended to many newer areas of energy production and conservation.

Other social and political reasons for government support of R and D include buyer protection in consumer goods (pharmaceuticals, food, transport, etc.). In other cases, government supported R and D may be justified in order to assess the external or social costs of new technology. Such technological assessment studies are carried out with respect to pollution, safety, public health, etc.

These considerations add up to the argument that in addition to basic research, government has an important role in financing scientific and technical activities when certain conditions exist. For various structural or financial reasons, the private sector may not be able to put a technology on a sound scientific basis thus requiring government financing of basic and applied research. At a more advanced level, industry may be unable to finance exploratory development activities and the testing of new products and processes. Additionally, market conditions may retard the development of a technology needed for social or political reasons.

What must be emphasized, however is that the role of the government should be restricted to applied research and exploratory development. The role of the government should not extend to costly commercial developments. As applied research and exploratory development are less expensive the government should spread its resources across a broad front. The government programs in support of R and D should be managed on an incremental, step-by-step basis, with the purpose of reducing key scientific and technical uncertainties to a degree that private firms can use the resulting knowledge to decide when (with their own money) they should move into full-scale commercial development.

This approach is the one that has been so successfully followed by the Departmentof Agriculture, the former National Advisory Committee on Aeronautics, and by the Atomic Energy Commission in the 1950's. In each of these areas the role of the government has been justified by such factors as the structure of the industry (agriculture), the need for exploratory research to reduce uncertainties, or the slow pace of the private sector in developing socially needed technology. In each of these examples, institutional mechanisms have facilitated cooperation among government, university, and academic laboratories with considerable success. With two exceptions the government has not attempted to finance commercial developments or undertaken the role of entrepreneur. The two exceptions are the breeder reactor and the aborted SST.

In general, most economists would accept the general principles that government should support industrial innovation when private firms are inefficient in the use of technical knowledge and when there is a divergence between private and public interest. However, application of these principles can lead to great abuse. They can become the rationale for completely inappropriate and extremely costly government initiatives in the area of commercial development. They become the basis for government subsidization of inefficient firms and ambitious projects of dubious economic merit. In particular, government funding of commercial developments is said to be necessary for the following highly dubious reasons:

(1) *The Scale Argument*

Undoubtedly the most important and pernicious argument for government financing of development projects is that certain modern technologies are so expensive to develop that the commercial capital market cannot mobilize sufficient funds. Usually such projects are justified on the grounds they will revolutionize the state of the technical art. Thus, the advocates of government funding of the supersonic transport (SST) argued that the development of the aircraft would revolutionize aircraft design and would have beneficial spillovers throughout the economy.

The arguments against the "scale argument" have been excellently summarized as follows:

> First, the commercial system in industrialized countries is normally quite capable of mobilising very large sums of money for civilian commerical developments: witness, for example, the IBM 360 series, the Boeing 747, the investments of the chemical companies in new products and large-scale processes, and of the oil companies in under-sea oil exploration and extraction. Second, if commercial money is not forthcoming for full-scale development, it is usually because entrepreneurs do not think that the technology, the market, and/or the management is such that an adequate rate of profit will be made. Third, government money invested in commercial development projects will therefore either be a substitute for industrial money, or invested in second-best projects, given that governments are not in a position to make better guesses than industrial firms about future technical and commercial prospects. Fourth, once governments invest in second-best commercial development projects, it becomes difficult to stop them, because of public commitments and of political lobbies and pressure groups. Fifth, this will lead to good money being thrown after bad, and to a degradation of the public service, then becomes an advocate of commercially questionable projects. Sixth, it will also lead to the degradation of the commercial capacity of private firms involved in such projects who devote their resources and their skills to political lobbying instead of to production and marketing. Seventh, the arguments that a low commercial rate of return is compensated by "externalities" such as exports and the general upgarding of industrial technique (which are generally invoked in the later stages of projects as they come under mounting criticism) are spurious; there is no reason to believe that industry-financed commercial developments produce such "externalities" to a lesser degree.

(2) *Security of Supply*

A second dubious justification for government financing of commercial development both in this country and abroad is that certain high technologies are of strategic importance. For industrial or military reasons, it is said, the United States should not be dependent upon a foreign source, or should not fall behind technologically. In the past, these arguments have been applied most forcefully to aerospace, electronics, and atomic energy. With the advent of the energy crisis and the launching of Project Independence, the security of supply argument has been applied to a broad spectrum of energy development: atomic energy, coal gasification, solar energy, etc. The security of supply arguments were also used for the justification of government financing of the commercial development of both the SST and the breeder reactor.

The economic criticism of the scale argument is less applicable to the security of supply argument. It may very well be that for political or military reasons, there is justification to invest funds in a project which cannot be justified by commercial standards. But as in the case of the SST, one must be wary of this argument. Many sins are committed in the name of "national security." At the least, each such project must be examined with great care. But beyond this case-by-case approach, the experience of the past several decades suggests great caution should be exercised in financing costly development for security of supply reasons. More importantly, lower cost alternatives are frequently available.

In this country and abroad the security of supply argument as applied to costly high technologies has led to incredible wastage of scarce R and D resources. In the three Western economies where it has had most influence—the United States, Great Britain, and France—it has had deleterious consequences for the overall health of industrial technology. The history of the past thirty years is strewn with costly high priority, low payoff projects all justified by security or prestige reasons: Concorde; Apollo; European satellite and launch projects; UK, Swedish, and French atomic projects; and a host of computer, aerospace, and electronic projects. Only the Japanese, West Germans, and the smaller advanced industrial countries have had the good sense to refrain from such temptations.

The recent history of ill-fated and extremely costly development projects undertaken for security of supply reasons suggests that several alternatives be considered. At the least such projects should be carried out on an incremental and step-by-step basis. The psychology of the "crash program" involving long-term and irreversible commitments should be avoided. It is extremely risky, as the British and French have discovered in their joint Concorde project, to make firm long-term commitments too early when technical, commercial, and political uncertainties are extremely high. It is best to wait until research and time have reduced the level of uncertainty before making a commitment to commercial development. The same caution may apply to many of the projects presently being considered in the United States today in the area of energy.

A second alternative is to borrow a leaf from the book of military R and D. This is the concept of basic capabilities research. The Department of Defense and many corporations support a very broad range of applied research and exploratory development in order to have the basic technology "on the shelf." The technology is there if it is required for full-scale development and production. Only sporadically has the United States outside the military area followed this concept of "on the shelf" capabilities. This policy was followed by the Atomic Energy Commission in its reactor demonstration program. But lacking this "on the shelf" concept in the area of civilian technology on at least two occasions the United States has found itself dangerously deficient in basic capabilities. The first was after the launching of the Soviet Sputnik when the United States discovered it lacked the applied mathematics, high heat resistant materials, and propulsion technology to launch its own space program. The other occasion is the present situation with respect to energy.

Over the past several decades we have basked in the euphoria of low cost energy supplies from overseas. As a consequence, there was little incentive or foresight to develop basic capabilities in new forms of energy production, storage, and conservation. We now find ourselves hobbled because we have failed to do the necessary applied research and exploratory development in a number of critical areas: coal gasification technology; energy storage and conversion systems; and unconventional methods for energy production. Most notable perhaps was our neglect of research on coal technologies: coal gasification, the manufacture of fertilizers from coal, the removal of sulphur from coal stack gas, etc. Despite our incredible reserves of coal, the methods for its recovery, combustion, and conversion remain primitive. Similarly, too little was done to advance fusion and solar energy or to develop energy storage technologies. How difficult our present situation would have been if we had developed "on the shelf" technologies in coal and other areas.

As we move into a highly uncertain future, the likelihood of other surprises comparable to Sputnik or the energy crisis is fairly great. Domestic or foreign events may necessitate the development of new technological capabilities. To be prepared, the United States must undertake basic capabilities R and D across a broad spectrum of science and technology. While this type of research is fortunately relatively cheap, such a program of "on the shelf" technology does necessitate the development of institutional mechanisms which can identify and support scientific and technological areas where America's basic capabilities and "on the shelf" technology are judged to be deficient.

THE NEED TO COUPLE TECHNOLOGY AND ECONOMIC POLICY

In addition to the adequate funding of basic research and experimental development, our most important need is to couple technology policy with socio-economic policy. At all levels of policy-making and across the broad spectrum of government activities, technological options and user-needs (or market-demand) must be brought together and integrated in policy-making. Such a recommendation seems self-evident and easy to accomplish. In fact, the coupling of these two aspects of government policy is too seldom achieved. While in theory it is easy to do, in practice it is exceptionally difficult because of institutional commitments and lack of sufficient knowledge. Among modern societies the Japanese (and perhaps the Germans) have been most successful in creating the institutional structure and national policies to integrate technology and economic policies in order to achieve their professed goal of rapid economic growth. In the case of the United States and Great Britain, although both societies have the richest scientific and technical resources in the non-communist world, they have both been much less successful in integrating technology policy into the larger framework of socio-economic policy and national goals, whether those goals be a more rapid rate of economic growth, more competitive exports, or a cleaner-safer environment. It is, therefore, imperative that we improve our ability to couple technology and our goals. Although technology alone cannot solve our problems, it is today a central ingredient in economic growth, competitive exports, and the solution of domestic problems.

The coupling of technology and user needs requires that government programs be problem-oriented. The need is to think in terms of problems (user needs or demands) which require solving and to ask how technology could help solve the problem. What are the technological roadblocks, if any, to solving recognized economic and social problems, and what can the government do to remove those roadblocks? It may very well be that the roadblocks are economic, legal, or social, but technological roadblocks may be identifiable. For example, what are the technological roadblocks to the development of such socially recognized needs as a less-polluting automobile engine, better construction methods, and energy-storage?

The emphasis on problem-solving, user need and demand-pull should carry over into all aspects of government policy-making. In particular, the following question must be asked: How do government policies and regulations influence the direction and character of technological innovation and productivity in the private sector? Or, to put it another way, how could government regulatory, taxation, and other policies be used in order to influence technological innovation in a socially desirable direction? As government economic and related policies are among the most important determinants of civilian technological innovation and industrial productivity how these policies affect the behavior and efficiency of industrial firms should be a major concern of government policy.

Unfortunately, we do not know very much about the net impact of government policies and regulations on specific industrial sectors. Moreover, purchasing, regulatory, and related policies affecting an industrial sector are frequently made by different agencies with little concern given to the overall effect on innovation and productivity. Nor does any agency seek to take a more olympian view of the overall impact of government policies on the innovative behavior of industry.

In response to this situation one economist has proposed a research-impact statement whenever a government agency undertakes a major action. Thus, the Interstate Commerce Commission should consider the impact of its rate structure on the railroad industry's incentives to innovate. The same type of evaluation should be required of the Food and Drug Administration, Environmental Protection Agency, General Services Administration, etc. At the least, these exercises would raise the "innovative consciousness" of the agencies dealing with specific industrial sectors.

In making this recommendation one is brought face-to-face with an extremely important lacune of government policy-making. The problems of R and D are those of micro-economic policy, that is, of that aspect of economies which deals with the determinants of prices and outputs of individual goods and services. In effect, what economic policies should be pursued in order to obtain a particular good (technological innovation) and service (scientific knowledge)? Yet, there is no government agency which was the primary responsibility for overseeing the micro-economic policies of the government, such as they are. For example, although the Council of Economic Advisers does devote some attention to micro-economic policy, its responsibility is primarily that of macro-policy, i.e., the general state of the economy. While one might expect regulatory agencies to be concerned with the micro-economic policy aspects of the specific sectors they regulate, that is, concern over the effects of regulatory policies on the output of goods and services, this too is seldom the case. In short, at both the highest policy level and at the operational level, the government lacks an adequate micro-economic analysis capability.

For this reason, my principal recommendation that economic and technology policies must be coupled and integrated confronts the fact that micro-economic policy hardly exists. The fundamental need therefore is for the federal government to develop a greater capability for micro-economic policy. In contrast to the present situation there must be a greater government focus on micro-economic aspects of the American economy. From this perspective, R and D is but one of several "goods or services" whose output must become the concern of a greater government emphasis on micro-economic policy and its impact on the private sector.

The need, therefore, is for a new or renovated government agency which would assume the leadership for improving the overall competence of the federal government with respect to micro-economic analysis and policy-making. If a high level agency were to carry out this mandate, it could undertake or support the types of studies and experiments which would encourage a higher rate of technological innovation. Such an improvement in the government's capacity for micro-economic analysis would not only be a major step forward in its own right but it could go a long way toward the achievement of the major recommendations of my testimony: the need to couple economic and technology policies.

In theory, the Office of Management and Budget could provide the leadership with respect to micro-economic policy. However, as its relationship to other agencies is frequently an adversary one, its utility in encouraging micro-economic studies at the agency level is undoubtedly limited. The Council of Economic Advisers or the Domestic Council are other possibilities. Or a Productivity Council with responsibility for sponsoring micro-economic studies might be created.

The important point, however, is not whether this or that reorganization of administrative responsibility is to be preferred. It is rather that micro-economic analysis in general and the coupling of technology and economic policies in particular should be given a much higher priority by the federal government. This could be achieved if the highest levels of the Executive Branch recognized this need and encouraged the type of approach to problem-solving being suggested here. What is important is the attitude that the government takes with respect to technological innovation and its important role in solving our social and economic problems.

A NATIONAL STRATEGY FOR SCIENCE AND TECHNOLOGY

For many scientists, engineers, and industrialists the idea of government priority-setting is anathema. Scientists in particular believe in the right of the scientist to do his own thing; he is among the last of the rugged individualists in this world. Fortunately, or unfortunately, science and technology have become too important to leave to the scientists and engineers.

The issue, however, is not whether the government will or will not set overall research priorities. It obviously does. A glance at the distribution of R and D funds over the past several decades and the heavy emphasis on "big science and technology" related to defense and prestige clearly indicates what these priorities have been. But we now confront a new set of national problems and require a revised set of national priorities for R and D. The questions we must answer include the following: What should be our R and D priorities? How should they be determined, and by whom? In short, we must confront the issue of priorities for R and D much more consciously and systematically than we have done in the past.

In the light of our pressing national problems the United States must develop an overall national strategy for science and technology. In the first place, we must develop the necessary mechanism by which to determine national priorities for R and D and the means by which to achieve them. The details need not be specified here. Such a mechanism for establishing and implementing a national strategy for science and technology could, as some have proposed, parallel in structure the Council of Economic Advisers. Or, it could be a rivival of the President's Science Adviser and Office of Science and Technology. The important point is rather that we require a better mechanism than we presently have at the highest level of government to ensure the health and vitality of American science and technology.

Secondly, a national science/technology plan or set of R and D priorities should be in the nature of targets and the setting forth of the direction in which we should move. They should be provisionary and capable of revision in the light of scientific and technological advance. In contrast to the type of planning that characterized the Apollo project (i.e., to land a man on the moon), incrementalism should be the primary characteristic of such a plan. As science, technology, and national objectives change, so must the features of the plan. For this reason, there is undoubtedly merit in the idea that the Council of Science and Technology Advisers submit an annual report to the Congress and the public in order that national R and D priorities can be reviewed and debated publicly. In short, the purpose of the R and D plan and annual report would be to set the agenda for a more conscious and systematic evaluation and discussion of national priorities rather than the establishment of hard and fast objectives.

Thirdly, the purpose of government policy should be to support and advance national capabilities in science and technology. On the level of basic and applied research, this means the broad support of university and, where appropriate, industrial as well as in-house government research. The identification of knowledge-gaps and promising opportunities would be a major responsibility of the science and technology policy mechanism. Both the scientific and technical communities could be brought into this effort through panels of experts, special task forces, etc. Beyond its support of basic and applied research, the government should fund experimental development. The identification of important social and economic technologies neglected by the private sector and the stockpiling of "on the shelf" technologies would be a major government responsibility. Unless a powerful case could be made, however, the government should not become involved in the commercial development of technological innovations. The major task of the government in the area of technology is not to supplant private enterprise but to complement it through research and experimental development programs which reduce uncertainty; it should only undertake those tasks which market and other imperfections inhibit industry from doing.

And, fourthly, more government agencies should be encouraged to develop R and D strategies and support basic research, experimental development, and graduate education in universities and schools of engineering. Too many government agencies (including Agriculture) tend to concentrate their support in their own laboratories and don't draw sufficiently upon the large reservoir of talent existing in institutions of higher learning. A new alliance must be forged between the agencies responsible for achieving our emerging set of national priorities and the American scientific-technical community. A major step in this direction would be taken if ERDA followed the example of NASA and supported a broad program of engineering studies and assumed part of the responsibility for replenishing our most basic resource—the supply of engineers and scientists.

Despite the pressing need for all these measures, I must conclude on a note of caution. The establishment of research priorities and an emphasis on more technology planning should be undertaken with a full appreciation of the limitations of such an effort. In the establishment of R and D priorities and the emphasis on integrating economic and technology policy, a potentially serious danger must be avoided. Behind the calls for more economic planning in the United States today as a response to our many economic problems, one fears there may lurk the desire to protect rather than to rejuvenate the American economy. The protection of existing industries and markets rather than the creation of new industries and markets could too easily become the purpose of government policy.

[There is a grave danger in the United States today that government decision-making and industry-wide, quasi-cartels could be substituted for the operation of the market mechanism. The temptation to use technology policy as a vehicle to supersede the discipline of the market is considerable. Employing an array of arguments—the threat of foreign competition, scale of technology, security of supply, the need for a quick solution to our energy problem, etc.—the proponents of greater government funding of commercial innovation urge the government to assume the role and risk of entrepreneurship. The proposals to revive the National Recovery Administration (NRA), to subsidize industry with public funds, and to put a floor under energy prices to stimulate innovation move in this dangerous direction.

If we were to move in this direction of subsidizing and protecting inefficient industry we would be following the British rather than the Japanese example. Invoking a variety of rationalizations and policy mechanisms, the British have subsidized and protected inefficient firms producing commercial innovations which the market would not accept. The Japanese, on the other hand, have been ruthless in eliminating inefficient firms and have stayed clear of government subsidization of commercial innovation. How unfortunate it would be if the United States in seeking to emulate "Japan Incorporated" fell into the error of "British Incorporated" instead.

In conclusion, I propose no panacea for the problem with which I began: the relative economic and industrial decline of the United States. What I have sought to do is to stress the importance of technological innovation if we are to grow economically, complete internationally, and meet our domestic social needs. Beyond this, I have set forth the direction in which we must move if we are to improve our innovative capacity and to use technology for socially and economically beneficial ends.

Chairman BENTSEN. Along that line, Mr. Gilpin, in your prepared statement you urge that the Federal Government provide a shelf of basic research, and that firms might be able to pick and choose among those items of research for commercial development.

What kind of criteria would you have for the Federal Government to determine what kind of basic research to engage in?

Mr. GILPIN. I think on basic research, the responsibility there on that end of the spectrum lies primarily with the scientific community. Where basic research becomes very expensive, such as high energy physics, and space and that sort of thing, of course, the Government has to step in and make some very basic decisions with respect to allocations. But, at the end of the spectrum where basic research is, I would leave it to the scientific and technological community. As one moves over toward the applications of research, then the role of the Government is to decide national priorities. For example, we should have had certain decisions with respect to energy development a long time ago; there were certainly warnings in the area from the scientific community that we were not building a base in energy for the long term. There were reports of the President's Science Advisory Committee to this effect, and one of the problems is we do not have a mechanism in the Government at the present time—I should have included this in my recommendations—we do not have a mechanism for making these determinations of priorities, targets, whatever you want to call them with respect to experimental technology.

Chairman BENTSEN. Somewhat along those lines, in its report for 1972, the National Science Foundation recommended that key technologies essential to the attainment of social goals were not presently commercially viable. They recommend the development, strengthening, and renewing of government-aided research and development.

How do you view this formulation of the role of Federal funding? Does that not call for the Federal funding of a wider array of hardware development that might not be commercially viable?

Mr. GILPIN. Yes. I think what they are talking about in the Science Foundation report is that there are not the basic technologies which are exploitable. In other words, let me put it in my own terminology, that we let the cupboard get rather bare with respect to technology, and that is the basic problem.

Again, to go back to energy, we have not developed these basic capabilities and much of the new energy technologies we are now using were spinoffs, fortuitous spinoffs, from the space program.

As I understand the science report, what they are saying is we have, as a nation, not done enough to build up our basic technological inventory, and that this is a fundamental problam.

Chairman BENTSEN. In your prepared statement you talk about the United States not developing new products for world markets, the evidence in the last century is that we have computers, we have the laser communications programs, nuclear reactors, consumer durable appliances—have we really done as badly as you suggest in that regard?

Mr. GILPIN. I would have to reread my prepared statement, Senator. I do not think I said it quite that badly. Where we have done well is in precisely the areas you are talking about, really high technology areas. We have continued to do fairly well in high technology areas, although even there, if you look at our terms of trade, our trade surplus has declined. It went into deficit with Japan; and, if you look for example at consumer technology, we have had a deficit. In the past we have had a surplus in consumer technology; we now have a deficit in consumer technology. We have been strong in machine tools.

Chairman BENTSEN. How do you arrive at saying we had a surplus, now we have a deficit? How do you determine that?

Mr. GILPIN. This is done through breakdowns of American imports, exports from the various sectors, and dividing up imports in terms of high technology and low technology, agriculture, machine tools, and so forth. For example, automobiles would be by this classification, low technology, where, of course, we have had a major deficit primarily through smaller automobile imports; in consumer goods we have not been as innovative as we have been in the past. Fortunately we have stood up well in certain high technology industries including agriculture as a high technology industry. But, where we have brought our trade back into balance is through two major devaluations of the dollar. In effect we have paid a price in terms of our standard of living and to the consumer.

Chairman BENTSEN. Would you agree that the recession has had a very major impact upon research and development for commercial firms?

Mr. GILPIN. Does it? Yes, for two reasons. One is that most firms, because of their own profit squeeze, have cut back on their investment in R. & D.; and second, because of the importance of market, and the markets have rather diminished. The firms do not have the incentive. So, both on the supply and demand sides there has been a tendency for firms to cut back their R. &. D. expenditures.

Chairman BENTSEN. You referred to the Mansfield amendment on the 1971 Military Procurement Act. We had a limitation put on that act that research could not be funded that did not have some military application. Before that, were there a number of things funded that did not have military application but had a significant application in the development of new products? Well, let me ask Mr. Hornig.

Mr. HORNIG. In the course of military applications, the DOD funded in a major way the basic research in this country. It acted like a very farsighted National Science Foundation before there was a National Science Foundation, at least prior to the Mansfield amendment. But I would say yes, the spending by the Department of Defense was very largely responsible for the development of the technologies which underlay the computer industry, for example. But the electronics industry in general and telecommunications are much the same.

Chairman BENTSEN. Would you agree with that statement, Mr. Gilpin?

Mr. GILPIN. Yes. I would say that, in a sense, beginning as far back as 1945, with the Office of Naval Research, the basic patron—or the patron of basic research, and much of the experimental development in this country, has been the Department of Defense. And contrary to all the charges against the Department of Defense, it has been fairly generous, and has not had many strings attached with respect to the universities. One problem is that there is an increasing divergence between the types of technologies that the military desires and the needs of the civilian economy. So you do not find as many cases like the computer today as we used to have in the past, but we do have other examples, such as the basic work at Bell Labs on the transistor. Also our major aviation developments—the 707, the 747—so that you have had a considerable spinoff.

Now, the Congress would look at that and say, it is not on a cost-effective basis, and has been very costly. But in the absence of other incentives and basic Government policy for experimental development and sufficient funding of basic research, the military filled in very well.

Chairman BENTSEN. We are running out of time. Let me ask you just one more question; I am concerned about the availability of scientific manpower. The 1974 report of the National Science Foundation states:

> It is anticipated that the Federal program of energy R. & D. would employ some 40,000 scientists and engineers and technicians when the program became fully operational. In 1973, about 50 percent of that number were employed. Manpower requirements in the private sector are substantially greater. A maximum effort by industry to develop domestic fuel sources for the next decade was estimated to require 230,000 scientists and engineers by 1980, and 308,000 by 1985, compared with the 141,000 in 1970. The supply situation will become considerably worse beyond the mid-1970's if the current trends continue toward an overall decrease in the number of graduating physical scientists and engineers.

Now, do you think we can handle an engineering challenge of this magnitude in a timely fashion, or are we facing another shortage of skilled personnel, as we did in the sputnik era some 15 years ago? I would like for each of you to comment on that, if you will, and then we will terminate our hearings today.

Mr. HORNIG. I have not studied the matter closely, but all the projections I have seen suggest that we are moving into a shortage area with respect to scientists and engineers in the years ahead.

Chairman BENTSEN. All the studies show that there will be a shortage?

Mr. HORNIG. Yes, and I think in light of this——

Chairman BENTSEN. What do you do about student counseling? Are they encouraged to go into this field?

Mr. HORNIG. Students are remarkably sensitive to what they see as the current demand. This has been particularly true in engineering, and it is not so much where counseling can help. They are most sensitive to what they see happening to recently graduated seniors, and this contributes to very great instability in the number of people.

You know, during the 1950's, we had this same phenomenon of a serious drop in the number of engineers. It then picked up very rapidly with Sputnik, and has been dropping again recently. And I think one will have a turnaround when students are persuaded that the jobs are available—not in the 1980's, but in 1976 or 1977, when they graduate. One thing which has aggravated the difficulty is that the Federal Government has eliminated almost all federally sponsored graduate fellowships; and this, to me, is a major policy failure. It is based on the expectation of surplus graduate students in the teaching profession, which is very true, including college teaching. But it neglects completely the fact that there are many other areas—energy is one—in which we are trying to attract people. The danger in eliminating fellowships is the failure to attract the very most able and the best people into the fields, quite aside from total numbers.

Chairman BENTSEN. Would you care to comment on that, Professor?

Mr. GILPIN. Just briefly. I agree entirely. As an example of a case in point, let me cite the field of energy though my information may be out of date. But my understanding is that the ERDA has not assumed the responsibility for graduate education in energy technology. From people I have talked to, there is extremely little. There are very few energy engineers at the present time being graduated in this country, and there are no fellowships in this field. NASA did assume responsibility for training engineers in the space technology fields. The Atomic Energy Commission has not, and one of the concerns that people have with ERDA is that the philosophy of AEC, rather than the philosophy of NASA, will prevail, and they will not, in a sense, replenish the supply of engineers, or even create the supply of engineers that are going to be required to develop the technologies in the 1980's and 1990's in this country.

Chairman BENTSEN. I think that is a highly significant statement that we ought to be concerned about, and direct our attention to.

Gentlemen, thank you very much, and I apologize again for the interruption in the hearing.

Mr. Wiesner's prepared statement will be included in the hearing record in its entirety, and tomorrow's hearing will be held in room 1224.

[The prepared statement of Mr. Wiesner follows:]

PREPARED STATEMENT OF JEROME B. WIESNER, PRESIDENT, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Mr. Chairman, members of the Committee, I am pleased to have this opportunity to appear before you to comment on matters relating to the health of the U.S. technological industry and related scientific activities. I am very pleased to have an opportunity to do this, for it is a question that I have thought about very seriously during the past several years as I have tried to understand the problems of our society and the complicated role of technology in the search for solutions. I am convinced that there are good technical solutions to the serious problems we face today but that it will be increasingly difficult to realize them because of the complexity and scale of our present technical enterprise and because of the increasing amount of social interaction that has become necessary before any new technology can be exploited. This is not necessarily bad, but it does add vastly to the time and cost required for a new technology.

I plan to cover a wide range of subjects and so will touch each of them but lightly and then perhaps you can ask me more about those of interest to you during the question period. I am mindful of the fact that I am not unbiased, perhaps in several ways. First of all, I am the president of a large research-oriented university and so the financial problems of the research community are very much on my mind. Second, for many years I participated in decisions regarding the R&D activities of the Federal Government, including those of

D.O.D., NASA, HEW, Department of Commerce, N.S.F., and the White House. I helped create some of the procedures and, I suppose, some of the problems. now under discussion, as for example, the White House advisory system, and so perhaps I am more disposed to believe in their potential effectiveness than some others might be. Thirdly, I am involved with several large industrial concerns so I see matters from their point of view as well.

In this testimony I will talk first about the questions of industrial research and industrial productivity to the extent that I can, and then I will discuss federally-sponsored basic and applied research. Finally, I will say jus a bit abount the social setting that I see as a major question.

I believe that the nation—and for that matter, the world—is in a major crisis whose solution requires, among other things, a high level of imaginative new technology as well as more thoughtful exploitation of existing technology. For this reason, I am concerned by the fact that it is becoming ever more difficult to create and exploit new technology.

You asked for my judgment regarding the adequacy of the nation's over-all R&D effort, public and private. I will confess at the start that I have not had enough time to collect an adequate amount of relevant information nor do I have the expertise to comment on many aspects of our national technical effort. In particular, I have not seen the details of recent D.O.D. R&D budgets, though I have general familarity with them and they do play a major role in the direction and stimulation of the U.S. technological efforts.

I find it extremely difficult to sum up the situation of U.S. civilian technology with respect to that of other leading industrial nations. First of all, the situation varies from field to field; second, this is a rapidly changing situation and in most fields the rate of change is much more important than the present situation, and this is difficult to measure or even judge; third, many non-technical factors—economics, suitability of the technology for the local situation, political posture, etc.—are important in decisions to purchase technology and may give quite wrong impressions regarding the underlying technological situation. For example, I believe that U.S. nuclear power reactors are more advanced than those available from Germany or France, yet many countries are choosing the latter countries products for economic or political reasons.

There are several useful criteria by which we can judge the state of an area of civilian technology. First of all, the state of the underlying applied science upon which it is based is an important factor. We can ask in this regard, for example, how good and how avalable are the materials needed for the realization of a given technology? How advanced are the processes to be exploited? How good are the auxiliaries such as controls, how sophisticated the designs?

A second important question is how well the technical industry is able to use the available know-how in its designs. A third important question is how quickly industry can bring into being a new design; do they lead or follow? An overriding question relates to the economics of the situation; is the new technology price competitive?

I will give you my impressions regarding some of these matters, but you must realize, as I implied earlier, that I have not made a profound study of these questions. What's more, there are people in this country who are experts on this matter and who could give you more substantial answers.

I believe that on the whole U.S. technology, particularly so-called high technology, is equal or superior to that found in other nations. By high technology, I mean such fields as electronics, applied physics, computers, scientific instruments, medical techniques and pharmaceuticals, aeronautics and astronautics, chemical processes, sophisticated materials, automation and automatic control, communications, agriculture and related technology, and perhaps some other fields that I have left out. In saying this, I don't mean to depreciate the technological accomplishments of other nations but to indicate that in these fields the quality and scope of the U.S. efforts is such that our industry and research efforts continue to set the pace in the creation of new understandings, new ideas, new applications and in their reduction to practice. In many other fields such as ship building, the automobile industry, the clothing industry, consumer electronics, consumer optics, other nations equal or surpass our capabilities.

In many fields the superiority of U.S. technology is decreasing and from an economic point of view this will present a serious problem in a very few years unless reversed. This change in relative quality of U.S. technology, vis-á-vis Europe and Japan is probably due to a number of factors, but primarily to the fact that other countries have made a major effort to build up their scientific and technological capabilities while in the United States, numerous obstacles to change and progress have grownup. To the extent that our technology fails to develop, we face not only the prospect of losing export markets and perhaps an increasing demand for foreign products, but even more serious, we may fail to create and put into production new processes and products needed to sustain the quality of our society. The sluggishness of our response to exciting technical opportunities in the energy area is an important example of this situation.

In my travels abroad, I have observed that U.S. technology is still almost universally held in the highest regard in both the capitalist and socialist nations, U.S. industrial and scientific activities are the standards by which other nations judge their efforts and which they would emulate and surpass. Frequently even when the technical products and plants they purchase are obtained elsewhere for political or economic reasons, the reference point will be U.S. designs and capabilities.

Our problem then, I believe, lies in the changing relative position with regard to the technical industry of other countries and if we want to take corrective action, we need to understand what factors effect the role of creation and introduction of new technology. To some degree, a diminution of the vast U.S. technological superiority that existed immediately after World War II was inevitable and even desirable for the European countries and Japan had been devastated by the war, while the development of U.S. industrial and scientific establishments had been stimulated—indeed, forced. In fact, the bulk of American accomplishments were based on the strong support provided for military and space programs at all levels including basic research, applied research, development and production. American industries can be divided into two distinct groups, those that were stimulated and supported substantially by the federal funds and those that were not. In general, those that were so supported learned to exploit research results and developed a unique capability and group of products while those not so supported did not generally become innovative in a technological sense. There were, of course, exceptions to this generalization. The chemical industry, for example, has had a long history of process and product innovation. The synthetic materials they produce have constantly been improved and productivity of the industry has improved steadily for many decades. I don't know why this has been the case. Perhaps because the chemical industry has always been a highly technical, capital-intensive industry, in that there is a great deal of competition, not only among producers within the industry but with producers of natural raw materials such as fibers, metals, woods, etc. as well. The agricultural field has been highly innovative and here the stimulation for improvement came from governmental initiative.

In general, and for many years, federal support of R&D had a definitively negative effect upon those sectors of civilian industry that were not associated with governmental programs because the latter drove up research costs, particularly salaries. For many years it was almost twice as expensive, per scientist or engineer, to do R&D in the United States as it was to do similar work in Europe and in Japan. As a consequence, one frequently found roughly half as many people working on comparable projects here as abroad. This situation has now changed—costs are now comparable—but we have not yet seen a significant positive reaction from this change.

Many civilian industries, particularly those in which the individual units were relatively small, as for example housing or machine tools, have never invested heavily in R&D. Governmental efforts to stimulate R&D activities in such fields have had only limited success for many reasons. First, R&D though costly, represents only a fraction of the total cost of developing a new product and taking it to the marketplace so that even when ideas for new products have been stimulated, the target industries have not had the resources needed to develop and exploit them.

Several factors have played a part in changing the relative position of U.S. high-technology industry, vis-a-vis that of other countries. The most important of these, I believe, relate to governmental actions, pro and con and to some extent, therefore, should be under your control.

Perhaps the single most important fact is that several countries have undertaken to support their technological industries through R&D funding, manufacturing subsidies, purchase of products, credit to foreign purchasers, direct sales help, and a variety of other forms of assistance. Germany, France, England and Japan have all done this at various times and for various industries. These countries have sought to build up aircraft, electronics, computer and nuclear power activities. In particular, the subsidies in the aviation and computer fields have been very substantial while here in the U.S. government support, provided indirectly, has not kept pace with the increased costs of development or readying for production.

In the U.S. there has been a leveling off of federal spending for R&D and this has effected adversely the development of and the market for new instruments and other sophisticated apparatus.

The Japanese government has been making a major effort to become the dominant force in the computer field. It has created a large R&D effort in industry where a vowed purpose is to surpass the American leadership in the computer field. It is also creating a vast R&D program to dominate the production-automation field, a move, which if it succeeds, would give Japanese industry a tremendous edge in manufacturing efficiency over industries in other countries.

Meanwhile, here in the United States, the forces have mostly conspired to slow down the pace and quality of innovative industry and in fact to slow down productivity increases generally.

For a number of years there has been inadequate capital for new plant construction so that in some important industries, steel or shipbuilding for example, the available technology has not been fully exploited. Under these conditions there is little incentive to invest in sill more advanced technology. I understand that the rate of saving in the United States which could contribute to capital formation is approximately ½ what it is in the nations we are concerned about.

Even in areas where the U.S. government does sponsor a substantial level of R&D, its policies do not always stimulate industrial initiative. For example, government patent policies, which seek to make the fruits of governmentally-sponsored R&D generally available, tend to inhibit private investment in the exploitation of new technology, particularly in industries that do not require manufacturing sophisticated know-how.

In other cases, the government influences and sometimes even dominates, the market decisions so that it is not possible for a businessman to judge the outcome of an investment he is contemplating. This is true, for example, in the drug field, and the field of agricultural chemicals where governmental agencies are attempting to deal with health hazards posed by the products of these industries. The goal is obviously desirable, but it must be realized that the consequences are not the desired ones—at least I don't believe hat the objective was to dry up the flow of new drugs and chemicals. But the high costs, the delays and the uncertainties introduced by the present procedures may make private investment in R&D in these fields unattractive for all but the very largest companies. Perhaps the government should find a way to share the cost and the risks that regulation and testing have added to the development costs of new products. Incidentally, these problems exist in many other fields in addition to the ones I chose as examples.

There is also some labor resistance to new technology, especially that which would drastically change the nature of work in a given industry. This is a real and understandable problem which is dealt with systematically in some of the other countries we are concerned about. In France and the Scandinavian countries, the government provides programs for reeducation to meet change and funds for re-location of labor if it should become necessary. Such programs would help to mitigate labor fears regarding new technology in industry.

Turning now to the problem of fundamental and exploratory research I would note that with the exception of energy-related areas, where it is to be hoped that wise handling of the new funds will permit new initiatives, the federal R&D budget for FY '76 will probably result in further retrenchment of ongoing activities and increased financial pressures on research-intensive institutions like the one I heard that have been forced to retrench for the past several years. In real dollars, R&D funds have not been keeping up with costs, and in addition student support provided by the federal government has been drastically cut. As an added burden most institutions have also had to finance federally and state mandated programs which add to the cost of doing research. These include much more elaborate animal care facilities, growing controls on human experimentation, many more small grants to be managed, more elaborate fiscal controls, equal opportunities programs for women and minorities, privacy programs, all added upon familiar general inflation and out-of-control energy costs. Most of the mandated programs are highly desirable and we support them *but* because no financial help accompanies them, they cause an almost unmanageable financial overload which causes fiscal control and budget cutting to take priority over educational excellence, innovation, student and public welfare.

A small group of research-oriented, graduate universities provide the bulk of the fundamental knowledge upon which the modern scientifically-based industry is built. Much of what we expect science and technology to provide in the future, requiring new understandings and new inventions—including a safer, more attractive environment. effective treatment, or better still, the prevention of such diseases as cancer, mental illness, aging, etc., the development of new energy sources and more energy-economical industrial processes, new sources of raw materials and a more effective agriculture, to cite a few examples—will, in my judgement, be verly slow in coming unless present trends are reversed.

On the whole, Federal support for research in universities provided through N.S.F., H.E.W., D.O.D., NASA, and other governmental agencies has not kept up with inflation for the past several years, so that there has been a net reduction of effective support in almost all areas of interest. This has also been a period in which there has been heavy emphasis on result-oriented work. on applied science as exemplified by N.S.F.'s RANNS program and the N.I.H. cancer program so that most of the funding increases that have occurred have not relieved the financial problems of basic research groups. In fact, as I already said, in most cases the situation has become more acute. This is particularly true in those areas of physics which were dependent upon the research division of the Atomic Energy Commission—now part of ERDA—for support. As the AEC rate became less central, its research activities in high energy physics, materials, plasma physics, nuclear chemistry, university-based power reactor research all were neglected. These remain very important areas, and I hope that within ERDA they can now be given the support they need.

I would now like to turn to the scientific manpower situation, the educational situation. Perhaps the most short-sighted action in the fields of science and technology taken by the federal government has been the cutbacks made in the graduate fellowship and traineeship programs. For many years these programs insured that many thousands of the nation's most outstanding college graduates were able to attend graduate schools, and once there, had the financial resources needed to allow them to concentrate on their educational goals. In past years the Congress has managed to insure that at least a portion of these were continued, and I hope that it will be possible to do so again. Perhaps there was a need to control and limit the number of graduate students in science, but the antagonistic, "meat ax" approach of Secretary Weinberger and the OMB has hardly helped maintain the quality of the American scientific establishment.

These many problems, taken together. will seriously hamper the ability of the nation's technical community to deal with its problems in the years ahead. The cutbacks in educational funds are especially disturbing to me. At M.I.T. we periodically make estimates of scientific and engineering manpower demands in order to plan our educational programs. We have done this in recent years to see if we could justify the diversion of substantial additional amounts of private resources from other needs to the support of graduate students. Our studies, repeated over several years, convince us that if the economy recovers to anything resembling normalcy, there will be substantial shortages of well-trained scientists and engineers during the last half of this decade. Unless the current governmental attitudes and present program are reversed quickly, it will not be possible to avoid this potential shortfall. Industry has recognized this danger and to some degree and in some areas is attempting to compensate for the Federal Government's retreat. However, non-governmental resources are not able to compensate for the large loss of support that has occurred in the physical sciences and is now threatened in the biological sciences.

Even in the applied areas that it is public policy to encourage (the environment, housing, urban affairs, health, etc.) the Federal initiatives have not been very effective. There are several reasons for this. First, the agencies tend to manage applied research programs in the same way that they do basic research, *i.e.*, with individual grants having a relatively short-time span even when the work itself is long-range in character. Only the mission-oriented agencies, like NASA. D.O.D., the Department of Agriculture and to some degree, H.E.W. have been effective in their support of applied research. The most successful efforts occur when the end result of the effort is a procurement action by the agency. H.U.D., D.O.T., and N.S.F.—agencies that are not actually consumers of their research products—have generally had too short-time a perspective about the work they sponsored and their efforts have been too small and to fragmented. Much of this work does not relate effectively to the industry it was intended to stimulate. By depending upon individual grants instead of building up a few problem-oriented centers, they effectively prevent any reinforcement between groups and investigators. In part, this mode of operation is a result of trying to be responsive to Congressional pressures for quick results and for widespread geographic allocation of research funds.

Turning now to another point, the withdrawal of D.O.D. from support of university research has had a very negative effect on both basic and applied research activities in our country. D.O.D. was an effective sponsor of research. Its program managers generally could relate their efforts to long-term needs and they were permitted to focus their programs in a limited number of locations so that what they did sponsor was more or less adequately supported. Their long-term interest in high technology in most fields, for example, electronics, aeronautics and astronautics, ships, communications, materials, fuels, etc., provided them with the insights needed to judge the quality and appropriateness of applied research activities. They had the ability to respond quickly and they understood the value of groups of scientists working together on related problems. The D.O.D. research directors had a degree of venturesomeness that was extremely valuable to the health and progress of U.S. science and technology. Equally valuable was the multiplicity of decision-making based upon independent judgements that resulted from having several potential sponsors for a given field. These advantages were discarded because Senator Mansfield, who perhaps didn't understand the positive side of the picture, believed that the Pentagon had too much influence on the American university campuses. Actually, I don't believe that this was truly so, at least not by the time that the so-called Mansfield amendment was passed. Both the national defense effort and the U.S. research effort have been hurt by the Mansfield amendment and the Congress would do well to examine this matter. I am not sure that people in D.O.D. would agree with me. They are less bothered by research programs and the problems of scientists now.

Turning to yet another issue. The emphasis on results and on the prudent use of funds has also tended to turn off imaginative projects where the risks of negative results are large even though the consequences of success should make such undertakings extraordinarily attractive. This is particularly true of projects that take a long time to mature, so that they have to be reassessed and defended periodically. For example, a faculty member at M.I.T. has been trying to develop a molecular microscope, *i.e.*, a basically new way of examining biological materials, but he can't say how effective it will be or how long it will take to develop it, consequently, it has been almost impossible to get substantial support for the work and it will take even longer than it would under good conditions to find out if this new tool is important or not. It is becoming almost impossible to get support to explore a radical, off-beat idea. It is also extremely difficult for a young, just emerging scientist, not comfortably fitted into an establishment laboratory, to get support to pursue his own ideas. Perhaps special attention should be given to these two problems.

I know this is not a budget hearing. I also realize that the federal government has a serious budget problem; yet I also believe that maintaining the quality and leadership of American science is of vital importance and that it would take only moderate amounts of money to vastly improve the situation I have been talking about, so I would like to say just a few words on this score.

The proposed budget for FY '76 hardly keep up with inflation. In fact, this has not been done for several years. Scientific progress has one unfortunate characteristic, the simple problems get answered quickly and lead to growing understanding and sophistication so that research projects almost always get more complex, equipment gets more costly and the time needed for a significant experiment in a given field grows. In some areas of physics it may require a half dozen years, dozens of scientists and many more engineers to carry out one experiment. The same thing is true in some areas of the earth sciences and astrophysics, for example. This means that, ignoring inflation, the cost of operating most research groups will grow over time, perhaps as much as 10% per year. Thus, you see that budgets of the past half decade have forced a curtailment of group effectiveness and productivity.

I would like to point up a factor which has accentuated the fiscal problems. During the early 1960's, many new research groups were started under the auspices of the N.S.F. "Centers of Excellence" program whose laudable purpose was to provide more widespread geographic distribution of academic research facilities as a means of encouraging technical development around the country. To some degree this has worked, but as the new groups have matured, they too have needed growing support and the level of Federal support has not been adjusted to compensate for this additional need.

Each year the percentage of good proposals and therefore potentially productive investigators that can be supported by the N.S.F. and N.I.H. has fallen. My cursory examination of the budgets of these two agencies leads me to the conclusion that this will happen again in FY '76 with the proposed budgets.

To sum up, I would say that the U.S. science and technology remain strong but that both industrial and academic research is beset by serious and growing problems which will increasingly sap their strength. I regard this as serious not only for purely economic reasons that I discussed earlier, but also because I believe that if we are to continue mankind's quest for a better life, our free-enterprise society has the best chance of pointing the way. To the extent that we make it impossible for new knowledge, new ideas, new technology and new industries to emerge and old industries to make better products and become even more productive, we will lose our initiative. To a considerable degree, the turmoil in our society comes from efforts to improve it, but we should recognize that we only have the means to contemplate those next steps because of the wealth that research and our technical industry make possible.

Thank you.

Chairman BENTSEN. The subcommittee is recessed.

[Whereupon, at 12:15 p.m., the subcommittee recessed, to reconvene at 10 a.m., Wednesday, July 16, 1975.]

TECHNOLOGY AND ECONOMIC GROWTH

WEDNESDAY, JULY 16, 1975

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON ECONOMIC GROWTH
OF THE JOINT ECONOMIC COMMITTEE,
Washington, D.C.

The subcommittee met, pursuant to notice, at 10:05 a.m., in room 1224, Dirksen Senate Office Building, Hon. Lloyd M. Bentsen, Jr. (chairman of the subcommittee), presiding.

Present: Senator Bentsen.

Also present: William A. Cox, William R. Buechner, Courtenay M. Slater, Lucy A. Falcone, and Robert D. Hamrin, professional staff members; Michael J. Runde, administrative assistant; and M. Catherine Miller, minority economist.

Chairman BENTSEN. The hearing will come to order.

Our first witness today will be Ms. Dixy Lee Ray, former Chairman of the Atomic Energy Commission, and formerly Assistant Secretary of State for Oceans and International Environmental and Scientific Affairs.

Ms. Ray, we are very pleased to have you. If you would proceed? And I say to the witnesses, we will move as expeditiously as we can. We have a problem with the Senate being in session. We are one of the very few committees that has special dispensation to meet today, being a special creature of both the Senate and the House. So, we will go under that.

I will have to ask that we limit each oral statement given to 15 minutes and then we will return to the witness for questions and additional comments by that witness. So, 15 minutes will not be all the witness will be allowed to speak, but we would like the prepared statement cut to that and we will take the entire statement for the record.

Ms. RAY. Thank you very, Senator. It is a pleasure for me to be here. You have already introduced me and I am happy to acknowledge that introduction and to say that at the present time I am a private citizen having left Government employment about 3 weeks ago.

My interest and direct involvement in the subject of the hearing this morning came from my 2½ years with the Atomic Energy Commission and a scant 5 months with the Department of State.

I have to acknowledge that I do not have a prepared statement, sir, but I do have some notes from which I should like to make a brief introductory statement for the record.

Chairman BENTSEN. Well then, doctor, if you do not have a prepared statement. I do, and I will read mine.

Ms. RAY. Fine.

Opening Statement of Chairman Bentsen

Chairman Bentsen. This is our second day of hearings on "Technology and Economic Growth." Our particular concern today is how will the Federal Government mix science and technological policy, how we can stimulate civilian technological innovation.

The American economy has always been technologically progressive. Many of the world's most important industrial advances ranging from mass production to artificial fibers, computers, and agricultural technology have been developed by American scientists and innovators and made commercial successes by American businessmen.

But, according to a Professor Gilpin, who testified before the subcommittee yesterday, technological innovation in the civilian industrial sector of our economy is in critical trouble. America's once unchallenged scientific and technological superiority has greatly deteriorated over the last decade, especially in the increasingly important technologies for economic growth, international trade competition, and social welfare.

We cannot expect American business to make the technical advances needed to maintain our standards of living and our competitiveness with our foreign rivals without the same kind of government support for civilian technological improvements that we find in West Germany and Japan, Canada, and even in developing countries like South Korea and Brazil.

But, as of today, the United States apparently has no national policy for research and development. By and large, the research and development programs that were developed in response to the military and space challenges from the U.S.S.R. have been dismantled and no new system has emerged to meet the new needs of today.

Our country faces a critical need for a national policy to encourage technological innovation in response to needs for energy sources, for economic growth, for environmental safeguards, and for international competition. We have, fortunately, taken the first steps toward a new science policy with the establishment of the Office of Technology Assessment and the Energy Research and Development Agency.

But, what has been done today falls short of what the national program needs and that is why we have these hearings today and want your contributions and your suggestions as to what can be done.

Now, if you would proceed.

STATEMENT OF DIXY LEE RAY, FORMER CHAIRMAN, ATOMIC ENERGY COMMISSION, AND FORMER ASSISTANT SECRETARY OF STATE FOR OCEANS AND INTERNATIONAL ENVIRONMENTAL AND SCIENTIFIC AFFAIRS

Ms. Ray. Thank you, Senator.

I must say I am in total agreement with your statement. I am pleased to have had an opportunity to read the report provided by Mr. Gilpin and in general find myself in agreement with him. We certainly have under consideration a most important topic. There is no question but what we live in a technological world, that our economic health is to a very large degree dependent upon the state of that technology.

I might add that the big problems in the world today are basically five in number: Food, energy, raw materials for resources, environment, and human populations. Every one of these is interrelated, and no one is going to be resolved without reference to the others. Every one has a very strong science and technology component. Research and development will be needed to resolve all of them.

Although science and technology cannot do the entire job because the problems go far beyond, into social and economic questions as well, that science component of these problems must be there. It is, I believe, quite correct that we do not have in the Federal Government today a coherent, comprehensive policy, and a mechanism for carrying it out, to see to it that the science and technology, the research and development component, of these problems is developed, let alone the things that can be done directly through Government activity. But much more important is how this can be carried out with Government assistance or at least Government understanding and a good Government relationship with private industry.

It is my feeling, very strongly, that whatever in the technological field can be done in the private sector ought to be done in the private sector. There are some things that Government can best do and must do, and you touched upon a few of them; national security—this is one.

There are very large areas such as the technical development in the field of aerospace in which I think it is worth while to reflect, Government involvement in the development of nearly all of the major types of aircraft, for example, has been a very important factor.

Professor Gilpin refers to the Boeing 747, but he did not bring out the fact that that particular jet, the first of the big jumbo wide-bodied jets, resulted from the fact that the Boeing Co. lost the contract for the C–5A for which they had been in competition and took the work which had been done under the Government contract and converted that into a civilian application.

This is one of the ways in which Government can be effective in certain kinds of industries in providing, through competitive contracts, what almost amounts to the seed money for the development of prototypes which, when it is appropriate, can then, under various circumstances, be converted and developed into a civilian application.

The premises that Professor Gilpin established that technological innovation is at a critical point in the United States today, that we face a very difficult competitive position in the world, that for many years our technological position was essentially unchallenged, I think is not widely recognized. That situation no longer exists today.

You mentioned that we are being in some instances surpassed by technological developments by West Germany—I think that is something which we must take a very careful look at. But I cannot entirely agree with Professor Gilpin's final conclusion that we can or should adopt the same mechanism that has been used by Japan so successfully.

Let me just say that it is my opinion that if we are to do that, we must first find another country that could take the place of the United States of America with respect to us; that is to say, take the place which the United States has been playing and continues to play with respect to Japan, provide for its security with our expenditures, with our military and national security developments, and therefore, allowing Japan's civilian economy complete freedom to go ahead and develop in the nonmilitary sector.

There is a similar aspect as far as West Germany is concerned, although their defense expenditures far exceed those of Japan. As long as the United States maintains a protective umbrella over other countries with our own security forces, we allow their civilian economies great opportunities.

Chairman BENTSEN. I agree with that. But insofar as Germany is concerned, they are actually spending a comparable percentage of their GNP on defense as the United States. In fact, their Secretary of Defense argued with me when I was over there that they are actually paying a larger percentage than we are.

When you get around to Japan, they are only spending about 1 percent. But I see some change in their attitude beginning to develop. They are trying to pick up more of their own defense.

Ms. RAY. The only other major thing—again this is particularly true with respect to Japan, at least in the international field—is the relationship between Government and industry. In order to be able to follow the route of Japan, and be competitive by that route in an international field, we would have to have a much closer relationship between Government and industry than exists in our country at the present time, while maintaining an arms-length relationship and full competitiveness domestically.

The question of antitrust, for example, really becomes very important in the international sphere. Whereas the governments in a number of other countries, and certainly this includes Japan, represents industry abroad and it assists with negotiations in private industrial contracts and agreements and economic activities. In the case of the United States, our relationship, even in our embassies abroad, even with an economic adviser, is very much an arm's-length type of representation. So that very often the industry which is involved in international negotiation is perceived as not having the Government backing, whatever Government understanding that may be.

In my opinion, if we are really to be competitive in the modern world, especially in international arrangements, we must find some way to allow our industries to form consortia—which would be in violation of our antitrust laws at the present time—and also to have a close, almost partnership relationship, with Government as is the fact in Japan and to a large extent in West Germany as well.

Chairman BENTSEN. Ms. Ray, let me ask you a little about your situation while you were in Government.

In your letter to President Ford announcing your resignation from the State Department, you said:

> Public Law 93–126, passed by the Congress in October of 1973, mandates a policy role for the Bureau of Oceans and International Environment and Scientific Affairs. Under present department procedures, the Bureau can do little but acquiesce in the policies set by others and attempt to implement its broad responsibilities with little authority and few resources. Similar kinds of problems plague our Nation's domestic science policy.

Now, what do you mean by similar kinds of problems plague our domestic science policy? Can you elaborate? What should we be doing differently?

Ms. RAY. The kinds of problems that can plague our domestic science policy have to do with it, just as you have mentioned earlier, the lack of a clear set of objectives and goals as to what we wish to achieve. There is, of course, support for science, both research and development, and for engineering developments and even pilot and demonstration plant activities and so on, available through various Government agencies and departments. But, there is no central coordinated, comprehensive policy or mechanism by which the Government interest can be expressed.

We have the Department of Defense which does its research and development and works through industry in contracts. We have the Energy Research and Development Administration, we have the Department of Commerce, we have the Department of Interior, each with its own policies with respect to the way of contracting what is required of the private sector and the interrelationship between the agency and the private sector.

Chairman BENTSEN. Well, let me delve into that a little.

When you accepted your position as Assistant Secretary, what was your understanding as to your responsibilities and your powers? What kind of a relationship did you have with Secretary Kissinger? What kind of responsibilities and powers did you actually have?

Ms. RAY. Secretary Kissinger and I discussed this before I was sworn in as Assistant Secretary and it was my understanding that the Bureau did have indeed the power to put its mandate and its responsibilities into action. The law which was passed mandated to the Bureau three primary responsibilities.

One, the Bureau was responsible for developing a comprehensive and coherent policy in science and technology as it relates to international affairs.

Second, the Bureau was responsible for advising the Secretary of State with respect to the science content of foreign policy before the formulation of an overall foreign policy in the area.

And, third, the Bureau's responsibility was to coordinate the activities in science and technology of other Federal departments and agencies. Very many, as you know, of the departments and agencies in Government have international programs but they are not coordinated—the international programs of the Department of Agriculture and those of the Department of Commerce and so on.

There is no central information gathering function even as to what all of these activities are for.

Now, those are the responsibilities. In order to be able to carry out those responsibilities, there have to be some resources, there have to be capable people, and there has to be some kind of budget. The Bureau was put together out of three existing functions or three previous existing offices within the Department of State, with personnel fully occupied doing other kinds of things which were also necessary and a legitimate part of the function.

For example, taking care of bilateral agreements of various kinds as they relate to science and technology, monitoring them, taking part in international negotiations on fisheries, treaties, space treaties, all that sort of thing, the science attaché program.

In other words, there are very many activities already going on. The personnel already existed in the offices which were put together to make the Bureau fully occupied in their time. There are only 78 personnel in that particular Bureau. In order to assume these new responsibilities that have to do with the function of developing policy and coordination, new personnel and new resources were required. It was my understanding that those would, in due time, be provided. After a few weeks it became quite evident not only that it would be probably fiscal year 1977 before there would be any additional resources, either in personnel or in budget, but that other parts of the Department were in fact in one way or another at least attempting to fulfill the responsibilities of the Bureau.

Chairman BENTSEN. Well, where would you vest the responsibility for the coordination of technology policy of these diverse bureaus? Would you actually favor a council of science and technology, or something like that?

Ms. RAY. So far as foreign policy is concerned and the international area, the Department of State ought to be and is the proper place for any coordination of the international activities for domestic science policy, inasmuch as existing departments and bureaus and agencies have legitimate functions in a variety of missions. For science and technology, I think there ought to be—there must be some kind of coordinating mechanism within the executive branch of the Government which nevertheless could be responsive to Congress. The idea of a council of scientific information and technological development to act as a coordinating and policy point, I think, is essential. It may be that the mehanism for a science adviser's office in the Office of the President, and I understand that legislation has now been sent to the Congress on that, may be able to take on that function or it may be provided for in that legislation.

If it is not—that is a very good possibility—it could best be set up on a statutory base, because that gives the Congress some involvement and oversight which I think is very necessary, or it could be set up by Executive order.

Chairman BENTSEN. Well, who actually set policy in your area of responsibility?

Ms. RAY. That is very hard to say, except the Secretary of State. Let me try to use an example——

Chairman BENTSEN. Would you repeat what you said, who actually set policy?

Ms. Ray. The Secretary of State did.

Chairman BENTSEN. Did you have much communication with the Secretary of State concerning this?

Ms. RAY. No, sir.

Chairman BENTSEN. Is the same problem true of other Assistant Secretaries over there?

Ms. RAY. I believe it is; yes.

Chairman BENTSEN. What changes would you recommend?

Ms. RAY. I would recommend that it is very necessary for the head of the department to have a mechanism, either through himself personally or through a deputy, to carry out the administrative and day-to-day management functions and see to it that the expertise and the talent of the organized bureaucracy, the organized departments is in fact an implementation of the policies that are set. As the situation exists in the Department of State, there is little contact beween the Secretary's office and the various bureaus and parts of the Department which in fact have the responsibility of carrying out all of the administrative chores and the day-to-day activities.

A policy means nothing if it is just pronounced and nothing happens. A policy, to be effective, has to be implemented; it has to be monitored; it has to make sure that things are happening as a result of that, and, at the present time, the Department of State is largely unmanaged.

Chairman BENTSEN. At the present time, the Department of State is largely what?

Ms. RAY. Unmanaged.

Chairman BENTSEN. Unmanaged?

Ms. RAY. Yes, sir. That is to say, the various bureaus and desks, and so on, go about their way-to-day activities with very little coordination and policy direction from the Secretary's office.

Chairman BENTSEN. In other words, you feel like you had little input to Secretary Kissinger, and in turn, he had little to you, insofar as your area of responsibilities?

Ms. RAY. Yes, sir. Communication is always a two-way street, and the lines of communication between the Secretary's office do not exist.

Chairman BENTSEN. There is no invitation for such communications by the Secretary of State's office?

Ms. RAY. No; there is, instead, a policy planning group of some 20-or-so policy planners directly related to the Secretary's office that carry out, for the most part, the development and setting of policy, the writing of speeches, where policies are announced, and so on, and one way or another, but very informally, through various staff contacts, communicate with the various bureaus.

Chairman BENTSEN. Well, do you think the Secretary and his advisers that are around him were sufficiently advised in your area of responsibility that they did not have to consult with you?

Ms. RAY. No, I do not think so. I think it is fair to say that we expect too much of the Secretary of the Department, particularly a Department with such enormous responsibilities as the Department of State. To expect the Secretary to be the developer of our international and foreign policy, to carry out international negotiations of various kinds, and to be involved in enormously important problems that, in fact, do affect the future of this world, and certainly, the future of our country, and at the same expect him to be able to administer the Department and so on, is indeed asking a great deal of one person. And then asking him to handle a second fulltime job, as well, being the Director of the National Security Council—I think it is asking entirely too much of one man, no matter how capable. And we have a very capable Secretary.

Chairman BENTSEN. I am not sure that anyone is asking the Secretary of State to make all of those decisions himself. I think he is self-anointed in that respect, and he has not had the ability to delegate responsibility. He is a brilliant and able man, but have gotten a foreign policy that is just an extension of his own personality. One man, regardless of how brilliant or how able he might be, does not have the ability to make all of these decisions by himself—to neglect a great many responsible people throughout his Department, who feel that they have some experience and some depth of knowledge. It ought to be probed. There ought to be communication there.

So he is insulated and isolated in making these decisions by himself. So I agree with you very much that the State Department has to be institutionalized and other people brought into the decision-making process. Otherwise, you have a foreign policy that is just a series of personal deals on the part of the Secretary of State.

For him to wear the hat of Secretary of State and that of the principal security adviser to the President, I think is a mistake. The latter office was set up so that the President could have options on advice, so he could listen to more than one person, as he arrives at a judgment as to what the foreign policy of this country ought to be.

So I absolutely concur with you, Ms. Ray, and I have introduced legislation to try to see that these responsibilities are divided up in the future.

Ms. RAY. And I would like to emphasize that there are many fine and capable and talented people in the Department of State, and a very large amount of expertise is not being used.

Chairman BENTSEN. Now, Ms. Ray, in your resignation letter you mentioned that the programs recommended in your report for the Nation's energy future to convert our economy from petroleum to a heavier reliance upon solid fuel languish or are submitted to stultefying and interminable feasibility studies, and that your synthetic fuel proposals had not received serious consideration. Will you expand on that?

Ms. RAY. Yes. The report that we made in December of 1973, as a result of a request from President Nixon, laid out a program that would in fact, if followed, provide for both Government research and development assistance by the Government to various sectors of the industry to do these two things: Develop the means for converting more of our dependence upon liquid fuels—that is, directly from petroleum—to a utilization of solid fuels, of which we have coal in abundance—and it should be a prime foundation stone of our energy policy—and the use of uranium, which is a developed technology and can be applied to the production of electricity, and should take up a reasonable amount of the load there.

And also, to continue, for the development of known processes, known technologies, to improve them and make them more economical and more engineeringly sound processes; to develop particularly from coal the synthetic fuels—liquid and gaseous fuels—that could be used in place of the direct use of petroleum, and save petroleum for its necessary function, very largely in the sector of transportation, and perhaps even more importantly, in the field of petrochemical industries. These recommendations, I say, have been studied. I think some of the delay in getting them, or some modification of them, into action, was occasioned by the reorganization of our energy research and development activities in the Federal Government, and with the emergency, now, of ERDA. I believe that in time, many of these recommendations, as they may be further developed, will in fact be put into effect.

I think I am concerned, and I believe this concern is shared by a number of other people, that we are moving far too slowly, that we are now far more dependent on imported oil than we were a year ago. We are now spending far more money, sending $25 to $30 billion out of the country this year for the payment of imported oil, as compared to $5 billion in 1973. This is a rate of increase of the kind we cannot very long sustain. We are faced with the likelihood of even further increases, and we still do not have a single operating demonstration plant, providing synthetic fuels, although several have been under construction for a number of years. And they were started under the aegis of the Department of Interior, but under a mechanism that makes it very difficult for the industry, private industry, to work; namely, a 50–50 matching grant.

Given the state of the economy, for the most part, industries have not been able to put up the front-end capital to do that. I think what we should do is move as vigorously in this field as we did in World War II with the synthetic rubber program. And I remember always a quotation——

Chairman BENTSEN. Ms. Ray, you have been reading my mail.

Ms. RAY. I remember a quotation from the report of that particular program, and I think it is a matter we should follow. The program was established with a maximum of Government support and a minimum of Government interference, and it was done by private industry. There were, at the time the program was begun, some 30 different possible means of making synthetic rubber. All of these were funded. And over 20 of them worked. The rest were simply eliminated. The operating plants which were put into existence and eventually became the source of almost all of our domestic rubber, were economic, and at the end of the program, were sold back to private industry, so that the taxpayers' money and investment was totally recovered.

I believe the same kind of thing could happen in a synthetic fuels industry, if we really stepped forward and made a partnership between Government and private industry, and saw to it that the assistance was given.

The terrible uncertainty which now exists in the energy field as to what Government regulation or rules or prices or controls are going to be, is eliminated, if the assistance is given in such a way that private industry can move into the field and have some assurance that its investment will not be totally lost.

Chairman BENTSEN. Ms. Ray, I would like to continue on with this, and you have done a good job, without a prepared statement. But we have limitations of time, and I am going to have to call this part of the hearings to an end.

Ms. RAY. Could I just make one more statement for the record?

Chairman BENTSEN. Yes.

Ms. RAY. I think it is imperative that at the present time—and I am speaking again in the international field, and international competitiveness—I think it is imperative that we examine what we are in fact doing or committed to doing in international science and technology. We have become involved in a number of joint commissions with other countries, and a number of bilateral agreements with other countries, some of them very recent, some of them of long duration, and all of them having words in them that involve exchange of information and experiments and data, and programs, and so, in science and technology. I think it is time to examine those programs very carefully and ask what is the United States getting out of them? What value are we putting on our knowledge, both our basic technical knowledge and our managerial knowledge, our knowledge of how to organize and commercialize the technology? What is the return to the United States?

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I think it is time that these questions were asked, because unless the interests of the United States are taken into consideration, we will find ourselves outcompeted, where we have provided the original stimulus and assistance—as for example, in computer technology and the optical industry, in soft lenses, soft lens grinding, in electronics, in aerospace, in the steelmaking, fertilizer technology, in deep drilling—in all of these, where we have provided the basic information first, and then we find that they are commercialized more economically in other countries.

I think it is a serious situation, and we must first see what in fact we are doing, in international technological cooperative agreements.

Chairman BENTSEN. Ms. Ray, you have highlighted the problems, and you have given us some suggested answers, and we are very appreciative of your testimony. Thank you.

And I would like for you to look at the record of your testimony, and I give you the liberty of amplifying on it.

Ms. RAY. I would be very happy to.

Senator BENTSEN. Thank you very much.

I would like to now call Mr. John Stephens, president of Excel-Mineral Co., Santa Barbara, Calif.; Prof. William B. Shockley, Stanford University, a Nobel Prize winner; Prof. Norman Ramsey of Harvard University; and Mr. Jacob Rabinow, Director of the Office of Invention and Innovation, National Bureau of Standards.

I would like to first call on Mr. John Stephens, who is a personal friend of mine, who has been very helpful in bringing some of the very most distinguished, able scientists to these hearings.

Mr. Stephens, would you proceed.

STATEMENT OF JOHN A. STEPHENS, PRESIDENT, EXCEL-MINERAL CO., SANTA BARBARA, CALIF.

Mr. STEPHENS. Thank you, Mr. Chairman.

The Joint Economic Committee and the subcommittee chairmen are to be congratulated on their perspicuity in holding these hearings to discuss a national policy fostering technological innovation generating economic growth. Economists themselves, except those present, were overlooking the significance of technological innovation until a few years ago, as noted by Robert Gilpin in his report to the subcommittee.

We hope that our comments today will be helpful in forming policies and programs including patent policies which will spark new life in innovators and inventors.

We speak of rejuvenating the economy, but it may well be that people need rejuvenating, too. It is not that we are jaded or tired; it is that we are dispirited and sense a loss of direction. Creative people rejuvenate themselves and others, too. Perhaps the innovators can help the spirit as well as the economy. Let me explain what I mean by this statement.

Our company is in the mining, mineral processing, and chemical business.

Our research experience falls into two categories: (1) The development of new equipment, processes, and products for our own use in our own machine shops from ideas provided by our own people, and (2) the development of products and processes from ideas of outside people and often not directly related to our own operations.

Our record of success has been much higher with our in-house ideas. Perhaps because the purpose was better understood and the problem more clearly and specifically defined. In one notable instance, our men built an automatic packaging machine which St. Regis Paper Co.'s packaging engineers said couldn't be done.

I want to call your attention to a windfall blessing from our endeavors and our successes. That is the pride and spirit and good humor generated in virtually all our employees by a successful idea. Our people want to improve their performance, qualitywise, quantitywise, and one of the most effective ways is to provide—or help them create—better tools and better procedures. Their paycheck increases, but the more profound prize is their self-esteem. I am convinced that this is universally true, that a sense of progress as to performance is as vital as progress in pay. There is no such thing as static performance. A machine will repeat itself, but not people, without becoming bored and spiritless. Innovation, creativity is vital both for the spirit and for the economy.

We did not always have the answer to a critical problem, and in this case we sought outside help.

By the time we came to this point, the problem was critical and specific. This brings me to practical suggestions. Those people working in a given area are best informed as to the state of the art and as to the critical problems, but perhaps having exhausted their imaginative repertoire, may not be the best suited to arrive at solutions. A better bet is a fresh mind with a track record of imaginative performance. What is needed is a clearinghouse, perhaps Government sponsored, which matches problems with the imaginative genius of men like our three witnesses and which awards medals and prizes for good problems and good solutions. We should honor our creative citizens who add to our wealth and lift our spirits.

This suggests another possibility. Where do ideas come from in the first place? We are such Yankee pragmatists! We are always so interested in what we can do with an idea—can we cash in on it, where will it take us—that we don't bother too much about how we got it. Bill Shockley, I know, has given some thought to this. There are numerous sources of new ideas, but one of the most productive is the analogical suggestiveness, for want of a better term, of other ideas. Ideas breed ideas. Ideas accumulating in our cultural heritage and in our mind's experiences form a pool, a reservoir, which through its powers of suggestiveness is continuously performing the mysterious act of creation.

I am probably the only one who understands how this takes place. In each of us there is a genie forever alert to the possibilities and needs of the moment—he is the selective genius. In some of us he is fat and lazy, and in some of us he is slim, alert, and forever the hunter seeking his prey. Someday we may discover the royal jelly that will make us all the swift hunter like the three superstar genies with us here today.

At the present, however, our best bet is to utilize their power to the maximum. Take these men of creative genius, these innovators and inventors, and give them roving chairs—from one university to the next—taking their ideas, their stories, and the models of their inventions, and let them, like pied pipers, bewitch the potentially pregnant youths of our land into following them. I nominate these three here today for that assignment.

Now I come to the really pleasant part of my assignment: the introduction of three superstars. I do not feel humble in introducing them, because they are not responsible for the genie that they have any more than I am for my own more modest genie.

Bill Shockley, we all know, hit the jackpot with his invention of the transistor. What most of us do not know is that, instead of retiring with richly deserved honor, he went to Stanford, where he became a teacher. There he has been a marvelous success in opening the minds of his students to radically new ways of discovering new problems and new solutions.

I suppose, Norman Ramsey, you might say, was given the highest award that a man in his profession, physics and nuclear particles, could desire. That was the appointment as one of the directors of the Fermi Laboratory, which is one of the highest prizes that could be given to any man by his peers.

Jack Rabinow I appreciate for his definition of genius. On one occasion, when he was required to introduce Bill Shockley, he said, "Bill Shockley is a genius. How do I define a genius? When I first met Bill Shockley, he asked me what I did, and I said, I do reading machines. I am one of the world's authorities on reading machines. Bill Shockley said, 'What are reading machines?' I told in 5 minutes what reading machines were, and I discovered after 5 minutes that Bill Shockley knew more about reading machines than I did. And anyone who knows more than I do after 5 minutes is a genius."

Jack Rabinow has been one of the most fertile and creative inventors in the United States. He started his own company, which was absorbed into Control Data, where he was a director of research for many years. He is now back at his own home with the Bureau of Standards. He is one of the best informed men in the United States on the whole subject of patents and on innovation.

Thank you very much.

Chairman BENTSEN. Mr. Stephens, that is quite a set of introductions.

Mr. Rabinow, I see you hold 209 U.S. patents. I just wondered what you do in your spare time.

Mr. RABINOW. I try to invent some more.

Chairman BENTSEN. Why do you not tell us about it.

STATEMENT OF JACOB RABINOW, CHIEF RESEARCH ENGINEER, INSTITUTE FOR APPLIED TECHNOLOGY, NATIONAL BUREAU OF STANDARDS

Mr. RABINOW. I feel that I am the low man on the totem pole. Yesterday you heard from great scientists, today you'll hear about the other end of technology. I convert what they produce into some useful goods, and if not always useful, at least sometimes.

I am very concerned about what is happening in the United States today. I will not repeat all the dire data you have already heard. I agree, we are slipping. In my prepared statement, which you have, I go into documentary evidence of this.

I have in front of me a report from the Department of Commerce, an "Early Warning" report from the Office of Technology Assessment and Forecast that studies the patent arts, because the patent arts are a direct indication of the innovative effort of a country. In other words, if you see how many patents are issued and to whom, you know who is working and how much work is being done, because patents not only preceed innovation, they also follow it. In many fields the foreigners get more patents in the United States than we do.

Chairman BENTSEN. Is that a change?

Mr. RABINOW. It is a change, and it has a curious spill-over. What happens in a particular field like, for example, ground transportation without wheels, is that the more they succeed, the more money and effort they put into it, and as the less we succeed, the quicker we get out of it.

So, as in two competing businesses—one rises and the other falls, and the spillover is very sudden. In many fields—and I list a summary of this report in my prepared statement which you have—where we were preeminent for many years, the foreigners are taking the lead. This is not only in cameras, but in melting of metals, ground transportation, certain fields of technology of transistors, and so on. This is very disturbing.

By the way, anything I say here today, is my own opinion. It has nothing to do with the opinions of the Bureau of Standards, the Department of Commerce, or anybody else in the Government. I have some nasty things to say about the Department of Justice and the Bureau of Standards, I think, would not second my statements. I hope you understand this.

I had some problems in coming here. But, the Bureau in its wisdom said I could say anything I like as long as I tell you that it is I speaking, and not the Bureau of Standards.

I will not go into the patent data of the report I cited, because they are listed in my proposed statement and it would take too long to repeat here. But, it is very disturbing that the other people are working harder and are getting ahead of us.

One of the other things I cite in the statement is the fact that a great many, probably most of our large corporations—and I have to be careful not to say all large corporations—are putting more and more effort into the short term development. That is, they would rather improve their present products than do long-range development.

They have several reasons for doing this. The study I cite was made by McGraw-Hill. The large corporation finds that they expect fewer and fewer new products to hit the market in the next 13 to 20 years, and more and more products with just minor improvements. This is a tragedy. It means that if you shoot for short term gains, you will make more money, but ultimately you go out of business. You have to have a balance of long term and short term development, as you do in all things.

The reason for this short term attitude is something like this. After World War II, there was a fashion to spend a great deal of money on R. & D. In fact, Wall Street said if you do not spend more than 5 percent or 10 percent on R. & D., you are not a "growth company". So, companies spent money foolishly. They spent money on research without knowing what research was all about. They hired people who were not good enough and they found that research did not pay because research usually does not pay off to the man who does it. First, you do not know if it is going to be to your benefit, or someone else's.

You do not know when it will pay off. It might pay off immediately; it may pay off in 20 years. For example, if Einstein came to you in 1905 and said I have a new theory of relativity, it has to do with light bending past objects, and so on, and I would like to have some money to study it further you would have asked, "of what use is it." The proper answer is "no use". I would like to quote the famous statement by Farraday when he discovered the magnetic field. The Exchequer of Great Britain said "of what good is it, Dr. Farraday? What use can we make of it?" He said, "I do not know of what use it is, but in 30 years you will be taxing it."

One does not know where research leads. You have to have a mix of R. & D. and businesses. Large corporations have discovered that they were not making money on R. & D. and that it was a great expense; so, they have cut back.

Another thing that has happened to us is this: In many large corporations, the manager is no longer the founder. The management is "professional" and I use this word with some disdain. The management does not know the technical aspects of the things they produce. They can run a business consisting of swimming pool management, lending agencies, a bank, spaghetti factory, all of this mixed together, and the top management does not personally care about any of it. They are professional managers. They did not inherit the business. They will not leave it to their children. They have no emotional involvement with the product. They did not pioneer nor invent it. All they care about is the profit and the stock options, and this leads to very-short-term results.

There is a study in Dunn's Magazine, which I cite, that states that many of our stock option plans naturally tend to reward short-term, immediate returns and very little based on long-range programs. If this continues, many large companies will undoubtedly suffer. This is not true in foreign countries where the management is much more stable.

Another thing that has happened in the United States is that with high interest rates it is very hard to raise money for small businesses. When you can make 8 or 9 percent on tax-free bonds, you would be insane to invest in one of my inventions.

I am beginning to think that I also am insane when I invest in my inventions when I can get nearly 10 percent on tax-free bonds.

Wall Street also got burned in the 1950's and 1960's, by investments in high technology industries. I talked to some Wall Street friends about this last week. They said that they "are very, very careful now about investing in new technology".

Now, the question is how important is all of this. Well, there are three ways in which a country can become rich and improve its standard of living. One, of course, is to be literally floating on a sea of gold or oil and sell the oil or gold for what it needs. This cannot be done in the United States. We have no such natural resources. Perhaps Kuwait can do it.

The second way, which is classic, is to rob somebody else. That is a good way. You get a colony and you make them work for you. This has been classic for many hundreds of years, but it is getting difficult; I think, for us, it is impossible. And the the third way, and the only way left by which we can improve our standard of living is to improve the technology; that is, for every hour of work of a man or woman to produce more bread, shoes, TV sets, whatever we like, and to produce more services. This means technology has to be supported, and there is no other way.

So, the question is whom should we support and where do the inventions come from, the inventions that make the great steps forward. So, I looked at the great inventions of our day. In the fields with which I am familiar, with only two exceptions, the great inventions did not come from the large corporations. I would like to read you a partial list of the inventions and innovations that were not accomplished in large corporate research laboratories. They are as follows: Atomic energy came from universities and Government; computers came from two guys who were students at the University of Pennsylvania; radar, xerography, microwave technology, lasers, jet engines—the last came from a garage in England—space technology; and inertial guidance from MIT; by the way, MIT seems to be in this business all the time; holography, color photography was not invented at Eastman Kodak, it was invented by two musicians; FM radio, Armstrong was the inventor of that, and eventually committed suicide because of the litigation on patents; medical developments—polio vaccine, heart pumps and pacers, kidney machines—flotation process for glass, which is the way all glass is now made, did not come from our glass companies; fiber optics; TV tape recording; instant photography; continuous casting of metals; the Honda engine, the Wankel engine came from foreign countries.

The only two inventions in technology with which I am familiar that came out of large corporations were the transistor that three brilliant men invented in the Bell labs, and TV as we know it, which was done by RCA.

I am leaving out chemistry because I am not a chemist, and I am leaving out the great contributions of large corporations who took over these things and did the industrial and commercial developments, which are very bit as important as the basic invention. But if you want to make giant steps forward, it is the middle inventor, the man who does not work for large corporations, who does not work in a basement, he is the man who takes the giant steps. He is the college professor and the independent inventor; the guy who works for the Government, for small companies, and so on. These are the people between the large corporations and the "basement inventors."

Now, when I talk about technology and the promotion of technology in the sense I use the word, the final technology converted to useful goods. I have to talk about the patent system, and unfortunately the patent system in the United States is under violent attack. It is under attack in a kind of a quiet way from many large corporations who literally do not give a damn about the patent system, they do not need it—I was told by the executives of some of the largest corporations in America. or in the world where I worked for them as a consultant, that as far as the patent system is concerned, they could live very well without it. A company that is 50 to 80 percent of an industry does not need patents. It sells products. Such companies do not make money on royalties. They have a tremendous patent staff which is a great expense. They have the staff just to defend themselves, and the vice president for patents of one of the largest corporations told me that if there were no patents, they would not have to have a hundred patent attorneys and obtain patents in 50 or 60 countries. They would not have to do much of the research that they do now just to protect themselves from research of other people. They could reduce R. &. D. and manufacture whatever products they darn please. It means they would have essentially no competition.

The Department of Justice is worried about the patent system. It seems to me they should consider the fact that the patent system keeps the big corporations honest, because they never know when some small guy will come up with a new product, like Xerography, or color pictures, or something, and make the big boys sit up. As a matter of fact, it is the little companies which the big boys absorb that often keep them alive.

I am particularly concerned about the attitude and actions of the Justice Department. I have had a running fight with the Antitrust Division of the Justice Department for a long time. They literally hate patents because patents smack of monopolies and they hate monopolies. Whenever there is an important court case in the Supreme Court or district court, where a patent is involved in an antitrust suit or an important patent fight, they enter as a friend of the court; always against the inventor or the owner of the patent. They entered the case, for example, in *Lear* v. *Atkins*, in the Supreme Court some years ago, and the Supreme Court decided for the first time in our history that I can give a man a license, he can develop a business based on my invention, get secondary patents, copyright a good label, and then sue me to prove that the patent is invalid; all this, after he has the business all sewed up. If he wins, he does not have to pay me any royalty; if he loses, he still has the license; he continues to do business as he did before. So, if he has to pay me $1 million in royalties, it pays him to set aside $50,000 and sue me.

This open invitation to patent suits is now the law, because the Department of Justice convinced the Supreme Court that it is always good to destroy a weak patent, no matter what the justice of the situation.

I have quotations in my prepared statement which I will not read now, where the Department of Justice proposed that they set up, in the Patent Office, a special agent of the Department of Justice to oppose any patent they wish to oppose on their own selective basis. In other words, I apply for a patent, the Patent Office goes through the routine, decides I am entitled to it, and the Department of Justice can enter the case before the patent is issued and fight me whenever they think it is an important patent and they feel like fighting me. The selection will be theirs, not mine, and not that of the Department of Commerce.

They also made other such statements. They would like to see all the patents obtained by the Government to be owned by the Government and never given to anyone on an exclusive basis; and they would like to see that even when the Government is not involved, that industry should be required to issue a mandatory license.

In this connection I would like to tell you about an experiment that our Government performed unwittingly. During World War II we confiscated all the industrial patents that belonged to our enemies—that, is Germany, Italy, Japan—and they became the property of the alien property custodian. There were 15,000 industrial patents that were made available to U.S. citizens, free. All you had to do was write a letter and get it. There was a $7 charge for the paperwork, but you didn't really need this formal license. Practically, none of those patents were used. They died on the vine, because nobody wants a patent that everybody else can get also, because the word "patent" means that I have some advantage for a few years, and you cannot compete with me. If everybody can get it, it is a piece of paper, and you might as well not bother.

The whole idea of the patent system is to give me some advantage, encourage me to put effort and money into it, to get some backing. The fact that the Government now has 22,000 patents developed on Government money and we cannot seem to give them away—is very significant. I think the Department of Commerce realizes full well, but the Department of Justice does not—that very often it is necessary, no matter who paid for the work, to give somebody the patent to develop it into a useful product. Society does not really care whether I make money on a patent or not. Society should care about one thing: Does it get built? Does it get developed? Does it aid our economy and our foreign trade and our standard of living?

This is something that I have argued with the Department of Justice privately and publicly. I cannot convince them. They say Government paid for it, it belongs to the Government." I say "Yes, it does, but what good does it do?"

Then there is talk about getting royalties to repay the Government. This is hogwash because the Government is a more-than-50-percent partner in any profit that I ever make on a patent, not only during the life of the patent, but forever. So, for the Government to extract royalties is silly. If I gave you a license on a patent, and you were willing to make a deal with me that you will let me and my heirs collect 50 percent of all your profits as long as your corporation exists, I will always take that instead of a 2 percent or 3 percent royalty. So, when the Government is talking about royalties, it is talking pure nonsense.

If the Department of Justice really wants to do something about the patent system as this system relates to large corporatons, let it propose bills where the patent rights to IBM or Genral Motors be different from the patent rights to me. But, to kill the patent system, as they are trying very hard to do is unfair, foolish, and certainly contrary to the Constitution. The new patent bills in Congress are all anti-inventor—this has been true of every bill in Congress today and for many years—because the Department of Justice has convinced people that the Patent Office should raise the fees and limit the rights.

Let me see, there is one more thing that I forgot. The bills all say that during the life of the patent the inventor will have to pay $2,000 or $3,000 in maintenance fees. Europe used these fees and we never did, this means that, in most cases, the inventor will be forced to abandon his patent before the 17 years are up. I think this is a deliberate attempt to weaken the patent system, to make the inventor have less incentives, and anything that does this is tragic. I think we need more incentives. We need more glory and more profit to the inventor, particularly the sophisticated inventor who knows what he is doing. We should provide more incentives to get the technical lifeblood that we need.

Now, if we do not do this because the Department of Justice worries about the large corporations, we are going to throw out the baby with the bathwater.

Thank you, Chairman Bentsen.

Chairman BENTSEN. Mr. Rabinow, that is a very interesting presentation, and we look forward to reading your prepared statement.

[The prepared statement, with attachments, of Mr. Rabinow follows:]

PREPARED STATEMENT OF JACOB RABINOW

My name is Jacob Rabinow; I reside at 6920 Selkirk Drive, Bethesda, Maryland. I am employed by the National Bureau of Standards and have the title of Chief Research Engineer of the Institute for Applied Technology.

I want to make it perfectly clear that I speak here as a private citizen and that the opinions expressed are my own and may not necessarily be the opinions of the Department of Commerce or other agencies of the U.S. Government.

I have been an inventor for practically my entire life and now hold 209 U.S. patents and something of the order of 100 patents in foreign countries. For my work as an inventor I have received many honors, among them a Certificate of Merit from President Truman and a Gold Medal from the Department of Commerce.

I was born in Kharkov, Russia, and came to the United States in 1921 at the age of 11. I was educated in New York and received two degrees in Electrical Engineering from the City College of New York. I have worked for the National Bureau of Standards from 1938 to 1954 and again from 1972 to the present. During the interruption of 18 years, I headed by own company for ten years and later merged it with Control Data Corporation, where I had the title of Vice President.

I am very grateful for the opportunity to speak here because I believe the health of the R&D effort in the U.S. and the general state of technological innovation is not as good as it should or could be, particularly considering our history, our vast resources, and our fiscal and intellectual powers. In order to put my fears into perspective, I would like to cite just a few documents that should be of interest.

1. The Department of Commerce has been studying the issuance of patents in certain specific technologies, to residents of the U.S. and to foreigners. The number of patents filed is an excellent indication of activity in a particular field. Attached hereto are two tables from a report entitled "Technology Assessment and Forecast: Early Warning Report of the Office of Technology Assessment and Forecast, December 1973." These two tables list the areas with the highest foreign shares of U.S. patents. It is interesting to note that in many areas of the patent art more U.S. patents are now being issued to foreigners than to ourselves. For example, in "Purification of Molten Iron," the foreign share is 77 percent; in the field of "Magnetic Field Responsive Resistors," it is 72 percent in "Superconductors," it is 60 percent; in Fuel Injection Pump Apparatus for Internal Combustion Engines," it is 57 percent; in "Automotive Fuel Control Devices," it is 61 percent; in "Ground Effect Machines," it is 54 percent.

Table 2 of the same report shows the areas of U.S. patents with the highest projected rate of increase in foreign shares. For example, in the "Magnetic Field Responsive Resistors," the present share is 72 percent and it will soon be 90 percent; in "Liquid Fuel Rockets," it is now 31 percent and soon will be 65 percent; in "Automatic Electric Arc Welding and Cutting," it is now 41 percent and is expected soon to be 78 percent; in "Pattern Recognition Systems," it is now 30 percent and it is climbing to 52 percent. I cite these examples as cases where we, in the U.S., were preeminent for many years and where we are now being rapidly overtaken.

2. Attached hereto are excerpts from the November 8, 1973 issue of *Electronics*. You will note the following heading: "New R&D Indicators: Picture is Grim:" Sub-heading: "NSF Board Reports Continuing Slippage in U.S. Staffs and Investment as Competing Nations Expand: Instrument Industry a Bright Spot." The following two paragraphs are separate quotations from this article:

"The picture presented in the 1973 report of the NSF's National Science Board, a 143-page document titled 'Science Indicators—1973,' contains some disturbing data for the U.S. and its electronics industries.

"The study documents a continuing downward slide in the R&D intensiveness of the five industries that account for 81% of the U.S. industrial investment in technological innovation—electrical equipment and communications, aircraft and missiles, professional and scientific instruments, machinery, and chemicals."

3. In a 1973 issue of *Scientific American*, there is an article in a section called "Science and the Citizen" under the heading, "The State of Science," (copy attached). You will find the following statement:

"How does one measure the health of a nation's science and technology, particularly at a time when U.S. science is believed to be suffering from a drastic loss of public support? The National Science Board of the National Science Foundation is attempting to make such a measurement by developing a body of data that 'would reveal the strengths and weaknesses of U.S. science and technology, in terms of the capacity and performance of the enterprise in contributing to national objectives'."

I find the following significant: "Although the board avoids drawing conclusions, the picture that emerges from its survey of resources is a descending curve."

4. In the May 12, 1973 issue of *Business Week* (copy attached), you will find the following heading: "R&D is Losing Its High Priority: The Shift is Away From New Product Development. Is Research Too Costly?" The following paragraphs are worth repeating:

"Your profits are at an all-time high. Your customers are buying your latest products faster than you can manufacture them. Your researchers tell you they have lots of ideas for new products just waiting to be developed. So, naturally, you step up your R&D spending, hoping to beat your competitors to market with that new model.

"It certainly sounds like the right scenario for 1973. But it is just not happening. According to an industrywide survey released this week by the McGraw-Hill Economics Dept., business is reluctant to increase its spending for research and development. And much of the money that is going to R&D, surprisingly enough, is aimed not at finding new products or processes but rather at improving existing ones.

"New Goal. Most startling of all, many companies are shifting their research goals. Traditionally, research is supposed to pay off in new products or processes. But 44 percent of the manufacturers surveyed said that their main R&D goal was to improve existing products. As a result, development of new products seems to be slowing. By 1976, the survey says, only 13 percent of industry sales ($135.7-billion) will be new products, down from the 18 percent that industry expected in last year's survey for 1975."

I would like to point out that these statistics were gathered before the onset of the current recession. The picture would be far worse if the figures were brought up to date.

In my opinion, this decline of the drive for new technology in the U.S. has been caused by several factors. The first is that there was an overemphasis on R&D in many of our industrial firms after World War II. In fact, Wall Street demanded that some arbitrary amount of R&D be done in a company for it to be called a "growth corporation." Some money was spent on R&D without much planning and without a clear picture of what was expected or what quality of technical personnel should do the work.

R&D, particularly research, is a very risky business. One never knows just exactly what the payoff will be or who will benefit from it. Nor can one predict as to whom the benefits will occur. *One thing, however, is certain*—that if long-term research is not done, then development will have nothing to develop and, ultimately, engineers will have nothing to engineer. Support of long-range research and advanced development must be done as an article of faith, based on past experience—experience that extends for hundreds of years. In practice, an arbitrary percentage of our gross national product must be spent on scientific research so that the progression of research, advanced development, product development, and detailed engineering must be carried out at a rate that will assure the continued growth of our economy.

Many of our corporations are no longer managed by their founders. The present day "professional manager" is often motivated by short-term interest only. He does not have any emotional involvement in his company's product, nor is he going to leave his business to his children.

Another factor that has caused the grim picture cited above is that there was disillusionment among the major suppliers of venture capital with high technology investments. Many of the investments of the 1950's and 1960's did not pay off. Investing in new technologies is always frought with great risk and almost certain losses. It is obvious that all new technical (and I imagine, social) developments cannot and should not be successful in even the best of all possible worlds. No intelligent society can adopt every new idea, even if the idea is better than the one before. Nor can one regulate the generation of new ideas to the exact number that can be adopted. In order to harvest the wheat, one must go through a great deal of chaff and a great deal of sand must be sifted to find a few diamonds. Therefore, investment in new technologies must, by the nature of the thing, be risky and the returns on the few successes accordingly great. Since private investment capital is drying up, I would strongly urge our Government to take steps to encourage private capital and, if necessary, provide investment capital directly to support the most promising inventions and innovations. This is done in many foreign countries. Attached hereto are excerpts from a report entitled "Technology Enhancement Program in Five Foreign Countries" which summarize some of the data of this report.

There is a new program of Energy-Related Inventions at the National Bureau of Standards. Through this program we can recommend to the Energy Research and Development Administration that grants be issued on promising inventions in the energy field. Consideration could also be given to encourage the Small Business Administration to provide capital to worthy technologies when private capital is not available.

Another factor that curtails the flow of capital into advanced technologies has been the high interest rates prevalent during the late 1960's and early 1970's. If money can double itself with relatively little risk in five to ten years, why should anyone invest in advanced technology or new inventions?

There are three ways by which a nation can improve its average standard of living: One is to be the happy possessor of very great natural resources which can be traded for desired goods with other nations. (For example, a small country that literally lives above a sea of oil.)

Another method by which a country can improve its standard of living is to exploit the people of another country, as by robbing them. This was a popular procedure during the past centuries but it is becoming progressively more difficult to do and, hopefully, will become impossible in the future.

The third and only way left to a civilized society, such as ours, is to improve our technology in such a way that the output of goods and services per man/woman hour of work continues to rise at a satisfactory rate. It is for this reason that I welcome the opportunity to testify here today.

In my mind, the advancement of technology is intimately interwoven with the operation of our patent system and I am dismayed by the many attacks made against this system and the efforts to limit the rights of the inventor and to reduce his incentives. This is done by well-meaning people in an effort to eliminate or to minimize some of the real or imaginary difficulties that they see in the performance of our patents.

The U.S. patent system is, in my opinion, if not the best in the world, certainly one of the best. It is not only outstanding in the details of its workings but particularly because it gives so much attention to the role of the inventor himself. While the system was basically designed to benefit the public, it makes a special effort to reward and honor the individual inventor—something which many foreign patent systems do not do to any equivalent degree.

I have plotted some interesting statistics about the way our patent system has worked during the past 20 years (See the attached plot). The number of patent applications in the U.S. has risen steadily from about 75,000 in 1954 to slightly over 100,000 in 1974. The number of patents issued has come up at a faster rate—from 35,000 in 1954 to about 80,000 in 1974. The larger increase in the number of patents issued, as compared to the number filed, has been due to the decreased pendency with which patents are being issued by the Patent Office.

A more interesting set of figures plotted shows that a percentage of U.S. patents issued to domestic corporations has remained essentially steady at about 50 percent, with some decrease in the past decade or so. The percentage of U.S. patents issued to independent inventors, however, has fallen steadily from about 44 percent in 1954 to about 25 percent in 1974. An even more interesting figure is the number of patents issued to foreign corporations. This has been rising steadily from about 5 percent in 1954 to over 20 percent in 1974. (The number of U.S. patents issued to foreign independent inventors is negligible.)

The decrease of patents issued to independent inventors is particularly significant since many of our great advances in technology have come from them. In my opinion, inventors can be roughly separated into three classes. The first is the employee of the large corporation. The second is the middle group of inventors who are highly trained and who work either in universities, Government laboratories, small businesses, or, occasionally, for themselves. The third group is the basement tinkerer—that is, a man or woman who is not highly trained technically and who comes up with inventions which, commonly, are not of great technical significance.

When one studies the output of these three groups, one finds that the great advances in technology made in our lifetime (say, roughly from the 1930's to the present) were made by the middle group of inventors and were made outside of the laboratories or the engineering departments of the largest U.S. corporations. Among these great inventions are: Atomic energy; computers; radar; xerography; microwave technology; laser; jet engines; space technology; inertial guidance; holography; instant photography (Polaroid); FM radio; medical developments (polio vaccine, heart pumps and pacers, kidney machines); flotation process for glass; fiber optics; magnetic recording; TV tape recording; magnetic core memories; color photography; continuous casting of metals; optical character recognition; mechanized wiring (printed circuits); foam rubber; the Honda engine; and the Wankel engine.

The two great advances in our technology that were made by large U.S. corporations were the Transistor, which was made at Bell Laboratories, and the Modern TV System, which came from RCA. I realize that this list is by no means complete and some items can be debated. Not being a chemist, I did not include the significant chemical patents. Nor do I want to belittle the great technological contributions made by large companies which adopted the great inventions made outside, perfected them and put them on the market. Useful technology consists not only of the brilliant breakthroughs that earn Nobel Prizes but of thousands upon thousands of important and less-known contributions that make the great breakthroughs practical and useful. Nevertheless, if the future of the country depends on giant steps forward in technology, as I believe it does, we must make sure that the opportunity and encouragement of the independent scientist and inventor must not be reduced in any way.

In this connection, I would like to quote a statement from a study made by a well known economist, Professor Edwin Mansfield, in the Journal of Political Economy, August 1964. Professor Mansfield "found that holding R&D outlays constant, the number of significant inventions made by large firms in the chemical, petroleum, and steel industries declined as the size of firm increased. Thus, contrary to popular belief, the inventive output per dollar of R&D expenditure in most of these cases seems to be lower in the largest firms than in large and medium-sized firms."

Since I say the American patent system is so great, why am I concerned? My concern is that I have been told by very high officials of several of our large corporations that many very large corporations (but by no means all) feel that they do not need a patent system, that they do not earn any appreciable percentage of their profits from royalties and since much of their research and patent work is done as a defensive mechanism against outsiders, they can prosper perfectly well on their ability to produce, sell and service their products better than any smaller competitor. For the small innovative company, however, patents are an absolute necessity; the risks are high and the protection must be available at least for some time so that a company can get a chance to grow. Thus, the patent system suffers from a lack of support from some of our largest corporations—a lack of support which is never expressed and which I cannot prove or document.

A second, and a much more direct attack on the patent system, has been carried on by the Antitrust Division of the Department of Justice for many years. I have heard some of them say, publicy and privately, that they believe in the patent system of the U.S. but that it has led to many abuses and monopoly practices which they would like to curtail. I have no quarrel with the Antitrust Division when they so ably fight excessive use of monopoly power. I agree with them that if patents are used in ways which are outside of the intended patent protection, such practices should be stopped. However, I would like to point out to the Antitrust Division that a strong patent system that encourages and protects inventors is one of the best defenses against monopoly power. In their attack on the alleged abuses, they have attacked the whole patent system and have tried to weaken it. For example, in many important court cases where patents were involved, the Department of Justice entered as a friend of the court on the side against the owner of the patent. I know of no case where the Department of Justice entered the case on the side of the inventor.

Perhaps the simplest way to illustrate their point of view is to cite an article by Morton Mintz in the Washington Post on December 4, 1972. The headline reads, "Justice Asks New Patent Procedures." The article quotes Bruce B. Wilson, a deputy assistant attorney general, as follows: "He cited 'a rather horrifying statistic': that 'more than 72 percent of the patents which have been litigated in the Courts of Appeals since 1966 were held invalid.'" What Mr. Wilson did not cite is that this 72 percent is 72 percent of less than 1 percent of all patents issued, and one could very well question whether the figure is at all significant. The article concludes, however, with Mr. Wilson's proposal of "the creation in the Justice Department of a new public patent counsel division that, on a *selective* basis, (emphasis mine) would appear before examiners and the Patent Office Board of Appeals to argue against the issuance of a patent." This is a most interesting proposal. It means that the Justice Department would entangle the applicant for an important patent in a difficult and expensive procedure whenever the Justice Department, and not the Patent Office, did not want the patent to issue. Heaven knows it is difficult enough to get a patent now and to make it economically viable later without the uncertainty of a possible fight with the Justice Department.

I would now like to quote from a speech that Judge Simon H. Rifkind delivered during the October 1972 meeting of the American Patent Law Association. The title of this talk was "Patents and Antitrust-Time for a Divorce." In this speech, he made several points. I would like to quote the following:

"As I read the Constitution, I find that the Founding Fathers regarded the progress of useful arts as a value of very high national priority and, in order to promote the realization of this value, they authorized the creation of a patent system.

"The authority which the Constitution extended has been exercised from the very beginning of our national existence. Certainly I do not have to tell this patent bar association that the first patent law was passed, I think in 1970.

"A few years ago the President's Commission on the Revision of the Patent System made extensive inquiry into the subject and arrived at a unanimous opinion that the patent system had well served the nation's interest and that it continues to serve it. Let me read you the quotation: 'The members of the Commission unanimously agreed that a patent system today is capable of continuing to provide an incentive to research, development and innovation. They have discovered no practical substitute for the unique service it renders.'

"That is high praise indeed for an institution which has been with us throughout the life of this nation, from its inception to this very day.

"Now let us look at a companion statute, the antitrust law. In comparison, the antitrust laws, although they are rooted in the common law, have no sanction in the Constitution itself, at least no explicit sanction. The power to enact the antitrust laws is derived from the commerce clause which is, of course, a blanket of tremendous magnitude.

"Federal legislation on the subject did not appear on the statute books until 1890. I should say, in comparison with the patent system, it is a relative newcomer.

"Despite this disparity in age and dignity, I think you will all agree with me that the Supreme Court regards the patent system with a jaundiced eye and treats it as subordinate to the policies expressed in the antitrust laws.

"The antitrust laws are invariably given an expansive reading, as expansive as the language will tolerate.

His conclusion is a very definite: "I should like to suggest, ladies and gentlemen, that the time has come for a divorce between patents and antitrust. As part of the divorce, I propose that patents resume their maiden name and no longer be called monopolies. I would like to suggest that patents not be subject to an antitrust defense. If the licensee or the infringer really wants to assert an antitrust claim against the licensor let him do so by an independent action but let the patent litigation proceed as a patent litigation."

In 1965, President Johnson appointed the President's Commission on the Patent System and on November 17, 1966, this Commission issued a report with its recommendations. The Commission, in discussing the antitrust laws in relation to patents, stated clearly that they did not wish in any way to suggest a weakening in the antitrust laws. They did say that there has been some difficulty in interpreting these laws in reference to patents. They recommended as follows: "All that the Commission believes to be required is explicit statutory language defining, for the purpose of assignments and licenses, the nature of the patent grant heretofore recognized under the patent statute or by decisional law. This is a right to exclude others from making, using and selling the patented invention."

As a result of this report, there were bills introduced in Congress that essentially followed all the recommendations. In relation to antitrust laws, however, there wasn't a single mention.

I had the honor of testifying before a Senate Subcommittee on Patents in 1967 on how an inventor reacted to one of the new bills. I stated that while the bill was a valid attempt to simplify some of the patent procedures, most of the recommendations to the committee and the elements of the bill were to limit the rights and freedom of the inventor. Relative to the antitrust provision, I stated that while I had no argument about the relationship of the antitrust laws with the patent laws, I felt that the relationship could be clarified by an Act of Congress—that an inventor has a right to know what his rights are so that he can plan his business deals accordingly. When he issues a license or sells a patent, he should know whether he will or will not run afoul of the antitrust laws.

Sometime later, the National Inventors Council had the pleasure of discussing the particular patent bill with a member of the staff of one of the Senators involved. When we asked why there was no mention of the antitrust-patent relationship in the bill, even though the President's Commission recommended such a section, we were told that the Antitrust Division of the Department of Justice violently opposed any such legislation by Congress and that they preferred to have case law. This situation prevails to this day. It is my opinion that laws made by judges, as a result of cases carefully selected by the Department of Justice or by others, are not the best way to create laws in a democracy. I do not believe that patents are monopolies. I am happy with the definition made by the Supreme Court before the turn of the century that a patent represents a piece of property and while the rights of patents can be abused, the abuses are no different than those of the property of land or any other.

If the Department of Justice feels that some of our very large corporations use patents to create monopolies far beyond those envisioned by the patent laws, they should suggest changes in the law which pertain to very large companies, or to specific abuses, and not attempt to weaken the whole patent system in general so that it adversely affects all inventors, many of whom would not dream of violating the antitrust laws.

I would like to make one more comment about my private discussions with some attorneys of the Department of Justice. I believe they are naive in their concept of what a patent is. In my opinion, their belief (expressed to me privately) that "anything that needs to be invented will be invented when needed" is nonsense. Another belief, suprisingly widely held, is that society does not have to reward inventors because they "would do it anyway, because they enjoy doing it so much." The act of conception of an invention may, indeed, be fun, but the hours of sweat and blood to make it a practical reality and get it into use is very hard work indeed.

Senator Bentsen raises the question on how can we improve the performance of Government scientists and, particularly, how can we improve the relationship of the Government scientists and engineers and those of the private sector. The answer, it seems to me, is to have as much expertise in the Government as possible, and this can only be done if the technical work in the Government is of the highest quality.

I hope I shall be forgiven for boasting when I point out that the National Bureau of Standards, because it does a great deal of its work inhouse and because of the excellence of its staff, has outstandingly good relations with industry. It has the respect of the industrial technical people—a respect it could not have if it were merely a contracting agency and if its technical staff merely sat at desks and handed out money.

In the ordnance business, I can cite the quality of work done by the Harry Diamond Laboratories, with which I had the honor to be associated many years ago and, again, the reasons are the same.

If industry is to respect the Government people and Government work, the Government must have high grade scientists and engineers. To attract them, to hold them and develop them, it must do considerable R&D work in its own establishment. I am very happy to have heard representatives of OMB, at a recent meeting, say that they recognize this fact. We, the engineers who work in Government, have always know this.

I have often been asked what should the policy of the Government be relative to those patents which it owns; that is, what should be done with the inventions made by Government employees or Government contractors. This is a difficult question. First of all, the number of patents per dollar that results from Government R&D is approximately equal to less than 2% of the number of patents per dollar which results from industrial R&D. Why this is so, I can only guess and I would not like to go on record with my doubts and suspicions.

The present policy of the Federal agencies relative to the patents owned by the Government is, to say the least, a hodgepodge. While some agencies follow the rules broadly laid down by the Memoranda from Presidents Kennedy and Nixon, the rules vary a great deal. Some Government agencies have statutory powers while others make decisions based more or less on precedent. The bill setting up the Energy Research and Development Administration (ERDA) states that wherever the invention was made as a result of Government contribution, ERDA takes title to the patent. However, the Administrator of ERDA has the right to issue exclusive or other limited licenses when, in his opinion, this is in the public interest.

The difficulty with such permissive legislation is that if the Administrator of ERDA is a brave and wise man, the policy is excellent but if he is afraid of criticism, then the safest thing would always be to take title. I believe, therefore, that it would be wise to pull all the different rules together and establish, by law, a unified but flexible Government policy which would spell out under what conditions the Government should take license and under what conditions it should issue exclusive or other kinds of licenses. An invention of a weapon has to be treated differently from an invention of an automobile clutch, and an invention for the cure of cancer does not need the same promotion as an invention for a computer memory. It is my sincere belief that, wherever possible, the Government should give full exclusive licenses to the inventor or to the company for whom he works even though the Government paid for the work that resulted in the invention. I say this not because I would like to see the inventors rewarded or become rich. This is really a minor consideration, except in an indirect way of encouraging other inventors. The main reason for saying this is that a patent which is free to everyone is not a patent at all and the whole intent of the patent system as an incentive to investment and innovation is destroyed by a free and/or universal licensing policy.

During World War II, the U.S. Government confiscated some 15,000 German and other enemy-owned patents. The Alien Property Custodian made them available free to any American corporation or individual who wanted to use them. The patents died on the vine. Our Government, at present, holds title to some 22,000 patents. The experience in freely licensing them is also far from encouraging. Society would be better served if an invention went into production even though the inventor or his backers make money on something which the Government had financed.

There have been suggestions made that when the Government lets one of its inventions go into private hands it should collect royalties to recompense it. To me, this is a rather childish suggestion because the U.S. Government is automatically a 50 percent partner in any profit that anyone makes on an invention. This 50 percent refund of the profits in the form of income taxes goes on not only during the life of the patent but forever or, at least, for the life of the corporation or the inventor. Moreover, the procedures for collecting Federal taxes on income, to say nothing of other Federal, State and local taxes, are much simpler than the collection of royalties with its contracts, inspections, and many legal problems.

In the testimony of Thomas E. Kauper, Assistant Attorney General of the Antitrust Division, Department of Justice, before the Subcommittee on the Environment Committee on Interior and Insular Affairs on February 1, 1974, you will note that in the case of Government-financed inventions the Department of Justice takes the position that they believe in the "title" policy of the Government; that is, the Government should own the rights to those inventions where the Government paid for the R&D work. He also states that the Department of Justice believes in the mandatory licensing of inventions, even when they are developed independently of Government support, when the Government or a court feels that such mandatory licensing is reasonably necessary for the common good. This is difficult to argue since the U.S. has no mandatory licensing except when the Government wants a license for its own use. It is difficult to foresee how this would work out. This is another illustration that when patents are discussed by the Antitrust Division of the Department of Justice the drive is always to limit the rights of the inventor. I know of no case where the reverse was true.

I do not believe that the Patent Office should impose maintenance fees on patents, as is being proposed in Congress today. Some of these bills will make the inventor pay $2,000–$3,000 during the life of the patent, in addition to the initial filing fees. This is common practice in Europe and I can testify from my own experience that this leads to abandonment of patents before the patent rights would normally expire. If the fees are intended to simply pay the Patent Office for its expenses, then I would suggest that this is not justified. The Patent Office benefits society far more than it benefits the inventor. These maintenance fees will make many inventors abandon patents before the payments come due and, in my opinion, this is simply a way of shortening the life of a patent and further reducing the rewards and motivation which the patent system is supposed to provide. I have been told that the income to the U.S. from abroad, in hard cash, based on royalties collected from foreign corporations, is more than $1 billion a year. It seems obvious that the taxes on this money (to say nothing of the taxes on the money earned in patents by U.S. corporations at home) are far greater than any possible cost of the patent system.

If we really want to know one aspect of the direct economic value of the U.S. Patent System, then I suggest that the Internal Revenue Service add one line to its present tax-return forms. The line should read: "Royalties on Patents." By summarizing these amounts, we would finally know whether the Patent Office fees really need to be raised to make it "self supporting," even in a simple-minded, direct dollar-and-cents way.

If we really want to improve the patent system of the U.S., a study should be made of the abuses in which patents may have been involved and the laws should be modified to correct these abuses. The whole patent system should not be weakened and the rights of the inventor should not be reduced at a time when we need more technological advancement to improve our balance of trade and improve our standard of living.

The Patent Office has progressively more and more difficulty in examining patent applications, both because the number of documents that have to be examined is constantly rising and because the sophistication of the technology is continuously increasing. I, therefore, urge the Congress to permit the Patent Office to increase both the quantity and the quality of its examining staff.

Our laws should be clarified so that the courts would have a better basis by which to judge the validity of a patent. I find it strange and incomprehensible that in certain districts in the U.S. the courts never hold the patent valid and, in some districts, more than one-half are held valid. I think the definition of what is an invention, what is obvious, and what is not obvious can be formulated. I sincerely hope that these technical matters and the rights of licensing could be clarified by the Congress. I urge this committee not to support any legislation that weakens the patent system and reduce the incentives to inventors and their backers. If there are any problems, we should not "throw out the baby with the bath water."

Thank you.

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TABLE 1.—AREAS WITH HIGHEST FOREIGN SHARE OF U.S. PATENTS

Rank and area	Foreign share (percent)	Page
1—Manipulation of weft drawn from a stationary package	85	161
2—Electrically operated camera shutters (3, 4)	80	43
3—Piezoelectric compositions	78	157
4—Certain anthraquinone compounds	78	153
5—Purification of molten iron	77	101
6—Preparing cast iron (2)	74	103
7—Magnetic field responsive resistors (2)	72	75
8—Nonlinear rods, strands or fibers	72	93
9—United-needle knitting machines (4)	70	159
10—Accelerator responsive automatic transmissions (2)	69	173
11—Light sensitive silver compositions with processing ingredient	66	39
12—Formaldehyde polymers	63	137
13—Automotive fuel control devices (4)	61	157
14—Superconductors (2)	60	79
15—Film roll-holding devices for cameras	60	45
16—Fuel injection pump apparatus for internal combustion engines	57	171
17—Homopolymers of certain vinyl halides	56	147
18—Fluid pressure driven metal deforming apparatus	55	111
19—Textile twisting with fluid jet	55	163
20—Certain copolymers prepared from unsaturated halo-hydrocarbon monomers (2)	54	145
21—Ground effect machines	54	181
22—Forming a composite or stratified article of indefinite length from a plastic or nonmetallic materials	53	131
23—Structurally defined rods, strands or fibers	52	95
24—Making strands from synthetic fibers	52	165
25—Semiconductor internal structure	52	81
26—Magnetic sound recording and reproducing structures (2, 3, 4)	52	57
27—Polyamide resins derived from amino carboxylic acids (2, 3, 4)	50	139
28—Magneto-hydrodynamic generators	49	61
29—Ignition timing control for internal combustion engines (4)	49	169
30—Electrical generator voltage regulation	49	65

Note: (1), (2), (3), (4)—Also appears on table 1, 2, 3, or 4, respectively.

TABLE 2.—AREAS WITH HIGHEST PROJECTED RATE OF INCREASE OF FOREIGN SHARE OF U.S. PATENTS

Rank and area	Foreign share (percent)	Projected foreign share (percent)	Page
1—Magnetic field responsive resistors (1)	72	+90	7-
2—Liquid fuel rockets	31	65	18-
3—Electrolytic coating of a localized area	30	61	12·
4—Telephone recording systems	35	70	5-
5—Certain copolymers prepared from unsaturated carboxylic acid ester monomers	42	82	143
6—Analysis of complex electrical signals	23	44	47
7—Electrical high frequency filters	22	43	77
8—Certain interpolymer resins	42	82	141
9—Automatic electric arc welding and cutting	41	78	109
10—Adhesive bonding of surfaces combined with a cutting step	30	57	127
11—Extrusion apparatus containing a shaping orifice	43	80	129
12—Accelerator responsive automatic transmissions (1)	69	+90	173
13—Pattern recognition systems	30	52	49
14—Electrical signal delay devices	29	50	73
15—Pressure responsive spray apparatus	45	77	201
16—Electronic musical instruments	41	70	59
17—Superconductors (1)	60	+90	79
18—Electronic pulse modulators	31	52	69
19—Packaging with package contents treating	21	36	203
20—Digital logic circuits	23	38	67
21—Certain amine- or amide-aldehyde mixed synthetic resins	40	67	149
22—Electrical conversion systems	48	80	63
23—Processes for manufacturing dynamoelectric machines	27	45	113
24—Electronic pulse generators	24	40	71
25—Synthetic resins admixed with a solvent	41	67	133
26—Suction dredgers	48	79	191
27—Polyamide resins derived from amino carboxylic acids (1, 3, 4)	50	80	139
28—Certain copolymers prepared from unsaturated halo-hydrocarbon monomers (1)	54	86	145
29—Magnetic sound recording and reproducing structures (1, 3, 4)	52	82	57
30—Preparing cast iron (1)	74	+90	103

Note: (1), (2), (3), (4)—Also appears on Table 1, 2, 3 or 4, respectively.

[From the Nov. 8, 1973, issue of Electronics]

NEW R. & D. INDICATORS: PICTURE IS GRIM

NSF BOARD REPORTS CONTINUING SLIPPAGE IN U.S. STAFFS AND INVESTMENT AS COMPETING NATIONS EXPAND; INSTRUMENT INDUSTRY A BRIGHT SPOT

(By Ray Connolly, Washington bureau manager)

Electronics and other high-technology industries in the U.S. that compete in the global market are beginning to get a comprehensive picture of where their country stands relative to the rest of the world in research and development. The image isn't a bright one, with R&D "intensiveness" continuing to drop.

The picture presented in the 1973 report of the NSF's National Science Board, a 143-page document titled "Science Indicators—1973," contains some disturbing data for the U.S. and its electronics industries. Described by board chairman H. E. Carter as "the first results from a newly initiated effort to develop indicators of the state of the science enterprise in the U.S.," the report provides a new perspective on the decline of the U.S. investment of dollars and manpower in R&D in comparison with development efforts of other major powers during the past decade (see "R&D money and people").

The study documents a continuing downward slide in the R&D intensiveness of the five industries that account for 81% of the U.S. industrial investment in technological innovation—electrical equipment and communications, aircraft and missiles, professional and scientific instruments, machinery, and chemicals. R&D intensiveness—defined as "the proportion of the total human and financial resources" invested by an industry—is measured chiefly by the number of R&D scientists and engineers per 1,000 employees, as well as the percentage of net sales devoted to R&D. Aircraft and missile manufacturers, for example, topped the five most intensive industries in 1970 with a people ratio of 74 per 1,000 and an investment of 18.5% of net sales. For the second-place electronics industries—which the Government includes in electrical equipment—there were 39 people and 7.5%, with instruments accounting for 31 people and 5.9%.

The indicators show that R&D intensity in the five industries dropped 25% between 1964 and 1970, although an upturn is now in sight. The downturn is believed related to another negative—the declining U.S. trade balance that has already developed in such technology-intensive industries as electronics.

"Though the U.S. maintains a strong position as a net exporter in these industries overall," the report declares, "indicators suggest that position may deteriorate in the near future with an increasing rate of decline in electronics exports and a less favorable ratio" of exports to imports for aircraft. That ratio stood at 9-to-1 in 1971. Instruments of all types proved the single exception, maintaining a steadily increasing annual investment in constant dollars over the 1961-1970 decade with its own funds (see "Industry spending for R&D, 1961–70"). And instruments have continued to post small but steady gains in net exports year after year.

Upbeat.—Although company expenditures for R&D in electronics and electrical equipment, which lead all other U.S. products, continue to rise, purchasing power was essentially unchanged between 1969 and 1970 after inflation was factored in. Nevertheless, an NSF survey of the 50 largest U.S. corporations, completed last year, shows electronics companies projecting increases in their own R&D investments between 1972 and 1975 in line with an all-industries forecast of 25%. Aerospace companies, on the other hand, "foresee future R&D growth at a pace somewhat below the rest of industry."

As for engineers and scientists employed in general industrial R&D, the number is projected to increase to 260,000 by 1975, up from a 1971 low of approximately 225,000. Again, electronics companies estimate they will absorb a proportionate share of this 16% increase. However, if the industrial R&D pattern set in the decade through 1972 continues as expected, the distribution of these jobs will change as emphasis on development increases.

The declining emphasis on basic research poses a long-term problem for the growth of electronics industries, especially in developing sources of engineering and scientific manpower. General Electric Co., for example, recently expressed concern to NSF's director H. Guyford Stever over what its R&D vice-president Arthur M. Bueche called "the indicators on our precious manpower resource."

Those indicators show that total national outlays for basic research in universities and colleges declined between 1968 and 1972 when measured in constant purchasing power of 1961 dollars. Affected disciplines included engineering and physics—both critical to electronics and the resupply of manpower pools for industry.

"Despite the grim picture, we at least have better data," observes staff director Robert Brainard.

"One of the past criticisms of our indicators is that they were not interpretive enough," Stever recalls. With NSF's new and more detailed annual analyses, he notes, "conclusions are being drawn, and we expect broader interpretations in the report next time." If U.S. technology is to get the funds it requires, Stever points out, "we have to have better things to say to the Office of Management and Budget and the Congress." Clearly, NSF's director believes he now has them.

[From the Science and the Citizen section of a 1973 issue of the Scientific American]

THE STATE OF SCIENCE

How does one measure the health of a nation's science and technology, particularly at a time when U.S. science is believed to be suffering from a drastic loss of public support? The National Science Board of the National Science Foundation is attempting to make such a measurement by developing a body of data that "would reveal the strengths and weaknesses of U.S. science and technology, in terms of the capacity and performance of the enterprise in contributing to national objectives." The first fruits of the effort—a set of indicators dealing mainly with resources and therefore concerned more with capacity than with performance—have been published by the board under the title *Science Indicators* 1972.

Although the board avoids drawing conclusions, the picture that emerges from its survey of resources is a descending curve. Examining the international position of U.S. science and technology, the board found that the proportion of the gross national product spent for research and development declined in the U.S. (from almost 3 percent in 1967 to 2.6 percent in 1971), France and the United Kingdom while rising in the U.S.S.R., Japan and West Germany. The number of scientists and engineers engaged in research and development per 10,000 of population "declined in the United States after 1969 but continued to increase in the U.S.S.R., Japan, West Germany and France, with the result that by 1971 the number . . . for the U.S.S.R. was 37 as compared with 25 for the United States and Japan, 15 for West Germany and 12 for France."

Indicators dealing with resources for research and development showed that in terms of constant (1958) dollars national expenditures declined 6 percent between 1968 and 1971 and rose slightly in 1972. By the same measure the Federal Government's expenditures for research and development declined 12 percent from 1968 through 1971. Moreover, although the share of Federal expenditures giong for research and development in areas others than national defense and space exploration rose from 14 percent in 1963 to 27 percent in 1972, defense and space research still took 73 percent of the money in 1972.

National expenditures for basic research, the board found, rose in terms of current dollars from 1960 to 1972, but in constant dollars "spending in 1972 was approximately equal to the 1967 level and some 6 percent lower than the peak year of 1968." The decline "was least in universities and colleges (3 percent) and largest in industry (14 percent)." In constant dollars the funds provided by the Federal Government for basic research declined 10 percent from 1968 to 1972. Moreover, "Federal support for young investigators (those holding a Ph. D. less than seven years) in universities and colleges declined to a greater than support for senior investigators."

Measuring the research and development "intensiveness" of U.S. industry by comparing the ratios of research and development expenditures to net sales and of research and development scientists and engineers to total employment, the board found that the figure rose between 1960 and 1964 "but declined thereafter to a level in 1970 which was lower than in 1960." The largest declines occurred in the industries that are most active in research and development. Another finding was that the proportion of industrial scientists and engineers employed by large companies (5,000 employees or more) rose from 70 percent in 1958 to 85 percent in 1971, whereas the proportion in small companies (1,000 employees or fewer) declined from 20 to 6 percent. Noting "ample historical evidence to suggest that small firms have produced more than a proportionate share of major innovations," the board warned that the changing employment figures "may be a dangerous signal."

[From the May 12, 1973, issue of Business Week]

R. & D. Is Losing Its High Priority

THE SHIFT IS AWAY FROM NEW PRODUCT DEVELOPMENT. IS RESEARCH TOO COSTLY?

Your profits are at an all-time high. Your customers are buying your latest products faster than you can manufacture them. Your researchers tell you they have lots of ideas for new products just waiting to be developed. So, naturally, you step up your R&D spending, hoping to beat your competitors to market with that new model.

It certainly sounds like the right scenario for 1973. But it is just not happening. According to an industrywide survey released this week by the McGraw-Hill Economics Dept., business is reluctant to increase its spending for research and development. And much of the money that is going to R&D, surprisingly enough, is aimed not at finding new products or processes but rather at improving existing ones.

The survey shows that U.S. Business expects to perform $21.2-billion worth of R&D this year, including government-sponsored research, which accounts for rougly half of the total. Though the total is 5% higher than last year, the gain is more than offset by inflation. The study says: "The salaries of scientists and engineers and the prices of materials . . . no doubt will increase more than 5.5%." It also reports that R&D expenditures will slip to 2.4% of industry sales in 1973 compared with 2.5% in 1972—with a further decline to 2.3% expected in 1976.

New goal.—Most startling of all, many companies are shifting their research goals. Traditionally, research is supposed to pay off in new products or processes. But 44% of the manufacturers surveyed said that their main R&D goal was to improve existing products. As a result, development of new products seems to be slowing. By 1976, the survey says, only 13% of industry sales ($134.7-billion) will be new products, down from the 18% that industry expected in last year's survey for 1975. Only three industries—instruments, machinery, and electrical machinery—expect 20% or more of their sales three years hence to come from new products. In short, companies now seem less willing to gamble on new products.

One reason for the trend may be that R&D is becoming too expensive. In a recent speech, Dr. T. R. Hopkins, president of Gulf Research & Development Co., said that the cost of supporting a professional researcher now runs $75,000 a year. Moreover, for every dollar spent on research, he said, a company must eventually invest $10 to manufacture the product developed. "At a 10% return, it gets back only the cost of doing research. At 20%, it could make a little money after paying for research and writing off duds. In other words, research is out-pricing itself."

Other companies apparently agree. This month, Aluminum Co. of America merged its long-independent research lab with three development divisions. The aim: to tie research more closely to marketing and manufacturing. And in a recent survey, the National Science Foundation reported that R&D executives were "closely scrutinizing" research to see if conformed to "business objectives." The NSF also said that many companies, rather than doing their own R&D, were tapping work done elsewhere.

Such attitudes mean that corporate labs are less likely than ever to achieve major research advances. In the McGraw-Hill survey, 73% of the manufacturers said they did not anticipate "a technological or basic research breakthrough" by 1976. The danger, of course, is that companies striving for near-term profit may be sacrificing long-term potential. As McGraw-Hill economist Douglas Greenwald puts it: "It may mean a lower growth rate in years to come."

A SLOW RISE IN RESEARCH AND DEVELOPMENT MEANS FEW NEW PRODUCTS IN 1975

Industry	1973 planned (millions)	1972 estimated (millions)	1972–73 percent change	R. & D. as percent of sales 1972	R. & D. as percent of sales 1973	New products as percent of 1976 sales	Volume of new product sales in 1976 (billions)
Electrical machinery and communications	$5,179	$4,840	7	8.45	8.15	20	$16.4
Aerospace	4,778	5,138	−7	22.17	18.24	18	6.13
Autos and other transportation equipment	2,302	2,093	10	2.87	2.81	13	11.11
Machinery	2,285	2,059	11	3.06	2.99	26	26.83
Chemicals	2,066	1,931	7	3.36	3.3	14	10.79
Scientific instruments	996	931	7	7.43	7.1	22	4.32
Petroleum products	556	530	5	1.88	1.86	5	1.93
Food and beverages	245	233	5	.21	.2	9	14.28
Fabricated metals and ordnance	237	228	4	.56	.53	15	8.51
Paper	220	208	6	.73	.72	14	5.33
Stone, clay and glass	217	175	24	.72	.83	13	3.91
Rubber products	216	193	12	1.08	1.11	17	4.0
Nonferrous metals	159	147	8	.75	.72	8	2.19
Steel	149	148	1	.34	.3	7	4.12
Textiles and apparel	58	55	6	.1	.09	9	3.83
All manufacturing	19,844	19,093	4	2.55	.4	13	134.75
All industries	21,229	20,192	5	NA	NA	NA	NA

[Excerpts from "Technology Enhancement Programs in Five Foreign Countries"]

A variety of technology enhancement mechanisms and incentives currently in operation in the five countries are, in effect, designed to encourage the commercialization, utilization, and diffusion of newly-developed technologies and products throughout industry. In this manner, modernization of equipment and facilities and increased industrial productivity are not only accelerated but made part of a continuous process of industrial technological renovation; e.g., Preproduction Order Support Program and Investment Grant Program (U.K.), first-year depreciation allowance and tax deduction on newly acquired assets (Japan), and PAIT (Canada).

The stimulation of invention and innovation and the commercialization of research findings that are in the public interest and appear to have a good industrial potential have received strong support by the governments of the five countries. All have established a special agency to deal specifically in this area; e.g., CPDL (Canada), ANVAR (France), JRDC (Japan), NRDC (U.K.), and Garsching Instrument (Germany). These agencies evaluate research findings primarily of government research laboratories and institutes, although the last four also accept applications from private research laboratories and private inventors. All five agencies underwrite part of the full cost of developing a new technology or product and require repayment of their investment plus the payment of royalties only in the event the venture is successful.

Two of the five countries under consideration have established special, government-supported independent lending agencies to provide development funds at favorable terms to qualified applicants. They and most of the others have initiated special programs to accomplish the same objectives; e.g., JDB and two lending institutions for small- and medium-sized enterprises (Japan), CNME and IDI (France) and Launching Aid (U.K.).

Because smaller and medium-sized firms normally face problems that are directly related to their size, most of the governments have established several technology enhancement programs and incentives that are especially designed to stimulate R&D activities and the application of newly-developed technologies in such firms; e.g., Financial Support for R&D in Industry (U.K.), two lending institutions and special tax incentives (Japan), IDI and Aid to Pre-Development (France).

The establishment of research associations and joint ventures both among private firms and between a government agency and one or more private firms is generally encouraged and supported whenever such action is likely to stimulate greater R&D activity or develop a needed new technology. Such arrangements are condoned in the interest of avoiding duplication of R&D, pooling resources, and spreading the risks. In cases involving the development of new expensive technologies the usual procedure is to create a quasi-government, special charter corporation. In Germany, such corporations may or may not be profit-making organizations. In Japan, they remain under government control during the development period of the new technology, and in Canada a number of consortia are currently in operation in which the government, industry, and a university are participants. The U.K. operates a special Grant Program to Research Associations to encourage cooperative industrial and product research among groups of firms with similar interests. In France, the Technical Professional Centers for which the Aid to Pre-Development Program has been designed are in reality industry associations.

A high level of funding provided for the implementation of a specific technology enhancement program does not necessarily ensure success or accomplishment of objectives. Several technology enhancement programs in the five countries have not been particularly successful in spite of the availability and spendin gof large amounts of money; e.g., Investment Grant Program, Financial Support for R&D in Industry, and Launching Aid (U.K.), Plan Calcul (France), IRDIA (Canada), New Process for Olefin Products (Japan). Fear of government interference and excessive "red tape" were the principal reasons given by the private sector.

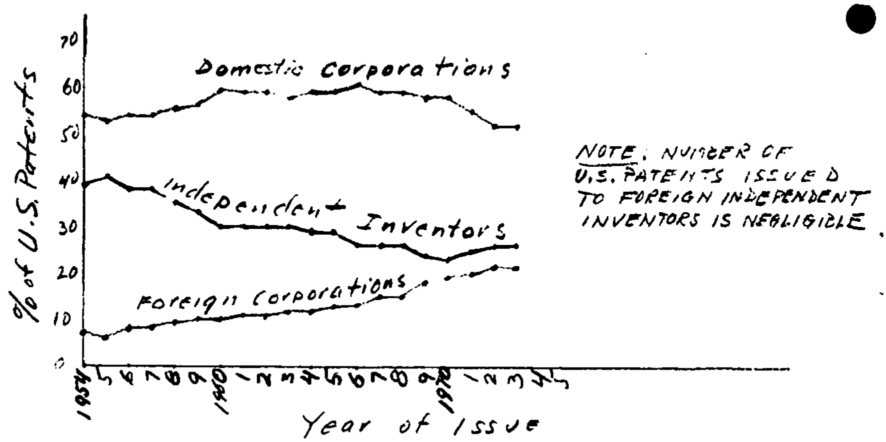
Success or effectiveness of a technology enhancement program is not necessarily a function of the time that the program has been in operation. The average duration of projects under the majortiy of programs discussed in the report is around three years, and it takes at least twice as long a time to identify and correct any defects that may be inherent in a program. Consequently, it is said that no assessment of the success or failure of a program can be meaningful unless the program is allowed to operate a reasonable amount of time; e.g., NRDC and Preproduction Order Support Program (U.K.), JDB (Japan), IRAP and PAIT (Canada), Concerted Actions Program (France). On the other hand, several technology enhancement programs have been successful after only a short period of operation, suggesting that, more than anything else, the inherent characteristics of a program rather than either duration or level of funding determine its effectiveness; e.g., the National R&D Program and the Atomic Energy Program (Japan), Investment Grant Program (U.K.), ANVAR and IDI (France), Industrial Post-doctorate Fellowship Program and IDAP (Canada), Garsching Instrument and New Technologies Program (Germany).

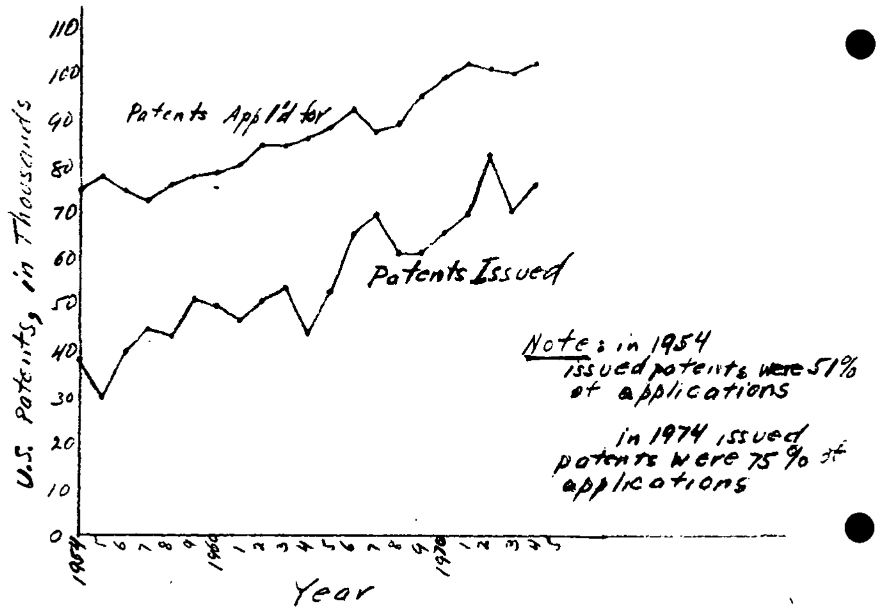
INTRODUCTION

Competition among advanced nations in the sale of their internationally-traded products, especially high-technology products, has increased greatly in recent years, and will become even more intense in the years ahead. This competition for market penetration and acquisition is the result of concerted efforts exerted by several countries during the last 20 years to modernize plants and other facilities, to restructure their industrial complex and upgrade the technological base of the country, helping thereby to capture a "fair" share of the world market for their industrial products. In addition, these countries have initiated a variety of civilian technology enhancement programs designed to encourage the development and application of new technologies through R&D investment and productivity enhancement in designated industrial sectors, promotion of joint ventures, and the use of tax and other incentives.

The United States is perhaps the only advanced nation in the free world which has not undertaken national programs to stimulate technology development in the civilian sector. It has, of course, provided considerable financial support of basic research, and has spent enormous amounts of money on R&D contracts to industry in connection with the country's defense and space exploration programs. Although the "spin off" to the civilian sector resulting from these expenditures (e.g., computers and civilian aircraft) has been rather limited, the programs helped the nation to attain technological leadership and a competitive advantage in world markets. That position remained virtually unchallenged throughout the fifties and the sixties.

Until recently, the United States had not even considered the question of government support to technology enhancement programs in the civilian sector. One reason was doubt about the effectiveness of such programs, particularly when applied to the U.S. cultural and institutional environment, and a second was inexperience in technology enhancement in areas other than space and defense. A third reason was the lack of adequate data and information to allow an objective evaluation of the potential impact of such programs, or the performance of cost-benefit analyses, or, more importantly, an assessment of the alternative uses (and potential benefits) for the government funds involved.





[Testimony of Jacob Schmookler Before the Subcommittee on Antitrust and Monopoly of the Committee on the Judiciary, U.S. Senate, May 26, 1965]

The Size of Firm and the Growth of Knowledge

The effect of the size of firm and market structure on economic progress has been a source of controversy for centuries. Much has been said but very little has been proved on both sides. If public policy is to reflect wisdom on this score, we shall have to replace plausible conjecture with hard fact.

This we are just beginning to do, but in my judgment many new facts will have to be put in evidence before abandonment of our antitrust laws can be justified. Quite the contrary, unless the evidence examined below can be explained away, it suggests that a more vigorous antitrust policy than our present one may be appropriate. Such an interpretation may indeed prove correct in the end. However, while I believe the evidence damages the case for bigness, it hardly seems solid enough to prove the case for smallness.

I shall confine my discussion to the effect of size of firm on the production of technological knowledge, because I know more about it. Of course, a balanced view of the relation of antitrust to economic progress would also need to take into account the effect of size of firm on the *use* of knowledge, and of market structure on both the production and use of technological knowledge, as well as other matters.

As soon as we ask, how does an increase in the size of firm affect the rate at which it produces new technological knowledge, a very embarrassing problem arises to cast a pall over the whole proceedings: how do you measure new technological knowledge? Theoretically it could be measured, at least for the purposes of the present problem, by the increase in output per unit of composite input that it makes possible. We have no such measure now, and until we do, any inferences based on other data must necessarily be provisional.

* * * * * * *

The greater R&D outlays per patent of large firms has several possible explanations. One is simply that large firms patent relatively less. This is probably true. However, well over three-fourths of the large firms participating in the N.S.F. survey reported in interviews that they patented "everything that is patentable or worthwhile".[1] In any case the difference in outlays per patent between large and small firms seems too great to be accounted for in this way.

Another possible explanation is that (*a*) the patents pending per firm reflect the efforts of both R&D and other technical and supervisory personnal, and (*b*) the inventive output of non-R&D personnel declines as the size of firm increases. The result is that small firms have a smaller R&D outlay per patent, because some of the relevant costs of inventing are borne by operating departments. I believe that a significant part of the result is to be explained this way, and I shall return to this point in another context later in the paper.

Still another possibility is that the inventions made differ according to size of firm, with big firms producing bigger, better, and more expensive inventions. Undoubtedly the inventions of big firms differ from those of small ones. They certainly seem to be more expensive. That they are, on the average, better in some significant sense seems very doubtful, however. Professor Edwin Mansfield found that holding R&D outlays constant, the number of significant inventions made by large firms in the chemical, petroleum, and steel industries declined as the size of firm increased. In Mansfield's words, "Thus, contrary to popular belief, the inventive output per dollar of R&D expenditure in most of these cases seems to be lower in th elargest firms than in large and medium-sized firms."[2] Since the inventions made by large firms are more likely to receive publicity than those by small firms, and therefore were more likely to be included in Mansfield's study, I regard this result as especially significant since it emerges despite the presumptive bias in the opposite direction.

The assumption that the inventions of large firms tend to be inferior to those of small ones receives support from another direction. Two independent studies indicate that small firms use commercially a larger proportion of their patented inventions than do big firms. A very extensive survey, based on a 2-percent random sample of inventions patented in 1938, 1948, and 1952, conducted by the Patent Foundation of George Washington University, found that large firms used only 51 percent of their inventions commercially compared to 71 percent for small firms. "Large" firms were defined as those holding over 100 patents, or with some patents and over $100 million in assets. The group also included some firms with between 75 and 100 patents. "Small" firms consisted simply of those not defined as large.[3] The use rate for patents held by large firms, indeed, was not significantly larger than the 49-percent use rate for inventions made by independent inventors.[4] This fact is indeed striking, since independents often have to go into business for themselves before they can use their ideas.

An entirely different study, conducted at the Harvard Business School, of very large firms exclusively, and covering all patents held by the firms surveyed,

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[1] National Science Foundation. *Science and Engineering in American Industry: Final Report on a 1953–54 Survey*, NSF 56–16, p. 37.

[2] Edwin Mansfield, "Industrial Research and Development Expenditures: Determinants, Prospects, and Relation to Size of Firm and Inventive Output," *Journal of Political Economy*, August 1964, 336.

[3] Barkev S. Sanders, "Patterns of Commercial Exploitation of Patented Inventions by Large and Small Companies," *Patent, Copyright, and Trademark Journal*, spring 1964, Tables 1 and 3.

[4] *Patent, Copyright, and Trademark Journal*, conference supplement, 1958, Appendix, Table 11.

found that firms with 1956 sales exceeding $500 million used only 51 percent of their patented inventions, compared to 56 percent for firms with sales of less than $500 million.[5]

In brief, existing comprehensive indexes of output of new technological knowledge suggest that beyond a certain not very large size, the bigger the firm the less efficient its knowledge-producing activities are likely to be. Evidently, as the size of firm increases, there is a decrease per dollar of R&D in (*a*) the number of patented inventions, (*b*) the percentage of patented inventions used commercially, and (*c*) the number of significant inventions.

These apparent disadvantages of bigness could be offset if R&D expenditures per firm rise as fast as R&D efficiency declines. While there is some tendency for this to happen, the tendency does not persist among the largest firms—the size group that is the natural focus of antitrust questions. It is true that the proportion of firms doing research and development increases as the size of firm increases. For * * *

[From the Washington Post, Dec. 4, 1972]

JUSTICE ASKS NEW PATENT PROCEDURES

(By Morton Mintz)

The Justice Department yesterday proposed a radical change in the way patents are issued and said it could produce huge cash savings for consumers.

Just one important patent, if wrongly issued, grants the holder a 17-year monopoly that can result in "immense costs to consumers," the second-ranking official in the department's Antitrust Division said.

Settlements exceeding $80 million already have been reached merely in the litigation resulting from the issuance of a patent on tetracycline, the antibiotic, that was obtained by fraud on the Patent Office, said Bruce B. Wilson, a deputy assistant attorney general.

Even the $80 million does not reflect "the full magnitude of overcharges that were made based primarily upon an invalid patent," he added.

Invalid patents are in fact commonplace, Wilson said in a speech prepared for the Philadelphia Patent Law Association.

He cited "a rather horrifying statistic": that "more than 72 percent of the patents which have been litigated in the Courts of Appeals since 1966 were held invalid."

Under present procedures, a person seeking a patent on an invention claimed to promote science and the useful arts files an application. His lawyers then argue the case for issuance before Patent Office examiners in an *ex parte*, or one-sided, proceeding.

There is none of "the scrutiny and illumination provided by an adversary proceeding," Wilson said. "But an adversary proceeding would provide additional protection for the public against unwarranted monopolies," he added.

Wilson proposed the creation in the Justice Department of a new public patent counsel division that, on a selective basis, would appear before examiners and the Patent Office Board of Appeals to argue against the issuance of a patent.

He estimated the cost at between $5 and $6 million a year, compared with current annual budgets of $12.8 mlilion for the Antitrust Division and more than $57 million for the Patent Office, which is a unit of the Commerce Department. "I think this money would be well spent," Wilson said.

[From the Wall Street Journal, Dec. 1, 1972]

PUBLIC PATENT COUNSEL, PART OF ANTITRUST UNIT, URGED BY JUSTICE AIDE

ADVERSARY PROCESS ON APPLICATIONS WOULD ALLOW CAREFUL SCRUTINY; 72 PERCENT REVIEWED SINCE 1966 INVALID

WASHINGTON—A top government antitrust official urged the creation of a "public patent counsed division" in the Justice Department to argue against issuance of patents in selected cases.

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[5] Frederic M. Scherer *et al.*, *Patents and the Corporation*, a report on industrial technology under changing public policy compiled by nine graduate students at Harvard Business School during the 1957–58 academic year, p. 112.

Bruce B. Wilson, deputy chief of the department's Antitrust Division, said government lawyers, arguing on behalf of the public would represent an initial step in reducing patent law uncertainty and "could improve the quality and reliability of patents." He made the remarks in a speech prepared for delivery before the Philadelphia Patent Law Association.

The government lawyers would appear before patent office examiners and appeals boards in selected, significant patent- applications proceedings Mr. Wilson suggested that their participation would turn patent-application bids into "adversary proceedings," in contrast to the present situation where skilled attorneys are on the side of the applicant—frequently large corporations on their research employes—arguing that patents should be issued. Adversary proceedings, Mr. Wilson insisted would "even up the sides" by enabling the patent office to scrutinize applications more thoroughly and by protecting the public against "unwarranted" patents.

Patent law uncertainty has stemmed in part from the many patents approvals that have been thrown out in the courts in recent years. Mr. Wilson said more than 72% of the patents reviewed by federal appellate courts since 1966 have been held invalid.

"A patent system which results in more than half of the patents which end up in court being declared invalid isn't working very well," he declared. Moreover, Mr. Wilson suggested that greater certainty of the validity of granted patents could well save big corporate patent holders large sums of money. For instance he said, settlements in the tetracycline drug litigation, which involved patent validity as well as antitrust issues, already have cost drug makers more than $80 million.

Mr. Wilson didn't say whether he thought his suggestion for public patent counsel may be adopted. However, during its last session Congress began to consider patent law revisions and the lawmakers are expected to return to the subject in January.

Chairman BENTSEN. Mr. Ramsey, would you proceed with your statement.

STATEMENT OF NORMAN RAMSEY, HIGGINS PROFESSOR OF PHYSICS, HARVARD UNIVERSITY, AND PRESIDENT, UNIVERSITIES RESEARCH ASSOCIATION

Mr. RAMSEY. The characteristic that distinguishes great research from merely good research is the quality of the questions asked. Once the important questions have been well and clearly formulated, the research is well underway. From this point of view, I would like to congratulate the subcommittee and its staff for their excellent and thought-provoking questions in the letter I received from he chairman. In fact, I am so impressed by the questions in the chairman's letter that I shall focus most of my remarks in turn on each of the topics raised in that letter with the headings of the first four topics being direct quotes from that letter.

I

You asked me to comment on "Conditions of management, organization, funding, Government policy, and other relevant factors that favor success in basic research and in transferring its results to commercial applications". The first portion of this topic concerns success in basic research itself, as it clearly should, since without success in basic research the applications cannot follow. Here I believe that many of the conditions have been remarkably favorable during most of the past 25 years or so, resulting in a flowering of American science during that period, and its advance to a position in which it clearly leads the world. Much of this success has been due to wise management policies in the Government funding of basic research. The three research supporting agencies with which I have been most closely associated are ERDA—AEC—NSF and ONR, especially before the drastic curtailment of ONR's basic research activities. These agencies have done a remarkably fine job in supporting research, and their dedicated staffs have a clear understanding of the delicate conditions under which basic research flourishes.

With the successes of basic research in this country in recent decades, it might seem unnecessary to devote much time to this aspect of the problem. However, this is not the case. At present, there are serious threats to basic research. Since mankind is left with many unsolved problems, there is a misguided tendency on the part of many people to forget the enormous positive results of research contributions from which we all benefit, and to blame research for not having solved everything. In recent years, the financial support in most areas of basic research has fallen far behind the inroads of inflation while at the same time the support of research in foreign countries has markedly risen. Furthermore, there is a very real danger that misguided efforts to increase the commercial applications of basic research may, though unintentionally, seriously inhibit the basic research itself, and at the same time dissipate our resources in unrewarding directions. Although there is a real place for research whose potential applications can be clearly foreseen, most technological advances have arisen from basic research whose applications were not foreseen. This was true of basic research many years ago, as illustrated by a later quote I will make from a speech by Sir J.J. Thomson in 1916. Incidentally, Sir J.J. Thomson was working on a field that later became electronics, though it was not then recognized as such; and he was under vigorous attack for dissipating his efforts on work that had no foreseeable applications. His only defense was to say that in an earlier period, X-rays had been discovered by the same process of pure research without predictable applications. But from the present point of view, it is obvious that electronics has many applications. But in 1916, these were not anticipated even by the leaders of the field.

The same unpredictability of important applications that was true in 1916 is equally true at the present; and just as an example it applies very markedly to the case of lasers. Although applications of laser beams are now springing up all over, none of these were anticipated when Townes was doing the basic research in molecular microwave spectroscopy and below masers which led to the laser developments. The potentiality of practical applications is indeed an appropriate argument for supporting some particular fields of research, but for most basic research the primary criteria should be the promise of a proposal to help fill a gap in our knowledge, or to open new vistas in our understanding of nature. The more fundamental the discovery, the more difficult it is to predict the future applications, but the more likely it is eventually to have applications of great significance. In basic research, the important problems are best identified by the many individual scientists groping for new ideas, and not by a Government policy committee. As a result, basic research best prospers by the management procedure in which individual scientists make proposals which are then evaluated by those knowledgeable in the field rather than one in which a powerful central committee determines the basic research directions and assigns the projects to participating scientists.

In summary, the greatest needs to favor success in basic research are increased funding and protection from complicated administrative procedures and harmful project evaluation criteria.

Now, let us turn to the second portion of this topic raised in the chairman's letter: Success in the transfer of the results of basic research to commercial applications. I feel the present procedures have often been poor, and have unintentionally even discouraged rather than encouraged the applications. For basic research, the excitement of the discovery and the satisfaction of advancing human knowledge can be adequate rewards to induce a scientist to work hard and creatively. On the other hand, for practical applications the principal rewards are often economic; yet with present procedures, the immediate rewards to the creative scientist for pursuing a patentable commercial application are often negative rather than positive. The patent procedures for scientists engaged in Government supported research may be well designed to protect the Government from patent payments and to provide remunerative employment for patent lawyers, but they do not encourage a scientist to create or pursue a patentable idea. If a scientist in a Government-supported research project makes a patentable discovery, he must divert several days from his productive life to the preparation and revision of a patent application for the Government, with no significant personal reward—in fact, ordinarily, without even special compensation for the work done.

I am firmly convinced that the number and quality of Government research applications would markedly increase if the inventors had a greater chance of reaping a reward in some way proportional to the value of their inventions. To achieve this, two steps appear necessary. Unless the Government is prepared to exploit the invention itself, suitable arrangements for assignment of patent rights should be made to justify the expenditure of development costs by private enterprise, and the inventor should get some potential reward, such as a prompt award or a modest royalty, or preferably both. Since it is an empty right the Government retains if its inventions are not commercially developed, the value to the country of bringing them into commercial applications is of even greater importance than protecting the Government from being charged for patents developed under its sponsorship.

One of the ways in which basic research stimulates technology is that the creative people involved in the work place high demands on themselves and on others for the development of supporting technology that goes beyond the limits that are normally available. The field of high-energy physics, for example, in this way for many years has stimulated electronic and computer developments, and now is a major stimulus to the development of the superconducting technology so badly needed in many of the alternatives for alleviating the energy crisis. ERAD has recently recognized the importance of the process, and has established a promising new program which has the potential to support important technology initiatives of this nature. The program, however, is just getting started. We have not had a chance to see it work. I hope that we will.

Another means by which research-supporting agencies could stimulate commercial applications is by sponsoring studies of possible applications of the basic research and technology developed under their sponsorship. Gatherings of creative people, similar to the summer studies that have proved to be effective in many fields, could be effective both in identifying applications and in arousing the interests of the participating scientists and engineers in commercial applications.

II

Now, the second topic raised in the chairman's letter pertains to my views of the "Adequacy of the U.S. civil research and development effort, private as well as public." As I discussed in greater detail in the preceding and following sections, I am convinced that the United States civil research and development effort is inadequate. The inadequacy has become more marked in recent years with the failure of the financial support in most areas even to keep up with inflation. Since I have commented on part of this topic already, and will do so on the remainder later, to avoid repetition, I have given only a short response there.

III

Now, on item three raised in the chairman's letter: "Are sufficient resources being devoted to R. & D. in the private sector to sustain our once-recognized technological leadership?"

The answer here is clearly no, though the situation varies markedly from industry to industry not only as to the amounts of resources, but also as to the effectiveness with which they are utilized. The Bell Telephone System, including the Bell Telephone Laboratories, for many years has probably done the best of any company in effectively devoting private resources to research and development and in deriving therefrom great value both to the company and to the public. It is paradoxical that while Congress is attempting to devise better means for bringing about improvements in the transfer of the results of research to commercial development, the Department of Justice is attempting to split up the most effective combination that exists for accomplishing this objective. A few other industries, such as those concerned with computers, chemicals, electronics, aircraft, pharmaceuticals, petroleum, have been reasonably effective both in supporting relevant research and in bringing the results to bear on the commercial products. On the other hand, many—and even most—industries such as housing, coal products and the automotive manufacturing industry seem either to support little research or to be rather ineffective in incorporating the results of research into their products.

IV

Now, I shall turn to the next topic raised by the chairman's letter: "To what extent should Government funding be increased or redirected?"

The Government funding for research needs to be increased, both to make up for the amount it has fallen behind inflation in recent years, and to accommodate critical new problems, such as those of energy and the environment, which can only be alleviated with the aid of science and technology.

As emphasized earlier, great care must be taken in redirecting funds to avoid cutting back on important basic research, just because of the difficulty of predicting its practical applications. Particle physics at the high energy accelerators supported by ERDA is now revealing major new insights into nature, even though it is far too early to foresee practical applications. It would be a major national disaster if such work were curtailed because of the impossibility of predicting the future.

However, while urging continued and increased support of important and promising basic research, even when immediate practical application cannot be predicted, I also recommend that the supporting agencies should periodically reconsider and redirect their programs from the point of view of reducing the activities in those areas where progress is slow, and neither fundamental new discoveries nor practical applications appear promising, and with the intent of increasing support in those areas that give promise of either major basic discoveries or practical applications, or both.

V. COST SAVING INNOVATIONS

Since my responses to the earlier questions also apply to the remaining questions that were raised by the chairman, I would like to devote my remaining time to two topics not explicitly raised by the chairman.

One is means for encouraging cost-saving inventions and innovations on Government-contracted projects, and for avoiding the cost overruns that characterize many projects. As a private citizen, I am appalled by the unnecessarily high costs and the large cost overruns that characterize many large Government contracts. Unfortunately, the heavyhanded design habits developed under such contracts tend to carry over to the commercial products of the same organization. I am afraid that many Government contract policies unintentionally invite high costs and overruns by not giving sufficient incentives to reducing costs and spending less than the planned amount. If a project runs well under the planned cost, the contractor will probably see the saved money taken away abruptly to be added to the project of another contractor who has an overrun, even though the money could well be devoted to improvements on the first project. Likewise, long in advance of construction, the proposal for a new project is so extensively reviewed to confirm that the costs are not underestimated that the designs are usually made on an excessively conservative basis.

Once this amount of money is allocated, there is little incentive to reduce the costs. But it must be remembered that whereas an excessively conservative design does increase the final cost, it does not necessarily diminish the likelihood of a cost overrun. One of the best means for avoiding a cost overrun is a vigorous design and construction effort to achieve a large underrun, but the Government ordinarily provides few incentives for doing so. To diminish both the total costs and the dangers of overruns, strong incentives should be provided to encourage cost underruns either through financial rewards or through the use of the saved money to improve the quality of the project.

Chairman BENTSEN. What do you mean by heavyhanded design?

Mr. RAMSEY. Well, that means, to be safe, you make the device twice as heavy, twice as big, not so ingenious. You do not really push hard, and you use routine procedures instead of imaginative procedures in the design. By ingenuity, you can save a great deal of money. This is one thing I am very familiar with.

Let us say we have had the good fortune, in the Fermi Labs with which I have been associated—actually, not as director of the project; I had been president of the university's research association, and Mr. Robert Wilson is the director, a brillant man at bringing the project in well under cost; namely, it was planned for $250 million—he did it for $243.5 million. He also produced twice what was asked for, twice the energy called for. And this was all done by virtue of pushing very, very hard to come in at the lowest possible cost, and to get the maximum amount of physics out of it.

Chairman BENTSEN. The C–5A, with 26 wheels on the bottom, is supposed to be able to land on a dirtfield, and nobody thinks you can land the biggest airplane in the world on a dirtfield. They came in with the 747, when actually they should have built the C–5A, but there was no way thev could do it with that design.

Mr. RAMSEY. It was just overdesigned. It is one of the ways in which you most run up costs, and the problem is that there really is not so much incentive for coming in under once the amount that has been fixed. If you come in under the design, the contract is cut back, and in our case, we rather abruptly lost $6½ million, which was taken and transferred to another project which had a cost overrun. We could have happily spent that to improve the laboratory, but we lost it, and were thereby penalized for the economical design. What I think is needed, is more incentives to make it really advantageous to the individual group working to come in at lower costs than anticipated.

VI. INDIVIDUAL PRIVATE INVESTORS

My final comment of a fully volunteered nature pertains to the plight of private inventors, which I think is a very real problem. In the days of Benjamin Franklin, many inventions could be reduced to practice by a single man utilizing his own financial resources. This is not true of most of the inventions now needed by society. It is a tremendously expensive project to get many of these underway, and even the relatively minor ones that an individual inventor could work on are quite expensively developed. I believe, however, there could be some help to the individual inventor if it were made possible for him to find out at a very low cost, without having to hire lawyers, whether his idea had already been patiented. In other words, to have the Government provide, as a service to encourage invention, a low-cost search fee which will enable the man to tell at the beginning whether he has a patentable idea, because many inventors are discouraged right at this stage from doing anything about it.

For example, I have frequently had ideas for good inventions unrelated to my work. However, I deliberately have not pursued these ideas for the simple reasons that I could afford neither the time nor the money to do so; even if my time were not a consideration, the cost alone would have been an adequate deterrent, and I am sure the same is true for most potential individual inventors. A considerable investment is required to determine that the invention has not previously been patented, more must be spent to secure the patent, much more must be paid to develop the invention and demonstrate its practicality, and if the previous investments are to be recovered, a much larger expenditure is required to put the invention into production and distribution. Although there is no easy solution to all of these problems, it would be of great help if the first step were made easily accessible to the individual inventor; that is, if a quick and relatively inexpensive patent search were provided as a Government service to any inventor who wrote in directly without the necessity of his going through a patent attorney in this first step. Computers could probably be used effectively in providing such a service. If such an inexpensive patent search procedure were provided, I strongly suspect that many inventions that are now abandoned at the first stage would survive to commercial application.

Well, for my final remark, and my concluding comment—I would like, just briefly, to refer to the previously mentioned excerpt of a speech by Mr. J. J. Thomson made on behalf of a delegation from the Conjoint Board of Scentific Studies in 1916 to Lord Crewe, in defense of his work for developing the field of electronics. It seems strange that, at the present time, he would have to defend this, but he did have to, and he made the following statement:

> By research in pure science, I mean research made without any idea of application to industrial matters, but solely with the view of extending our knowledge of the laws of nature. I will give just one example of the utility of this kind of research, one that has been brought into great prominence by the war—I mean, the use of X-rays in surgery. Now, how was this method discovered? It was not the result of a research in applied science starting to find an improved method of locating bullet wounds. This might have led to improved probes, but we cannot imagine it leading to the discovery of X-rays. No, this method is due to an investigation in pure science, made with the object of discovering what is the nature of electricity. The experiments which led to this discovery seemed to be as remote from humanistic interest—to use a much misappropriated word—as anything that could well be imagined. The apparatus consisted of glass vessels from which the last drops of air had been sucked, and which emitted a weird greenish light when stimulated by formidable-looking instruments called induction coils. Nearby, perhaps, were great coils of wire and iron built up into electromagnets. I know well the impression it made on an average spectator, for I have been occupied in experiments of this kind nearly all my life, notwithstanding the advice, given in perfectly good faith by non-scientific visitors to the laboratory, to put that aside and spend my time on something useful.[1]

Incidentally, there is a quotation from J. J. Thomson's son, G. P. Thomson, who incidentally is a Nobel Prize winner for his own discoveries. G. P. Thomson said he had heard his father use another example; that "If Government laboratories"—and by that, he meant the laboratories used for only directed, clearly-defined objectives—"If Government laboratories had been operating in the Stone Age, we should have wonderful stone axes, but no one would have discovered metals".

Chairman Bentsen. That is a good statement, Doctor; and because of the limitations of time, and my concern that we might get a vote here to disrupt us in our attention to this, I would like to move right on and ask Mr. Shockley to present his statement.

STATEMENT OF WILLIAM SHOCKLEY, PROFESSOR, STANFORD UNIVERSITY, AND NOBEL PRIZE WINNER

Mr. Shockley. Senator Bentsen, this is an opportunity I appreciate very much for several reasons. One, it is my first opportunity to testify at a congressional hearing, and second, my preparation of

[1] From "J. J. Thomson and the Cavendish Laboratory in His Day," by G. P. Thomson (New York: Doubleday), 1965, pp. 167–168.

my testimony has led me into a research activity that I found very engrossing. I have also been impressed to note that several of the points that Professor Ramsey and Mr. Rabinow have emphasized were ones that I had independently selected as important.

In my preparation I attempted to sharply define my conclusions. With this purpose in mind, I formulated them as three recommendations to the subcommittee in the manuscript that I shall use as notes for my testimony. A fourth point, that I would have included in an extended draft, appears in my manuscript associated with the concept of the "law of excluded optimum" in government supported research.

My three points recommend certain topics for review by your subcommittee. For each of the three I have also stated by opinion of what such a review would establish.

My first point concerns patents and proposes a review that might shed light on Jack Rabinow's observations about decreasing American inventiveness. In my manuscript I state the subject for review as follows: What should be carried out is a review and inquiry into court records, including evaluation by qualified experts on invention, to determine if the significance of patents is being degraded by unsound court decisions.

My opinion of what will be found reads thus: My own opinion is that degradation of patents is occurring and that new legal machinery will be necessary to forestall adverse effects on invention and innovation. This opinion, is I believe, in keeping with themes of the two previous speakers.

My second point continues in the vein of what Professor Ramsey had to say, particularly as he emphasized the role of Bell Laboratories. I recommend a review of large and well integrated establishments, not restricted to Bell Laboratories, including participation by a qualified research director and possibly administered by the Industrial Research Institute, which does studies in this area, of the role played in promoting economic growth through innovation by large integrated industrial research and development establishments.

My opinion is that this review will clearly establish that some unsound antimonopoly proposals, for example the separation of Bell Laboratories from other parts of the Bell System, will inhibit economic growth.

I suddenly realize that I neglected to state my disclaimers as I should have done at the very beginning of my remarks. My disclaimers are similar to Mr. Rabinow's. I am not speaking for the Bell System, nor for Stanford University.

My third point calls for an evaluation of the recommendation of the board of directors of the IEEE (Institute of Electrical and Electronic Engineers) that national policy be established to devote 3 percent of the GNP to R. & D.

My opinion is that such a policy should be established.

Chairman BENTSEN. What percentage is it now?

Mr. SHOCKLEY. I shall refer to chart 1 of my prepared statement—copies have been made available to all my listeners—which I obtained by telephone request to my old friend, Don Fink, who used to be general manager of the IEEE headquarters and now is executive consultant. The figures are surprising. In answer to your question, if you will look on this chart at the top dashed line, you will see total national expenditures for R. & D. expressed in constant value 1967 dollars. Note that this has decreased by about 10 percent from more than about $23 billion in 1969 to less than $ 22 billion in 1975.

A telling observation noted in the table of chart 1 in my prepared statement is that when inflation is discounted, government supported R. & D. has decreased at 3 percent per year, on the average, from 1967 to 1975. This decrease in Federal support more than offsets the small increase of 1.8 percent per year in non-Federal so that, as is reported in the IEEE position paper approved by the board of directors that I am submitting as an exhibit, the total for R. & D. has fallen from 3 percent of the GNP in 1964 to less than an estimated 2.4 percent in 1975.

With respect to innovation and international trade, I shall next read a paragraph from the IEEE document:

> The steady upward trend of economic indicators from 1950 to 1970 was paced by new technical ideas and methods leading to such new products as the digital computer and the myriad of other devices based on the ubiquitous transistor. Products like these, the so-called technology-intensive manufactured products, classified by the Department of Commerce, have played a vital role in America's balance of foreign trade. Since 1951, such products have consistently generated a favorable balance of trade of from $5 billion to $10 billion. Within any other commodity group, today, with agricultural products, they provide the only—and this was underlined in the IEEE statement—favorable trade balance.

I have read this paragraph since it is a key item in support of the focus of my testimony on the three recommendations for reviews by your subcommittee. This paragraph also serves to introduce my next topic—the impact of technological advances on economic growth. I shall also use facts about chart 3 of my prepared statement to illustrate the role of motivational factors in stimulating innovation.

Chart 3 shows that over a period of 50 years there has been a continual, very uniform increase in the hourly earnings of Western Electric Manufacture Employees. Overall Labor Department figures for hourly workers look almost the same. In contrast, the chart shows a rapid and continuous decrease in the cost of a long distance telephone call, specifically a 3-minute, station-to-station call from San Francisco to New York. I have compared wages and telephone rates, to obtain what is called "real wages" per work hour—a meaningful measure of economic growth. This growth reflects what the individual would feel—that is, what he can buy per hour of work. That "real wages in calls-per-hour-of-work" index has, during the 50 years shown on chart 3, increased about 75-fold.

For example, a worker would have had to work a whole week in 1919 to pay for one transcontinental phone call. But 50 years later, he could buy three such calls with his pay for 1 hour of work.

There can be no doubt that the phenomenal economic growth of telephone service was the consequence of technological innovation and thus based on R. & D. investments.

Now, at the risk of seeming to be a Bell System public relations man, I shall quote some additional numbers from my manuscript that point out that, compared to other countries, and to other industries in our country, our telephone system is outstanding.

Two hours of labor in the United States will buy you a telephone for a month. Other figures are: 5 hours in Germany, 5 hours in

Italy, a little over 6 in the United Kingdom, 14 to 15 hours in Japan and France.

I also compared telephones with other United States items by using the data from a New York Times article. This article did not give hourly wakes but did give figures for median family income. The Times' point was that inflation was not all that bad—and the Times' best example of where it was not hurting was the telephone. Telephone costs dropped 34 percent during the 26 years from 1948 to 1974 while at the same time family income increased 298 percent. This means real family income for phone service had increased at a compound interest rate averaging 7.2 percent per year—a value consistent with the somewhat higher one obtained for the 50 years on my chart 3.

A pound of chicken came closest to the telephone's success in beating inflation; I believe that technology was important for this. The "real family income" increased in purchasing power for chicken at an average rate of 5.8 percent per year.

The relatively negligible progress in the automotive industry is reflected by the fact that a median family income remained substantially constant in purchasing power for a family sized Chevrolet. It increased an average rate of only 0.7 percent per year.

To me these observations illustrate the impact of R. & D. based technological innovation on economic growth.

Chairman BENTSEN. Let me ask you this. Those are impressive figures. Having looked at so many of those, when you talk about real family income, that is colored a little by the fact that you see a lot more women working and young people than we have in years past.

Mr. SHOCKLEY. Well, the family income, you see, increased by practically a factor of four in real dollars during the 26 years. Even if you allowed for double the number of work hours per week for the family, it would not change the percentage value for the rate of growth much.

Chairman BENTSEN. That is correct. I am not arguing that there is not a substantial increase. But I do say that there is something else behind that particular number in the way of more members of a family working for wages than previously.

Mr. SHOCKLEY. Senator Bentsen, one thing of which I became convinced in looking at this subject is that it is an operational-researchable area. I entirely agree that there is a lot more to be said, and I have probably said too much on the 14 sheets of manuscript. I am working hard to drive home thoughts of primary significance in the limited length of time I have.

The area I shall discuss next concerns motivation in general, the motivation to file patents, and so on. I am tempted to discuss my own research on the statistics on creativity and individual differences between research workers that I think are very relevant and relate to the importance of the quality of the people who are active in innovative organizations. But these topics are not as close to any of my three recommendations as the topic of patents that I shall discuss next. My point on the important motivational aspect of patents can be summed up in an acronym that I invented while preparing this testimony. The new word is IBLER. It sums up the key thought behind the "Eighth Power of Congress." I came first to appreciate what lies behind patent law only 2 years ago after Jack Rabinow had called my attention to it.

Before that time, I had not realized that plans for patents were put into the Constitution by our country's Founding Fathers. To me, the phrasing of this power expresses deep wisdom about the basic nature of man, a wisdom which the Founding Fathers grasped and utilized for the benefit of the Nation. I regard this realistic utilization of man's capacities and limitations as an outstanding example of what I think of as the essence of the conservative viewpoint. Here is how the Constitution reads on this point:

> The Congress shall have the power . . . to promote the progress of science and useful arts by securing for limited times to authors and inventors the exclusive rights to their respective writings and discoveries.

The phrase that I coined to encapsulate what I believe to be the essential wisdom of this power of Congress is "incentive based on limited exclusive rights"—giving my acromyn IBLER. I believe that this concept, in various forms, has been a theme in the remarks of the previous speakers today. I consider that the concept of appealing to the selfish motivations of some—but no means all—inventive individuals in exchange for their making their inventions public through issued patents, with resultant benefit to society, is a wonderful compromise of emphasis between altruism and cynicism. It gives me great respect for the human wisdom of our Founding Fathers.

I shall now turn to a related type of profit-motivated incentive on an organizational, as much as on an individual, scale. My example is related closely to what the IEEE position paper identified as an 'autstanding example of technology-intensive activity. The theme of my discussion is expressed by the title of an article in the June 1974 issue of Fortune entitled "California's Great Breeding Ground for Industry"—an article honoring the contributions made by Stanford's Professor Terman in building up a technologically intensive business community around Stanford.

In 1955, I attempted to start a transistor business in California. One of my motivations was that I had come to the conclusion that the most creative people were not adequately rewarded as employees in industry. My conclusion was based on my operations-research findings, later published, and included, in part, in exhibits that supplement my testimony. One of my conclusions is that the creativity of individuals can be increased by making them more comfortable about living with their failures and, indeed, using these as stepping stones to progress. I emphasize this point in my article, in one of my exhibits, entitled "The Invention of the Transistor, An Example of Creative-Failure-Methodology." Creative failure is perhaps a most appropriate description for my business experiences in California as will become clear from these quotations from the article in Fortune:

> In the 1950's, Shockley gathered around him a large group of gifted young electronics specialists whom he picked from big companies and universities around the country.
>
> In 1957, however, his operation ran into trouble when eight of those bright young men, including 29-year-old Robert N. Noyce, left and with the backing of Fairchild Camera and Instrument Corporation, founded Fairchild Semiconductors in Palo Alto.

Fortune says parenthetically:

> Shockley subsequently went on to other things, becoming the center of controversy because of his view that intelligence is inherited and that genetics more than environment, accounts for the low scores by blacks on I.Q. tests.

Closing the parenthesis, Fortune continues:

"Fairchild became one of those amazing corporate seedbeds. No fewer than 38 companies, including Noyce's enormously successful Intel Corp., have been started by Fairchild employees. They in turn, have turned part of Santa Clara county into Silicon Valley, the world capital of semiconductor technolgy, with sales of about $2.5 billion. The valley accounts for about 8 percent of the total U.S. sales in electronics and some 40 percent of the Nation's semiconductor output.

This conclusion of my quotation from Fortune shows that "silicon valley" is outstanding as an example of the production of the technology-intensive items that are so important, as the IEEE position statement notes, for America's balance of foreign trade. The history of "silicon valley" is thus highly relevant to this subcommittee's stated intent to examine "the extent to which the United States industries are maintaining their positions relative to foreign competitors in important fields of technology development."

Accordingly, I propose that the "silicon valley" case is an example very relevant to the interests of the subcommittee, and thus perhaps worth study in terms of motivational incentives like those that I have defined with the IBLER acronym. The 1974 Fortune article lacks the perspective that throws light on motivation in successful entrepreneurship contained in another of my exhibits, a three-articles series entitled "Silicon Valley, U.S.A." by Don C. Hoefler in Electronic News for January 1970.

I do not recall meeting Mr. Hoefler who described me thus:

Despite an uncanny genius for spotting and recruiting talent (hero worship, admitted one of his early employees) he was less adroit in managing that talent.

Hoefler does report person-to-person discussions with key figures of his story. What I wish to emphasize in the Hoefler articles is the role that he describes of profit motivation among the individuals who have made silicon valley so important a contributor to our Nation's economic growth.

I do not take a position on the accuracy of his specific reports of seemingly dubious ethical standards of the participants. Even as fables, his articles are worthy of consideration as guidance for planning legislation which may influence innovation.

I see the pattern described by Hoefler as an extension of the process that I emphasized in discussing IBLER—incentives based on limited exclusive rights. IBLER is one facet of our motivation system that can harness selfish instincts, as well as altruistic ones, so as to create a social system that is far more noble in its contributions to human welfare than the sum of the motivations of the individual members.

The effectiveness of the IBLER motivation is threatened if I am sound in my opinion about my first recommendation that this subcommittee should promote a review of court decisions on patent infringement suits. To support my opinion, I shall review the chance events that informed me of the threat to the significance of patents. The key incident involved a Berkeley California patent attorney. As a director of the Alameda County Lawyers Club, he invited me to address a club meeting on my genetic interests that I quoted from Fortune. To my complaints that my sincerity on these matters had been publicly impugned, he arranged for me to take a polygraph test at the club's expense—which I did and passed.

Incidentally, I believe that the polygraph is a neglected tool in government. Indeed, I proposed, when accepting William F. Buckley's invitation to appear on his TV program, to do so attached to a polygraph; furthermore, I suggested, that if polygraphs accompanied political speeches, it might revolutionize our political system.

To return to the IBLER issue, in the course of my discussions with the patent attorney, he told me that his firm felt that the significance of patents was being degraded. As a consequence, he was shifting his emphasis toward business law. His evaluation meant to me that our Nation was losing the effect of the incentives based on limited exclusive rights—IBLER was being weakened. I was disturbed. I had come to revere IBLER as a precious legacy from the wisdom of our Founding Fathers—a realistic wisdom founded on understanding the basic nature of man.

Accordingly, when the opportunity to present this testimony arose, I pursued in detail my patent friend's conclusions. He told me that infringement of the patent for one large company's invention selling for more than a half a million dollars had clearly occurred but that the suit was lost when the Federal judge found the patent invalid.

In the other case, the three-man company selling a hardware store product had lost its business because its market was flooded by products that infringed on its issued patent. However, a remedy by lawsuit was not considered worthwhile by my attorney friend's firm because of the 10-to-1 estimate that the issued patent would be found invalid.

My manuscript summarizes this situation in the following paragraph:

I say that if these instances are representative, then the resulting disillusionment about the value of patents will inevitably discourage the utilization of one of our Nation's greatest resources: the intelligent initiative of inventive citizens.

I believe that establishing what the facts are about the seeming degradation of patents in court is a highly appropriate activity for a congressional committee on economic growth.

In my further inquiries, I asked several qualified individuals whether the importance of patents was indeed being degraded. Some of them were no more alert to this possibility than I had been, but those who were aware, expressed deep concern.

My inquiries at Stanford, revealed a paper presented by the University's director of technology licensing. The gist of his position is summarized by the title of an article about his paper: "Nader, Federal Officials Stymie Technological Innovation."

I discussed my concerns about incentives and the IBLER aspects with Lewis Branscomb and Bruce Hannay, leaders of the research organizations of IBM and Bell Laboratories. A key conclusion from those conversations is that degradation of patents must unintentionally have the consequence of enhancing secrecy in industry, an effect precisely opposite to the intent of the Constitution in giving Congress the power of patents to promote the progress of science and the useful arts through limited exclusive rights.

Such inquiries led me to suggest in my recommendations the organized explorations with participation of highly qualified experts such as Branscomb and Hannay. One usable organized vehicle for this purpose is the IRI, or Industrial Research Institute of New York City. I learned about IRI from Hannay, who was its last year's president. IRI has seriously reviewed closely related matters. I read a draft of this paragraph of my testimony to Branscomb and to Hannay and have verified their concern about these matters and their willingness to do what they practically can to contribute to the wisdom of any related legislation that might develop because of the activities of the Subcommittee on Economic Growth.

Chairman BENTSEN. We have just a few minutes left and I would like to pursue a few questions.

Your prepared statement will be included in the hearing record. Thank you.

[The prepared statement of Mr. Shockley follows:]

PREPARED STATEMENT OF WILLIAM SHOCKLEY

Mr. Chairman and members of the Subcommittee, I appreciate the opportunity and the honor of being here to express thoughts that I hope may contribute to the future welfare of our nation. This occasion has led me to establish communication with several formerly close associates, and I have found this a rewarding experience and have incorporated a number of their ideas into my testimony.

My research and thinking since the invitation to testify has led me to recommend three specific review activities for the consideration of the Subcommittee. The proposed reviews relate to the conditions of funding and government policy, topics emphasized in the letter of invitation to me. The first review recommendation focuses on the federal courts as a relevant aspect of the environment that can significantly influence the success of attempts to stimulate invention and hence national leadership in commercial development—again topics mentioned in the letter. I shall follow each of my three recommendations with a statement of my own opinion of what an objective review will conclude. The bulk of my testimony will consist of a discussion of the considerations that led me to the topics proposed for review and my opinions about them. My review recommendations and by opinions are as follows:

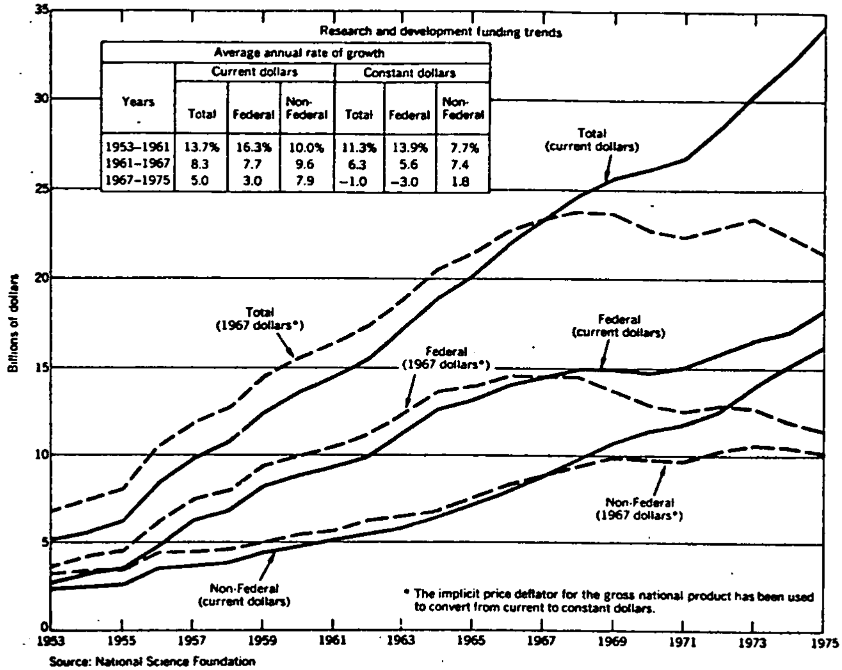
(1) A review of and an inquiry into court records including evaluation by qualified experts on invention to determine if the significance of patents is being degraded by unsound court decisions. My own opinion is that degradation is occurring and that new legal machinery will be necessary to forestall adverse effects on invention and innovation.

(2) A review (including participation by qualified research directors and possibly administered by the Industrial Research Institute) of the role played in promoting economic growth through innovation by large, integrated, industrial research and development establishments. My opinion is that this will clearly establish that some unsound anti-monopoly proposals, for example the separation of Bell Laboratories from other parts of the Bell system, will inhibit economic growth.

(3) An evaluation of the recommendation of the Board of Directors of the IEEE (Institute of Electrical and Electronic Engineers) that a national policy be established to devote 3% of the GNP to R&D. My opinion is that such a policy should be established.

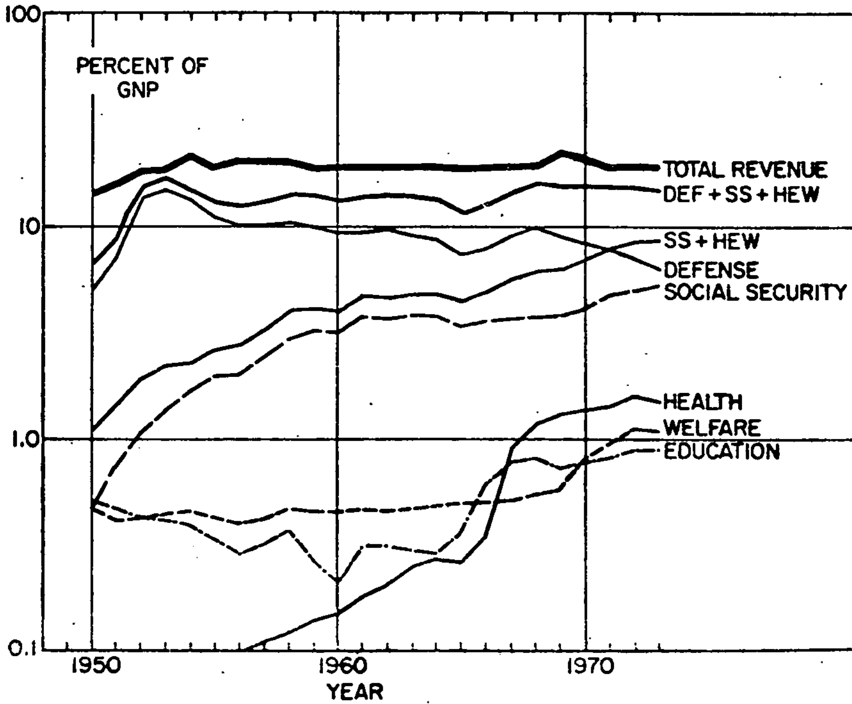
The third recommendation bears specifically upon the first two questions in the letter of invitation that ask about the level of funding in the private sector and the role of government funding. I came to it as a direct consequence of my making an inquiry to my World War II Pentagon office-mate, Mr. Donald G. Fink, formerly General Manager and now Executive Consultant of the IEEE. One of the exhibits with my written statement that supplements my testimony is a position paper approved by the Board of Directors of the IEEE (Institute of Electrical and Electronics Engineers). This board consists of the elected and appointed directors and headquarters staff of that organization. This position paper has been circulated since 1 May 1975 to the White House and several committees of Congress. From it I shall quote a paragraph that underlines a principal theme of my testimony.

CHART 1



Years	Average annual rate of growth					
	Current dollars			Constant dollars		
	Total	Federal	Non-Federal	Total	Federal	Non-Federal
1953–1961	13.7%	16.3%	10.0%	11.3%	13.9%	7.7%
1961–1967	8.3	7.7	9.6	6.3	5.6	7.4
1967–1975	5.0	3.0	7.9	−1.0	−3.0	1.8

CHART 2



"The steady upwards trend of economic indicators from 1950 to 1970 was paced by new technical ideas and methods, leading to such new products as the digital computer, and the myriad of other devices based on the ubiquitous transistor. Products like these, the so-called technology-intensive manufactured products classified by the Department of Commerce, have played a vital role in America's balance of foreign trade. Since 1951 such products have consistently generated a favorable balance of trade of from $5 to $10 billion, more than any other commodity group. Today, with agricultural products, they provide the *only* favorable trade balance."

This quotation focuses attention on the transistor area which I had selected as the central topic for my discussion before seeing the IEEE position paper.

In respect to the Subcommittee's question about private and government funding for R&D, I call your attention to one of the IEEE paper's charts that stresses that when inflation is discounted, government supported R&D has decreased at 3.0% per year on the average from 1967 to 1975. This decrease more than offset the increase of non-federal support and caused a net decrease from 1973 to 1975 of 10% in the overall national R&D effort expressed as a fraction of the GNP. The paper urges that a national policy be established to stabilize the national R&D effort at no less than 3% of the GNP. I consider this paper to present well formulated observations worthy of further consideration.

I found a chart prepared by Dr. Bernard Oliver, who heads research at Hewlett-Packard, very informative, and with his permission modified it as presented here. It shows that, as a percentage of the GNP, the total government revenue has remained nearly constant for two decades and has been largely expended on two broad categories, one being National Defense and the other being those items associated with Health, Education and Welfare and with Social Security.

I shall next use dollar costs as a tool for measuring technological progress and relate this to economic growth, an analysis in keeping with the subject of discussion at this hearing.

ECONOMIC PERSPECTIVE ON ELECTRONIC GROWTH

My next chart is closely related to the observations of the IEEE position paper about technology-intensive products. However, it is not concerned with products to be sold per se but instead with services made possible by the same technology—namely telephone service. I prepared this chart while I was still involved in 1963 with the transistor business venture that I shall refer to below in discussing innovative motivations. It was printed as "Scientific Thinking and Problems of Growth" in a University of California publication about a conference entitled The Impact of Science: California and the Challenge of Growth. My paper is one of the exhibits with my written statement.

I have introduced this chart because it dramatizes the continuity of the technological advancement achieved in the reduced cost of long distance telephone calls by the integrated R&D facilities of the Bell System. It is one of the items of evidence for my second recommendation. The progress is presented here in terms of the economic concept of "real wages" defined as the purchasing power of the earnings for one hour of work. In the chart "real wages" is measured by the number (or fractions) of transcontinental telephone calls that can be purchased with the earnings for one hour of work by a Western Electric "hourly-rated" worker. This chart shows a relatively uniform increase in wages at 4.3% per annum from $0.502 per hour in 1919 to $2.58 in 1961. Long-distance, transcontinental telephone rates fell, less regularly, at a rate of 5.0% per year, for the average represented by the straight line on the chart. The result is that a worker who could buy only about one such call for a week's work in 1919 could in 1963 buy 1.5 such calls with the earnings for one hour of work—meaning 75 times as much purchasing power, or "real wages", for this item.

I have obtained data that extends the chart to 1974. The earnings fall accurately along the line but the dcrease in rates has been slightly more gradual. However, new technological advances, now in the making, involving replacing copper wires with fiber optics with enormously increased message handling capacity, have, I believe, the potential to bring the costs down again to the line that I drew from 1919 to 1963.

At the risk of seeming to be a Bell public relations person, I shall expand on this last point by comparing Bell System costs with those of other countries and with other American industries. The hours of work required to pay monthly telephone charges are stated by Bell Labs to be as follows: 2.17 hours in U.S.A.; 5.67 in Germany; 5.75 in Italy; 6.33 in U.K.; 14.58 in Japan; 14.9 in France.

Data that permit ready comparisons between the technology-intensive telephone costs and other industries were published in THE NEW YORK TIMES. THE TIMES reviewed inflation and family income and quoted changing prices from 1948 to 1974. Median family income increased 3.985 times and long-distance phone calls (New York to Topeka, Kansas) decreased by 34.2%. This means that the identical portion of family income that could purchase one phone call 26 years ago can now purchase 6.60 calls, a 560% increase in "real family income" measure in phone calls. This corresponds to an average rate of increase of 7.2% per year. The next best item was a pound of chicken; for which there has also been significant technological progress; for it the average increase was 5.8% per year. The relatively negligible progress in the automotive industry is reflected by the fact that purchasing power for a family size Chevrolet remained substantially constant; it increased at an average rate of only 0.7% per year. On the other hand, hospital costs per in-patient day increased 777.8% corresponding to a rate of loss of purchasing power of 3.0% per year.

In respect to the Subcommittee's query about government policy and my response in my second recommendation, I wish to emphasize two aspects of these observations on changes in real wages and family purchasing power: First, in keeping with the point quoted from the IEEE position paper, the high technology component of telephone service has permitted the enormous economic progress; and second, there has been a remarkable continuity in progress over half a century in the economies made possible by the highly diversified and centralized laboratory establishment maintained by the Bell System.

CHART 3

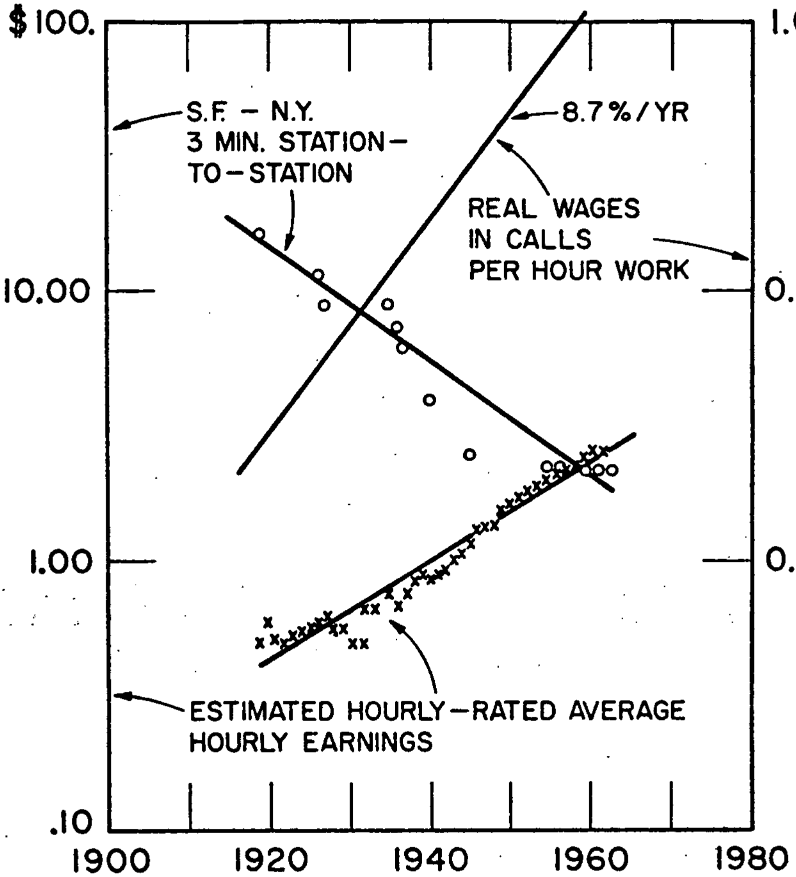


Figure 4—Growth of real wages in terms of long-distance telephone calls.

• I am indebted to Gordon N. Thayer, Vice President of American Telephone and Telegraph, for furnishing me the data shown in Figure 4. To the best of my knowledge, these data have not previously been published, certainly not in the form presented here.

Bell Laboratories is but one of several examples of highly developed, large-scale R&D establishments. The Industrial Research Institute, about which I shall say more when I discuss the possible degradation of patents, has found that a little more than half of all the industrial funds are spent by only 20 companies.

As an example of technological invention and innovation that contributed to the economic growth that is the central topic of this hearing, I shall discuss the "transistor story" at Bell Laboratories.

THE TRANSISTOR STORY: THE WILL TO THINK AND THE OPPORTUNITY TO ACT

I carried out extensive research in 1972 on the details of events during the few months at the end of 1947 and early 1948 when the earliest and essential transistor inventions were made. These illustrate specifics of the factors that are important in motivating creative individuals to develop the new thoughts essential for the technological progress that causes the economic growth that is the subject of special interest of this Subcommittee. My research was carried out in preparation for lectures at celebrations of the transistor's twenty-fifth birthday. The details are in my exhibit entitled "The Invention of the Transistor—An Example of Creative-Failure Methodology". That exhibit discusses the failure of my field-effect transistor proposals of 1939 and again, independently, of 1945. The team that I supervised emphasized research on the new physics of surfaces proposed by John Bardeen in order to explain the failure of my field-effect proposals. During that time, we put aside efforts to make a transistor.

But the situation changed on 17 Nov 47. Then Walter Brattain, who with Bardeen and me won the 1956 Nobel Prize in physics for our transistor contributions, did an experiment suggested that day by another member of the team. The result was evidence that the field-effect might be made to work. This put all the motivating factors into mesh.

The effect is shown by the increased tempt of Bardeen, Bratain, and me in a chart reprinted here from the exhibit. My research on records shows that, during the five months after the breakthrough experiment, each of us filled nearly ten times as many pages of is laboratory notebook with observations closely related to patentable ideas as he had used in the preceding five months. The point-contact transistor of Bardeen and Brattain was born on 16 Dec 47, less than one month after 17 Nov 47. I invented the junction transistor about one month later. However, the junction transistor was not realized in a convincing form until early 1951. The stimulus of possible application in military uses was important in achieving its existence as early as that date.

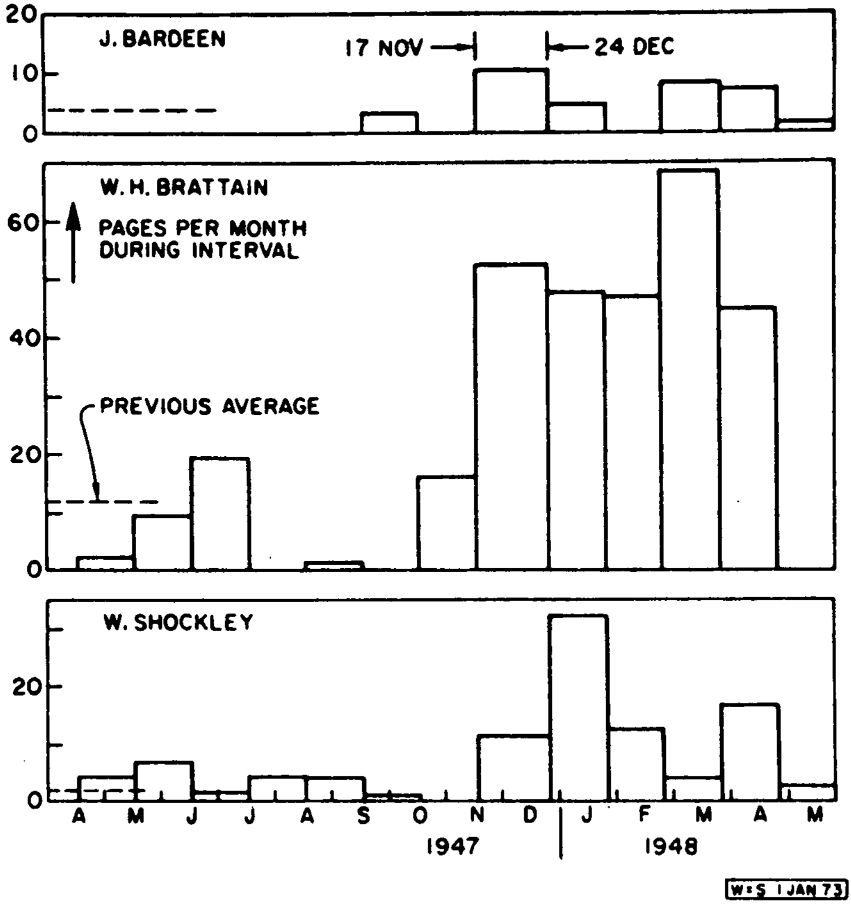
There were many motivating factors: All of us realized—somewhat vaguely, perhaps—that a semiconductor amplifier would be of great value to our employer, the Bell System, and, indeed, to society in general. Furthermore, we all, I believe, had confidence in the management of the Laboratories that we would be justly recognized for our contributions. Thus, we had a full head of steam—so to speak—and when the experiment of 17 Nov 47 occurred, we had the "will to think"—to think hard and creatively—about how to exploit the new possibility. This is indicated by another chart that shows details of the five transistor patent applications that were filed prior to the public announcement of success at the end of June 1948. I have borrowed the phrase "will to think" from a conversation of 1940 with Enrico Rermi in which he used it to describe his own reaction to the knowledge that government support would be forthcoming for his experiments related to atomic energy.

When I talked to Fermi, James Fisk and I had independently invented the "segregation" or "lumping' principle that permits nuclear power plants to operate without isotope enrichment of the uranium. I tried to interest Bell Laboratories in setting up a trial unit. I was disappointed that they did not do so. However, I accepted what I even then appreciated was a sound policy decision. Our findings were available to the National Academy of Sciences which had requested the investigation that led to our invention.

A main objective that I have in mind in presenting this testimony about the transistor story is, as I stated earlier, to support my opinion in my second recommendation that large, diversified industrial laboratories do play a natural and important role in innovation.

The merits of the wide variety of facilities and the diversified skills of the staff played an important role in my participation at Bell Laboratories in 1966 after I had acquired an interest in the possibility of using the propagation of magnetic domains to simulate neutral effects with possible thinking or computing applications. I found that related device development programs were well under way. These led to what is now known as the magnetic bubble devices which may replace disk files, an important component in computer memories. In the context of this testimony, especially as it relates to my second recommendation, I recall several exciting instances in which I was able to contribute to the program so as to become a co-inventor of at least one important patent. An essential feature of one of my contributions was the fact that I could draw individuals with highly diversified special knowledge into a conference. This led to the first consideration of really promising crystals with desirable properties. In order to present in detail the need for having such diversified activities under one roof, I asked for and received a document that is one of the exhibits of my written statement. This document, sent to me by Dr. Derek Scovill, a co-inventor with me of one of the patents, describes how extensive were the interactions among different groups of specialists that occurred during the creation of what appears to be an important innovation in storing and processing digital information in ways that have special adavntages, some of these being particularly important for telephone applications.

CHART 4



The influence of "the will to think," indicated on 17 November 1947, upon the rate at which notebook entries were made related to the patent of table I.

(Areas represent total pages and ordinates represent rates. For Brattain's notebook the period of 25 April to 25 May 1948 was not comparable to earlier dates because he shared additional new notebooks with technical assistants.)

CHART 5

THE 5 TRANSISTOR DEVICE PATENTS FILED BEFORE THE PUBLIC ANNOUNCEMENT ON JUNE 30, 1947

No.	Patent dates		Inventors[2]	Invention[3]	Conception	Reduction to practice	Div. CIP
	Filed	Issued[1]					
1	Feb. 26, 1948	Oct. 3, 1950	W. H. B., R. B. G.	Electrolyte FET	Nov. 20, 1947	Nov. 21, 1947	0
2	do	do	J. B.	Inver. layer IGFET	do	(?)	0
3	do	July 17, 1951	R. B. G.	Elect-form inver. layer	December 1947 (?)	December 1947 (?)	0
4	June 17, 1948[4]	Oct. 3, 1950	J. B., W. H. B.	Point-contact transistor	Dec. 15, 1947	Dec. 23, 1947	[5] 3
5	June 26, 1948	Sept. 25, 1951	W. S.	Junction transistor	Jan. 23, 1948[1]	April 1950[6]	[7] 3

[1] Patent numbers: 2,524,034; 2,524,033; 2,560,792; 2,524,035; 2,569,347.

[2] John Bardeen, Walter Houser Brattain, Robert Bernard Gibney, William Bradford Shockley.

[3] Modern terminology is used: IGFET—insulated-gate, field-effect transistor (1) is inversion-layer channel FET with electrolyte gate. (2) is IGFET with inversion layer channel. (3) is electrolytic processing to form inversion layer. (4) and (5) are the basic point contact and junction transistor patents.

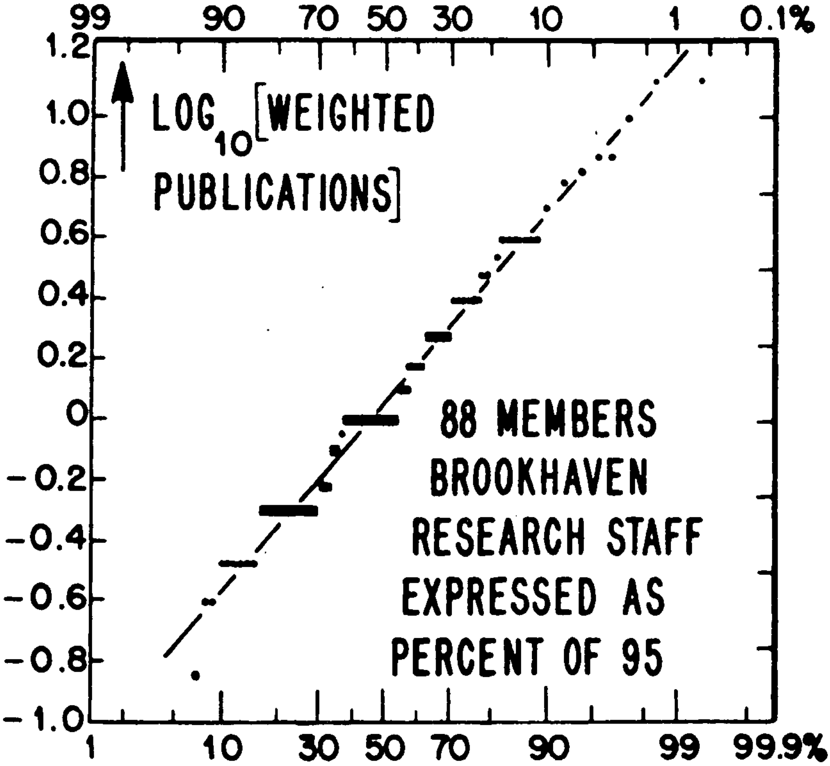
[4] Originally filed Feb. 26, 1948; abandoned and refiled to include current gain at collector.

[5] Includes "divisions" and "continuations in part" of these patent applications.

[6] The dates are late November 1947 for the p-n unction with drop of electrolyte, see discussion of claim 29 under Dec. 4, 1947 in the "Magic Month," subsec. III A.

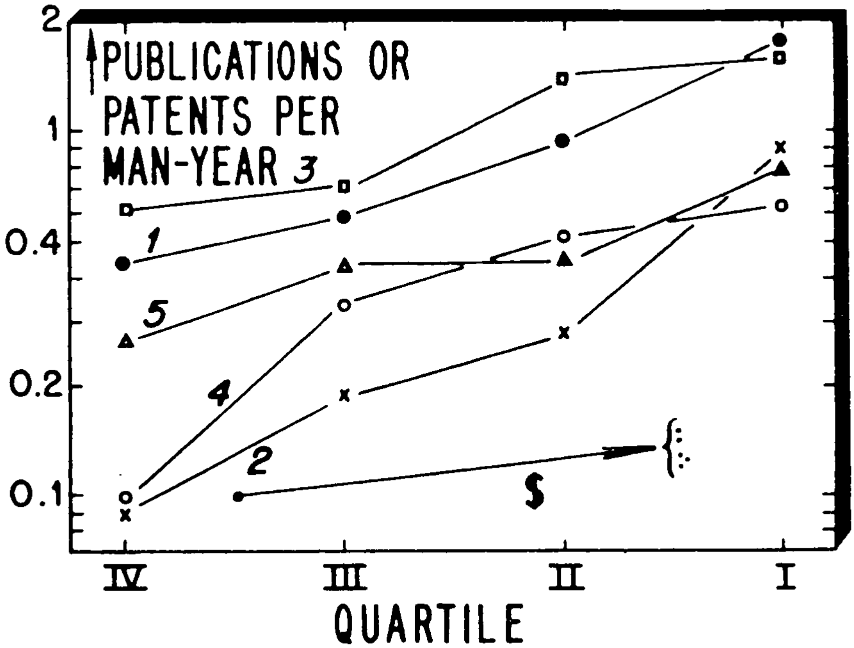
[7] 1 CIP on negative resistance from transit time, conceived on Jan. 24, 1948, had in turn 2 additional CIP's.

CHART 6



Cumulative distribution of logarithm of "weighted" rate of publication at Brookhaven National Lab. plotted on probability paper.

CHART 7



Relationship between productivity and quartile number and salary and quartile number for several laboratories.

To add prespective to some of these observations, I note that the personal factors that influence an individual s accomplishments in the invention and innovation may be classified in terms of basic ability (the genetic endowment of the individual) and his environment. I shall distinguish between two environmental factors: emotional and physical. All of these interact to produce motivation. At Bell Laboratories on 17 Nov 1947 an outstanding combination of positive values for all of these components occurred. The transistor quickly followed. I shall next turn to another aspect of the multiplicity of factors that contribute to productivity. It is that when a result is the consequence of many independent causes, then a statistical distribution of the so-called normal type is to be expected. This is relevant to my next observations.

INDIVIDUAL DIFFERENCES IN CREATIVITY

At about the same time that the transistor was being created, I undertook a study of the statistics of creativity of the presonnel at Bell Laboratories as related to recruiting policies. I verified that some of the candidates who made the best impressions during interviews were turning down our offers and taking positions elsewhere. I tried to answer this question: Would it have paid off to have offered them higher salaries? I returned to this research when invited to give a lecture for the Operations Research Society (of which, I recall, I was a charter member who failed to keep up his dues). My study was published (March 1967 in the PROCEEDINGS OF THE I. R. E. (Institute of Radio Engineers, later a part of the IEEE). The essential results are included in the exhibit of the IMPACT OF SCIENCE.

My studies confirmed earlier findings by others: There were enormous differences between one individual and another in the quality of his contributions to new knowledge or to inventions. One of the charts of my exhibit shows (by using what is called "probability paper") that at Brookhaven National Laboratory an individual at the bottom of the most highly productive quartile for scientific publications published 3.2 times, or 220%, more scientific publications than did a median member of the staff. A person at the top of the bottom 25% published 3.2 times less, or only 31.25 as much as the median person. A distribution with these characteristics (and related ones) is called a "log-normal" distribution. I discovered from my research that this general law applied to each of several industrial laboratories (I obtained the data by agreeing not to reveal the sources) and also Brookhaven National Laboratory and a division of the National Bureau of Standards. I also found that the same law applied to patents just as it did for scientific publications. For the Physical Research Department at Bell Laboratories, I found a factor of 2.1 for scientific publications between median and top quartile compared to 4.0 for patents—inventions were contributed by a small fraction of the population than was new science. For other cases, ratios of about the same size were also obtained—more than 2 and less than 4 in general.

I suggested a theory of a mechanism involving many independent causes that could lead to the observed log-normal distribution. This was published in the I. R. E. reference that I have mentioned.

I also compared the differences in research productivity with differences in salary as shown on another chart. This comparison indicated that in a typical industrial organization to obtain a 10% increase in salary, one would have to increase his output by, perhaps, 30 to 50%. In other words, making high salary offers might be very profitable. I also concluded that very productive people were underpaid.

These were some of the thoughts and motivations that led me in 1955 to seek financial backing to try to start a new semiconductor company. The result may be regarded as an extreme, perhaps unique, example of creative-failure methodology—especially for those who later created "Silicon Valley" in California, called "the world capital of semiconductor technology" by FORTUNE magazine—the next topic of my testimony.

MOTIVATIONS FOR INVENTION, INNOVATION AND ENTREPRENEURSHIP

As my exhibitor on "The Invention of the Transistor" emphasizes, I regard the constitutional power of Congress over patents to be a key factor in motivating invention. The Constitution states:

"The Congress shall have the power to promote the progress of science and the useful arts, by security for limited times to authors and inventors the exclusive rights to their respective writings and discoveries."

My experiences and observations, especially as related to the invention and development of the transistor and the growth of Silicon Valley, as I shall explain, have impressed me with the wisdom of this power in providing incentives based on limited exclusive rights. Guaranteeing limited exclusve rights transmutes what might otherwise be socially destructive, selfish instincts so that they "promote the progress of science and the useful arts." I hold this to be such a significant concept in respect to economic growth and the free enterprise system that I shall encapsulate it in the acronym IBLER—incentives based on limited exclusive rights.

My focus on the IBLER concept is consistent with the most important recommendation of my testimony today. It concerns research to determine if our courts, no doubt with the noblest, but probably unrealistic intentions, are degrading the impact of IBLER on our national potential for invention and innovation.

I shall illustrate how these thoughts relate to technological progress by quoting three paragraphs entitled "The semiconductor tree" from Fortune's June 1974 article about the contributions of Stanford's Professor Terman to "California's Great Breeding Ground For Industry":

"A large segment of Santa Clara County's technological activity owes its existence to another, somewhat similar chain of circumstances. William B. Shockley, co-inventor of the transistor, returned to Palo Alto, his boyhood town, in 1956 and set up Shockley Transistor Corp. The transistor was the successor to the vacuum tube, perfected in Palo Alto fifty years earlier, and Santa Clara County was becoming the logical place for electronics manufacturing.

"In the fifties, Shockley gathered around him a large group of gifted young electronic specialists whom he picked from big companies and universities around the country. In 1957, however, his operation ran into trouble when eight of those bright young men, including twenty-nine-year-old Robert N. Noyce, left and with the backing of Fairchild Camera & Instrument Corp., founded Fairchild Semiconductors in Palo Alto. (Shockley subsequently went on to other things, becoming a center of controversy because of his view that intelligence is inherited, and that genetics, more than environment, accounts for low scores by blacks on I.Q. tests.)

"Fairchild became one of those amazing corporate seed beds. No fewer than thirty-eight companies, including Noyce's enormously successful Intel Corp., have been started by former Fairchild employees. They have turned part of Santa Clara County into "Silicon Valley," the world capital of semiconductor technology. With sales of about $2.5 billion, the valley accounts for about 8% of the total U.S. sales in electronics and some 40% of the nation's semiconductor output."

The Fortune article makes it quite evident that Silicon Valley is a key factor in the production of the technology-intensive items important in the "vital role in America's balance of foreign trade" discussed in the IEEE position paper and, therefore, in the Subcommittee's examination of "the extent to which United States industries are maintaining their positions relative to foreign competitors in important fields of technology development". The 1974 Fortune article lacks the perspective that throws light on motivation in successful entrepreneurship contained in the January 1970 articles of one of my exhibits, a three-part series entitled "Silicon Valley U.S.A." by Don C. Hoefler in ELECTRONICS NEWS. I do not recall meeting Mr. Hoefler who described me thus: "Despite an uncanny genius for spotting and recruiting talent ('hero worship,' admitted one of his early employees), he was less adroit in managing that talent." Hoefler does report person-to-person discussions with key figures of his story.

What I wish to emphasize in the Hoefler articles is the role that he describes of profit motivation among the individuals who made Silicon Valley so important a contributor to our nation's economic growth. I do not take a position on the accuracy of his specific reports of seemingly dubious ethical standards of the participants. Even as fables, his articles are worthy of consideration as guidance for planning legislation as it may influence innovation. I see the pattern described by Hoefler as an extension of the processes that I emphasized in discussing IBLER—incentives based on limited exclusive rights. IBLER is one facet of a motivation system that can harness selfish instincts, as well as altruistic ones, so as to create a social system that is far more noble in its contributions to human welfare than the sum of the motivation of the individual members.

I have here put in words for the first time what my own experiences and observations have led me to believe is an essential and fundamental feature of American greatness. One aspect of it, namely that contained in the limited exclusive rights incentive of patents—the IBLER concept—is, I fear, now threatened by the degradation by our federal courts of the significance of patents—a principal topic of my testimony and the subject of my first recommendation to the Subcommittee.

ARE FEDERAL COURT DECISIONS DEGRADING THE SIGNIFICANCE OF PATENTS?

An important input to my testimony came to me several months ago from a Berkeley, California patent attorney. As a director of the Alameda County Lawyers Club, he had asked me to speak on the topics mentioned in FORTUNE and, in addition, in response to my complaints that my sincerity on these matters had been publicly impugned, he had arranged for me to be given a polygraph test at the Club's expense. (I passed.) In the course of our discussions, he told me that his firm felt that the value of patents was being degraded and as a consequence he was himself shifting his emphasis towards business law. These statements indicated an attack on the incentives based on limited exclusive rights—my IBLER acronymn—that I had come to revere as a precious legacy from the wisdom of our founding fathers—a realistic wisdom founded on realism about the nature of man.

Accordingly, in preparation for this testimony, I discussed two specific cases with this attorney. The patent for one large company's invention, selling for more than $500,000, had been clearly infringed but the suit was lost when the federal judge found the patent invalid. In the other case, a three-man company, selling a hardware-store product, had lost its business because its market was flooded by products that infringed its issued patent; however, a remedy by law suit was not considered worthwhile because of the ten-to-one estimate that the issued patent would be found invalid.

If inventive and innovative individuals come to believe that these instances are representative, then *the resulting disillusionment about the value of patents will inevitably discourage the utilization of one of our nation's greatest resources—the intelligent initiative of inventive citizens.* I believe that *establishing what the facts are about the seeming degradation of patents in court is a highly appropriate activity for a Congressional committee on economic growth.*

In my further inquiries, I asked several qualified individuals whether the importance of patents was indeed being degraded. Some were no more alert to this possibility than I had been. But those who were aware expressed deep concern. One very disturbing case involved a close colleague of mine at Stanford. (That case was not available to me for quotation. However, a similar case was communicated to me in a letter dated 26 Aug 75 from Mr. Robert G. Merrick and I have submitted this to supplement my testimony.) The Stanford case led me to an article in Stanford University's CAMPUS REPORT (25 Jun 75, p. 8) based on a speech to the San Francisco Patent Law Association by Niels Rimer, Stanford's director of technology licensing. The gist of his position is summarized by the title of the article :"Nader, federal officials stymie technological innovation".

I discussed my concerns about incentives based on the limited exclusive rights (IBLER) aspect of patents during my telephone inquiries to Drs. Lewis Branscomb and Bruce Hannay, leaders of the research organizations of IBM and Bell Laboratories respectively. A key conclusion is that degradation of patents must unintentionally have the consequence of enhancing secrecy in industry—an effect opposite to the intent given to Congress in the "power to promote the progress of science and the useful arts . . . [through] . . . exclusive right[s]."

On the basis of my inquiries, I suggest organized explorations with the participation of highly qualified experts such as Branscomb and Hannay. One usable and organized vehicle is the IRI (Industrial Research Institute of New York City), an organization that I learned about from Hannay, last year's president. IRI has seriously reviewed closely related matters. I have read a draft of this paragraph of my testimony to Branscomb and to Hannay and have verified their concern about these matters and their willingness to do what they practically can to contribute to the wisdom of any related legislation that might develop because of the activities of the Subcommittee on Economic Growth.

Some added thoughts: One topic that may be worthy of a recommendation for review concerns what I have spoken of as the "law of excluded optimum". I have included an exhibit on this topic with my written statement. Our transistor work at Bell Laboratories did not exclude, but instead focused upon, the optimum that I have in mind. We worked on the basic science related to the very practical problem that the field-effect transistor did not work—whereas existing scientific knowledge said that it should. When I was directing Shockley Transistor while Silicon Valley was starting, I found it almost impossible to find government support for basic scientific studies in areas that might involve practical problems like the one that led to the transistor. Such problems include those related to manufacture or to device failure. The development people seemed afraid to support such work because they might be criticized for invading the research people's area and vice versa. Such restraints at Bell Laboratories would have stymied the program that created the transistor. Possibly such research is a natural area for government laboratories.

[I note that in the typed text of this testimony the name of H. E. Derrick Scovil is misspelled.]

In closing, I again express my appreciation both for the privilege of attempting to contribute to our nation's future and also for the stimulating experience provided by this occasion for preparing my testimony.

Chairman BENTSEN. Now, I would ask. Mr. Ramsey, I was interested by a proposal of yours to sponsor studies of potential commercial applications of newly developed basic science. But earlier in your statement you seemed to be speaking against it. You say in summary, the greatest needs for success in basic research are increased funding and protection from complicated administrative procedures and harmful evaluation of criteria. You do not feel that is in conflict?

Mr. RAMSEY. No, I feel those are completely mutually supportive. One is the question pertaining to the support of the basic investigation and the second involves explicitly looking over some of the products of the research from the point of view of picking out from some of them that do have a commercial application. I think they are fully compatible views. In fact, even mutually supportive views.

Chairman BENTSEN. Well, is this done now, by NSF, NIH, or private groups?

Mr. RAMSEY. I would say not very much, to my knowledge. Certainly not getting the mutually creative people involved. I think many of us would be happy to be called for meetings, spend a period of time, a few days a week, not only looking at our own developments, but also those of others, to see where there might be applications. I think this could be done very well.

Things of that kind have been done for various military developments, at various periods in the past. When there has been a worry about air defense, they frequently call together a group of potential contributors to worry about what could be done. And these groups have been remarkably effective in pulling together different things from science that could be used in that direction. But to my knowledge, it has not been done, and is certainly not done to any extensive way, by the supporting agencies for essentially civilian applications.

Chairman BENTSEN. A couple of you made the point earlier that if you have an inventor working for the Government, who comes up with a new invention, and yet does not share in the rewards, other than the salary already paid, you feel that there should be some royalty, some of the fruits garnered to him for the development of it, and that as a result there would be more such inventions for the country.

Mr. RAMSEY. I feel it would help. It is nice to feel that you get something out of it, because, in the case of the other rewards, they are much more indirect, and do not really stimulate the man concerned to think very hard about what can go in that direction. It is not a primary concern and there is no special inducement to pursue it further.

Mr. RABINOW. Can I make a statement about this?

Chairman BENTSEN. Yes.

Mr. RABINOW. I am supposed to be the most prolific inventor the Government ever had, and I have gotten a gold medal and some honors from the Government for this. The fact is that you cannot treat all inventions equally. There are inventions in weaponry, for example——

Chairman BENTSEN. It isn't bragging, if you have done it, and I guess you have done it.

Mr. RABINOW. I think it is bragging, even if you have done it, and I am not denying the fact that I am bragging, but the reason I say this is because it is necessary to say it, because I speak from some experience.

My feeling is that the Government should give the guy something, because the thing that makes for invention is the climate in which we live. It is not only the salaries, but the honors you get. If you want to create great basketball players, you give them honors and great money, and you get great basketball players. You pay Pele $4 million for playing soccer, and suddenly, soccer will take a new lease on life.

The thing that happens is that the Government does not reward inventors, and does not know what to do with its patents. Now, Russia, in which everybody works for the Government, found that they did not get as many inventions as they wanted. So they put in a patent system, and a Government employee in Russia does get royalties on his invention. They call it rewards based on the production quantity, but it is a royalty in one form or another.

I have met the man who runs the Russian patent system. I speak Russian fluently—I was born there—and I have found that their system works very well. They pay an inventor up to 20,000 rubles for each patent.

Chairman BENTSEN. How about a private corporation?

Mr. RABINOW. Well, private corporations vary. In IBM, you can get up to something like $50,000 as an award for an outstanding invention. Many companies give you something whenever you file for a patent. Some companies do nothing. It varies. For example, Polaroid, which puts out a tremendous number of patents, does not have an automatic reward system for inventions.

Mr. Land is the chief inventor. He likes inventions. He knows the difference between good and bad technical achievements. The inventors do very well in salaries, stock options, and so on. So, if you have a situation where the boss is an inventor, and he likes inventions, the people invent.

This happened in my company, because I loved inventions for their own sake, even if I could not use them. And the people feel it, and also the salaries show it. All I can tell you is, the climate is very important.

I would like to say one other thing which I forgot to say in my testimony. There was a statement made by one of the people of the Department of Justice that patents are really not being supported in court, that 72 percent of patents tried in courts are found invalid. Now the fact is that it is 72 percent of 1 percent, because only 1 percent of all patents are tried for validity in court. And the Department of Justice neglected to say that it is 72 percent of 1 percent, which makes the story quite different.

Our courts do not like patents in some districts. In the 9th District, they never support a patent. They have found every patent invalid that they have even tried in the last 30 or 40 years. In other courts, it is different.

I think Congress could do well to investigate why it is so, and try to clarify the patent laws. I do not think we should depend on court law, as the Department of Justice says they like to. I think the Congress should say what an invention is and what it is not. It is difficult to do, but I can assure you it can be done better than it has been done in the past, so that the courts will have something to go by.

Another thing I would like to see done is that the Internal Revenue should put a line into the tax return forms to say, "Royalties Collected on Patents." There is a line in the Internal Revenue form that just says, "Royalties." Unfortunately, it means royalties on patents, books, and music, and there is no way for the Government to find out exactly what it collects in taxes on patents. And if there were a separte line, "Patents, local and foreign," we would, I think, find a very interesting statistic—what the Patent Office collects from foreign countries in royalties paid to the American citizens—something over $1 billion a year. And the taxes on this amount to about half, which means that the Patent Office earns about half a billion dollars from abroad.

And yet there are bills in Congress that say the Patent Office should be made self-supporting by fees, and I think that is a tragedy. If the Patent Office collects in taxes from foreign countries half a billion dollars alone, one could very well say the Patent Office should be a service to society, and not have to be self-supporting.

Chairman BENTSEN. I have heard a couple references to Bell Laboratories, which has been a perennial success in communication technology breakthroughs. Is there something unique about Bell Laboratories as compared to other very large corporations?

Mr. SHOCKLEY. Well, I would like to say something about Bell Labs in respect to these points that have been made about motivations. The payments for patents at Bell Laboratories was, and I think still is, $1 to sign the patent agreement upon becoming employed. I think the point that Mr. Robinson made about his own experience is very relevant to what went on in Bell Laboratories. In contrast to that, if one tried to set up a set of rules whereby one would be able to determine the just deserts for contributions to invention and innovation by the process of examining what existed on records on paper, then I believe that the effects in a place like Bell Laboratories would be very adverse. It would leave out the most important feature of sound human judgment by competent administrators.

Cumulative wisdom based on competence and experience is going to be better than any rule that can be set up and put on a computer in an attempt to give to each person his just rewards for his contributions to creativity. The organization and the progression of people through Bell Laboratories and the tenure of individuals there—these are all of high caliber.

A few other places must be comparable in quality of personnel and continuity of experience, but I believe that Bell must be very near the top. This continuity in personnel and spirit means that one can count on about the best that can be expected from management considering the fundamntal human limitations that exist everywhere. Thus, in a good organization of capable experienced people, an employee can count upon a really high degree of justice. For example, I think of a person who might promote high inventiveness among the individuals in the group he led so that they would file many valuable patents while he, their leader, did not. Yet he might be the one most responsible for the group creativity. In a good organization, administrative judgment would give appropriate recognition and this would be felt, although perhaps not analyzed, by all who were involved.

The form of managerial justice that I am trying to describe is more difficult to codify so as to approach administration by mechanized rules than it is to carry out in practice by proven competent leaders. About the excellence of Bell Labs, I have often speculated about the extent of the influence of the humanitarian motives of Alexander Graham Bell. He was concerned with deafness and with other human problems and with how to improve the lot of mankind generally. To what degree, I wonder, do these basic humanitarian motivations and values of Alexander Graham Bell continue to permeate the system and contribute to its outstanding contributions to the benefit of people and to economic growth.

Another interpretation of the public-spirited posture of the Bell System is more cynical. This is that it is simply smart public relations rather than actual humanitarianism like Bell's. I don't know how to prove what is actually the case but my personal experience makes me favor the Bell tradition theory. I have not heard others discuss this topic and this is the first time that I recall making a public statement of my views. One item of my testimony that I feel supports my view is the remarkable continuity of the economic growth represented in the steadily decreasing cost of telephone service that I stressed in chart 3 of my prepared statement, and other quantitative facts.

Chairman BENTSEN. What do you think about the big ticket items, Government participation in the development of such things as the SST and the fast breeder reactor?

Mr. SHOCKLEY. Well, here I would think, and quote from what Norman Ramsey was pointing out—No, it was Ms. Ray who particularly emphasized this point—when there has been a national emergency of some sort, such as occurred in World War II, then the very best brains have been gathered together and have focused on objectives which were clearly in the national interest so that the consequence was that things just moved ahead with enormous speed. The Radiation Laboratory at MIT was one example, and the Manhattan project for the atomic bomb was another.

What I regard as an essential ingredient for motivation is a phrase that I used in my exhibit on the invention of the transistor. I learned this phrase from Enrico Fermi when I visited him in late 1939. At Bell Labs, we had been asked to do research on atomic energy. Jim Fisk and I had independently, as had several others, including Fermi, invented the essential feature of nuclear power—the principle of segregation. I am sure that when I talked to Fermi, he had already invented this. But neither of us mentioned it to the other. Fermi told me that Government support would be available for research to release atomic energy by producing a chain reaction. He then used the phrase "the will to think."

What Fermi meant by his "will to think phrase" was this: He needed to plan experiments about slowing down neutrons and other things needed to make a chain reaction work. To plan these would be hard work. It would involve thinking. Thinking is one of the hardest forms of work. It calls for will power. If the experiments could not be done because of lack of funds, the thinking would be futile. Fermi told me that assurance of financial support for doing the experiments gave him "the will to think" of how to design the best experiments. I believe that this concept of "the will to think" can be used to increase creativity.

I have written up in several places—one is my exhibit on creative failure and the transistor invention—how learning to live with failures can make one more creative. What I say about myself—and I am sure Mr. Stephens would say the same thing—is that, when we look at how long it took us to get certain ideas, we are impressed with how dumb we were—on how long it took us and how stupid we were. But we have learned to live with this stupidity, and to find from it what relationships we should have seen in the first place. This recognition that we aren't perfect but that persistence pays is a very important factor, I think, in giving one "the will to think"—you don't need to worry so much about the mistakes you make, providing they are not too dangerous or too expensive.

Chairman BENTSEN. I am very much impressed by the statements, and appreciative of them. And we have run out of time. Thank you.

Mr. Shockley, you have submitted a number of relevant articles, and they will be printed in the hearing record.

[The material referred to follows:]

EXHIBIT 1

SCIENCE, TECHNOLOGY AND AMERICA'S FUTURE

REDUCED FEDERAL SUPPORT OF RESEARCH AND DEVELOPMENT THREATENS THE UNITED STATES' HISTORIC POSITION OF WORLD LEADERSHIP

1. The Growth of America's Technical Power.—Since the founding of the Republic, when the Constitution gave to Congress the power "to promote the progress of science and the useful arts", it has been the policy of the government to encourage science and engineering. By supporting the educational programs higher learning, by encouraging the immigration of talented scientists and engineers from abroad, and by substantial financial support of industrial, academic and government technical institutions, the Federal government has become the senior partner in maintaining America's leadership in science and engineering. The government's role is measured by the Federal funds dvotd to support of research and development which, since World War II, have exceeded the funds available from other sources.

The national strength provided by this R & D effort pervades the social, economic and political life of our country. The measures of national strength—gross national product, per capita consumption of energy, productivity of labor and capital, balance of trade—all these reflect ultimately the impact of new and better products and of more efficient methods of production, and these in turn are based on technical know-how. As a direct consequence of the Federal and private support of R & D, and the products derived from that support, most Americans enjoy a standard of living far exceeding that of any other nation in the World.

The steady upward trend of economic indicators from 1950 to 1970 was paced by new technical ideas and methods, leading to such new products as the digital computer, and the myriad of other devices based on the ubiquitous transistor. Products like these, the so-called technology-intensive manufactured products classified by the Department of Commerce, have played a vital role in America's balance of foreign trade. Since 1951 such products have consistently generated a favorable balance of trade of from $5 to $10 billion, more than any other major commodity group. Today, with agriculture products, they provide the *only* favorable balance.

To maintain this leadership, we cannot rely on our present momentum. As the effects of obsolescence and competition have so often proved in the past, to *keep* ahead a *steady* infusion of new ideas and methods must be forthcoming. It is an axiom of society that the principal ingredient of leadership in any endeavor, be it with respect to nations, corporations or warfare, is a high degree of ingenuity and innovation on the part of the leader. Until 1965, there was no doubt but that the United States enjoyed leadership in ingenuity and innovation.

Prior to 1965, in fact, Federal and private support of R & D engaged an increasing share of the productive effort and resources of our country. During the decade from 1953 to 1963, for example, under the influence of the Cold War and the Apollo Project, total support of R & D doubled in relation to the gross national product, reaching a peak figure of 3% of GNP. *But since 1964, the trend has been reversed.* That total R & D funding fell to 2.3% of GNP in 1974. The drop in Federal support of R & D has been even steeper, dropping from 2% of GNP in 1964 to 1.2% in 1974.

Reports that may mislead the unwary show that support of R & D, in both the public and the private sectors, continues upward in *current dollars.* But since R & D consumes man-hours and capital equipment that must be paid for at continually inflated prices, these figures must be recast to eliminate the effects of inflation. In constant 1967 dollars, Federal funding of R & D has been *decreasing* since 1966. Non-Federal support, on which the Administration has counted to take over an increasing share, remained relatively constant from 1969 to 1971 and has displayed a downward trend since 1973.

These are clear danger signals. It will not be possible for the U.S.A. to maintain her leadership in science and technology, if these trends continue. The inevitable result will be a concomitant economic and political decline, meaning that the relative quality of life for citizens of the U.S.C. will suffer.

2. The Downward Trend in Government Support of R. & D.—Recognizing the crucial role that research and development played on both sides in World War II, Congress and the Administration early in the post-war period set up funding procedures to encourage R. & D., principally in connection with contracts issued by the Department of Defense. These methods included direct grants to non-profit research organizations, cost-plus-fixed-fee (CPFF) contracts to universities and to other organizations having research staffs and laboratories, an allowable cost for industrial research and development in connection with production contracts, and CPFF funding of research and development proposals. In addition, the Department of Defense and other government agencies conducted substantial R. & D. efforts with their own laboratories.

The primary thrust of the DoD support was related to weapon systems and their components. In addition, there was a clear and acknowledged appreciation of the importance of *advancing the state of the art* in non-weapons systems. Thus, technical work was funded that had *exploration* as its focus, looking for new paths and breakthroughs. Federally funded research could be conducted with freedom to pursue objectives of indirect value to a specific weapon or system. In those years, there was open recognition of the "spreading power" of undirected, long-term research.

After the broad specifications for the Apollo Project were laid down, the principal effort of NASA in the space program was to meet these specifications, to produce reliable hardware, to develop new systems and new, difficult-to-attain standards of systems performance. The groundwork for the attainment of the Apollo Mission's goals was established in basic research leading to advancement of the state of the art in specific areas (for example, solving the problem of dissipating the heat generated during rocket re-entry to the Earth's atmosphere, new fuel cells and solar cells, and many other similar advances). The achievement of the Apollo Project was a triumph of systems engineering, in which the space program broke ground entirely new in the history of organized human endeavor. At all events, even before the goal was achieved, the lowering priority of the space effort was evident, and NASA's contribution to Federal support of R. & D. has displayed a continually decreasing percentage since 1966.

Equally important, in the mid-1960's new attitudes surfaced in Defense R. & D. Support of free and open inquiry in research was deemed inappropriate in the DoD. The key-word persisting today so far as DoD research is concerned, is "relevance". If the proposed R. & D. program is directly relevant to a weapon, a system, or a mission, it is eligible for DoD support. If such direct relevance is not evident, no DoD support is forthcoming. The heritage of President Roosevelt's Science Advisor, Dr. Vannevar Bush—who influenced so heavily the post-war thinking on Capitol Hill and in the White House—had come to an end.

In retrospect, it is evident that this shift in emphasis was caused primarily by the shortage of funds resulting from the U.S. commitment in Asia. It is fair to say, in fact, that the change in the DoD rules was forced more by the drain on our resources than by loss of interest in basic research. Whatever the cause, the result is the same: the large resources of the Defense Department are not available for speculative research.

These changes have tended to underplay the broad objectives of basic research and to overplay the narrower objectives of advanced development. The result has been fewer break-throughs, fewer new basic new ideas, less new knowledge to be tapped by DoD, NASA, by the public and private sectors at large.

The change in support in DoD was coupled with a plan that basic research would increasingly be supported by another Federal agency, the National Science Foundation. It was also coupled with the hope that basic research, without which no long range program of new product development can survive, would be supported by the private sector, when it was clear that sufficient funds were not available for it from the Federal government.

NSF R & D funding has never been large (currently less than $1 billion) and it remains small compared with the R & D expenditures of other government agencies (HEW, ERDA, DoD and NASA together have R & D obligations in fiscal 1975 just exceeding $15 billion, in 1975 dollars).

While NSF has supported basic research within its resources, there has been a reduction of support of research—with long-term objectives—by DoD and NASA.

3. The Trend in Private-Sector Support of R & D.—The expectation that non-Federal (private sector) support of R & D would close the gap left by decreasing Federal support has not been realized. Even before the present sharp recession in business activity forced cutbacks in industrial support of R & D, the non-Federal expenditures, in 1967 dollars, had leveled off, following a steady increase from $2.1 billion in 1953 to $8.8 billion in 1967. The peak of $10.6 billion reached in 1973 has been followed by a decrease to a figure estimated for 1975 at $10.1 billion.

Although Federal funding continues to exceed non-Federal today, the excess has narrowed to about $1 billion, or roughly five percent of the total funding—the lowest percentage in post-war history. Suffice to say that the combined effect of reduced government and private-sector support has over a two-year span produced a *decline in the total R & D effort of the country amounting to 10%* ($2 billion in constant dollars).

It must be emphasized that these trends are expressed in dollar amounts corrected for inflation, and hence represent the substantive value of the R & D *effort* funded, rather than the current *price* of that effort. The accompanying chart, which shows the trend lines both in current and in constant dollars, indicates how misleading the current-dollar trends are.

No responsible assessment of future needs can be made in terms of the current costs in 1975 dollars. What counts in achieving results from the R & D investment is the quality and quantity of the manpower and equipment involved. *A drop in two years of nearly 10 percent of the real value of the R & D effort of the country cannot be dismissed on any basis other than that it is a temporary shortfall. It must be rapidly corrected.* If the trend is allowed to continue, America's leadership in science and technology must falter, and we will surely become, as quoted by Michael Boretsky, "just another industrialized country".

4. Recommendations.—In view of the facts reported herein respecting the reduced support of R & D effort in the United States, we recommend that policies and procedures be adopted by the appropriate Federal government agencies, with the support of the Congress and the Administration, to insure:

a. That the national policy be to devote 3% of the Gross National Product to R & D, and that the Congress and the Executive Branch take the requisite steps to assure that this ratio is maintained.

b. That the Federal government take the necessary steps to fill the gap between private-sector support of R & D and the 3% figure cited above.

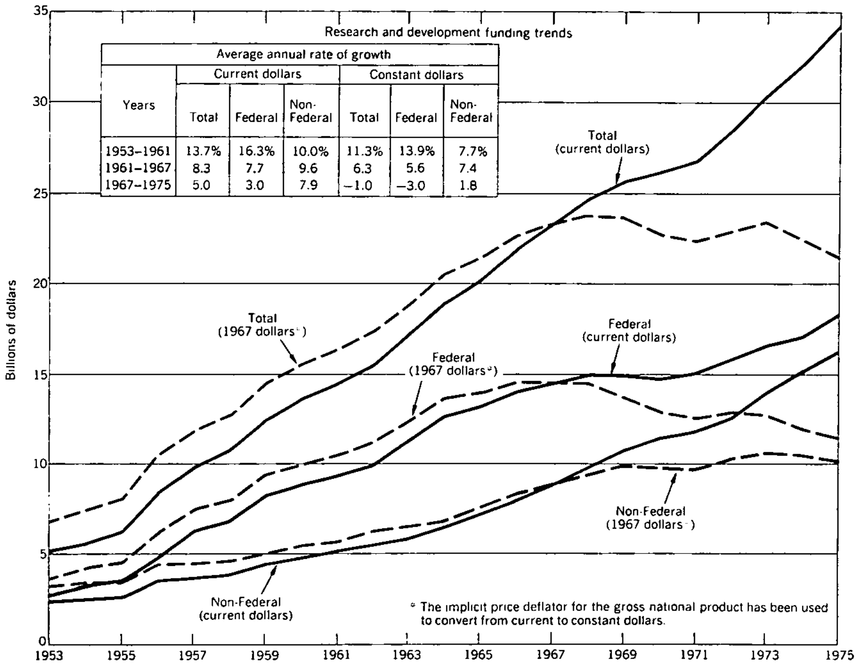
c. That the ratio of the funding of R & D by private institutions and manufacturing industry to the GNP be maintained at or above 1%, through encouragement by government allowances and inducements, so that a growth rate of the industrial component of the gross national product, is achieved and maintained. Specifically, we recommend that a substantial (e.g. 10–12%) depreciation tax credit be allowed on investment in research and development, and that an allowance (e.g. 1%) on government contracts be permitted for support of industrial research and development.

d. The government regulations and procedures that presently discourage or prohibit the funding of basic research, having long-term objectives, be modified to encourage such research in any field within the competence and mission of the funding agency.

e. To guide the effort aimed at achieving the national R & D goal of 3% of GNP, that broadly qualified scientists and engineers be involved at the policy-making level in both the Legislative and Executive Branches. Specifically, it is recommended that a close relationship between the President and the technical community be established through such means as the appointment of a Presidential Science and Technology Advisor with direct access to the President.

5. Conclusion.—It is the considered opinion of our Institute that only by taking such steps as are here recommended can the United States regain its momentum in science and engineering. Unless the lagging support of R & D in the United States is promptly replaced by healthy growth, the traditional leadership of the United States, and the quality of life enjoyed by her citizens, will inevitably suffer.

CHART 1



Source: National Science Foundation

CHART 2

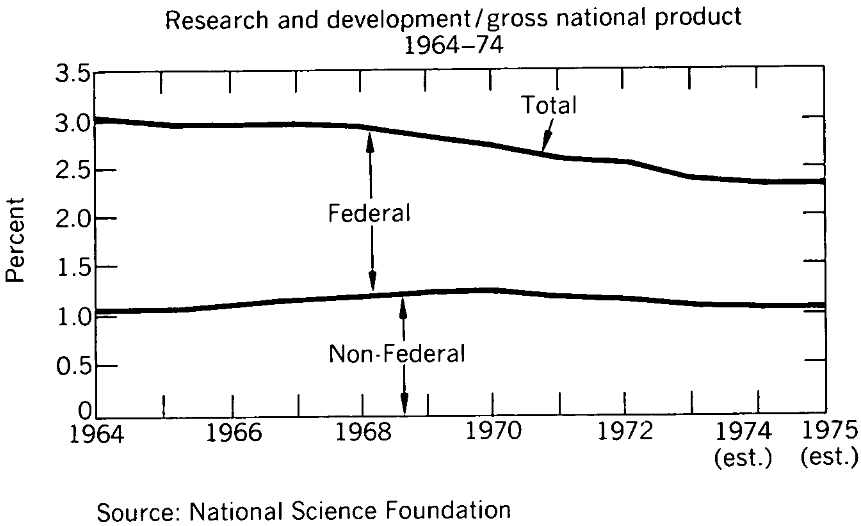


EXHIBIT NO. 2

[Excerpt from "The Impact of Science," Conference Number IV of a series entitled "California and the Challenge of Growth," sponsored by the University of California, San Diego, June 13–14, 1963]

SCIENTIFIC THINKING AND PROBLEMS OF GROWTH

(By William Shockley)

Science and industrial growth are related in many ways. In fact, as I shall maintain in this talk, science as applied by engineers is the chief source of industrial growth and of increase in standard of living for the population as a whole. Before discussing this aspect, however, I would like to emphasize that other relationships exist between science and the problem of growth. In particular, scientific methods of thinking have been useful to the human race in dealing with problems of all sorts having to do with quantitative aspects of the universe, the world, and society. Problems of growth are quantitative in many aspects. I shall also illusrtate a scientific attack on an important social problem by indicating how a quantitatve approach toward the operating of research and development by the government can reveal certain fundamental weaknesses. Quantitative facts about these weaknesses may in turn suggest valuable means of making corrections. As a somewhat facetious example of the application of scientific thinking to growth problems, I shall start by discussing the mechanisms of approach to equal unattractiveness of all place to live.

My comments in regard to the theory of "iso-unattractiveness" are only halfway facetious. Applying scientific reasoning and a detached approach to the problem of growth in California, I come to some very unpleasant conclusions. I think these unpleasant conclusions should be faced by the citizens of California and that action should be considered by their representatives in government. Briefly, what I think is happening is that the natural attractiveness of California is being offset by the rapid growth of population, so that in due course it will become as unattractive as some of the least desirable industrial cities in the East and Middle West or the baking climate of portions of the Corn Belt.

In brief, the theory of iso-unattractiveness compares people to heat energy and unattractiveness to temperature. It is well known that higher temperature drives heat energy to places of lower temperature. The flow of heat continues until finally an isothermal situation arises. In the case of human beings, higher unattractiveness drives people to places of lower unattractiveness. Just as heat energy raises temperature, people increase the unattractiveness of a place. They produce smog in the air by burning leaves, newspapers, and other rubbish in their back yards. When they don't do this, the disposal companies may frequently dispose of these same waste products to produce air pollution. The presence of people tends to clutter up the scenery with unattractive advertising signs and the edges of the highways with beer bottles and other rubbish. Only after these effects become so obnoxious that they offset the climatic advantages and some people move away as fast as others arrive does the net flow of people towards such a place decrease. When this occurs, iso-unattractiveness has been established. (It should be remarked in passing that here is an initial transient phase in which people make places more attractive; for example, in a completely desert area there may be an initial increase in attractiveness by the appearance of a store that sells food and a place where medical attention can be received.)

What can be done to offset the dismal future for California predicted by this theory of iso-unattractiveness? I think there can be a good answer only if the people are prepared to face the issue and take remedial and precautionary steps. To offset the smog problem additional research can be carried out and better, and probably more expensive, means found to dispose of waste. For example, revenues from a one-dollar-a-gallon tax on gasoline sold in California could be put to good purpose for control of pollution and better research on control methods. Of course, the one-dollar-a-gallon gasoline tax in itself is extremely unattractive; this alone would keep many people from coming to California. From my point of view and that of many of us, this is a desirable outcome; we would prefer to have financial unattractiveness[1] in this beautiful country due to the high cost of improvements in ways of living than to have physical unattrativeness resulting from destruction of the natural beauty by a rapid, uncontrolled increase of population.

I shall next consider economic growth in general and relate this to the contributions made by science.

One of the standard methods of measuring economic growth is to study the change in gross national product. The gross national product, however, is a poor measure of economic growth because of the changing value of the dollar. Furthermore, it gives little information about the welfare of individuals, since if the population grows faster than the gross national product, individuals will not necessarily be any better off. Consequently, other measures of growth may be much better. One of these, for example, is the availability of electrical power in kilowatts.

Figure 1 represents the growth of electrical-power-producing capacity over a period of about three decades. This is not the actual rate of production; it represents the capacity of the electrical-producing plant to generate power for several countries. Electrical power is a measure of technological progress in a country, and the rates of increase are much greater than those of the gross national product itself. It is interesting to note that the rate in the USSR is significantly higher than in the United States, so that, if this difference continues, in about 1985 the USSR will have caught up with us. The fact that East Germany's growth is a lot lower than the USSR poses some interesting questions about the internal structure of the Communist community. West Germany is growing roughly as fast as the United States.

In respect to problems of growth, a very significant number to consider is the percentage increase per year. An important mental tool here is the quantity 70 per cent. (This is a consequence of the mathematical fact that the natural logarithm of 2 is approximately 0.70.) This 70 per cent quantity has the consequence that in order for something to double at a rate of 3.5 per cent per year it will take twenty years, so that 20 times 3.5 per cent equals 70 per cent. At 7 per cent a year it takes ten years to double, and so on. It is seen that at the rate of increase of 13 per cent a year prevailing in the USSR it takes a little less than five and one-half years for the kilowatt capacity to double.

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[1] The mathematically inclined reader will realize that unlike the scalar quantity temperature, unattractiveness is probable a nonlinear operator on the taste vectors of individual's

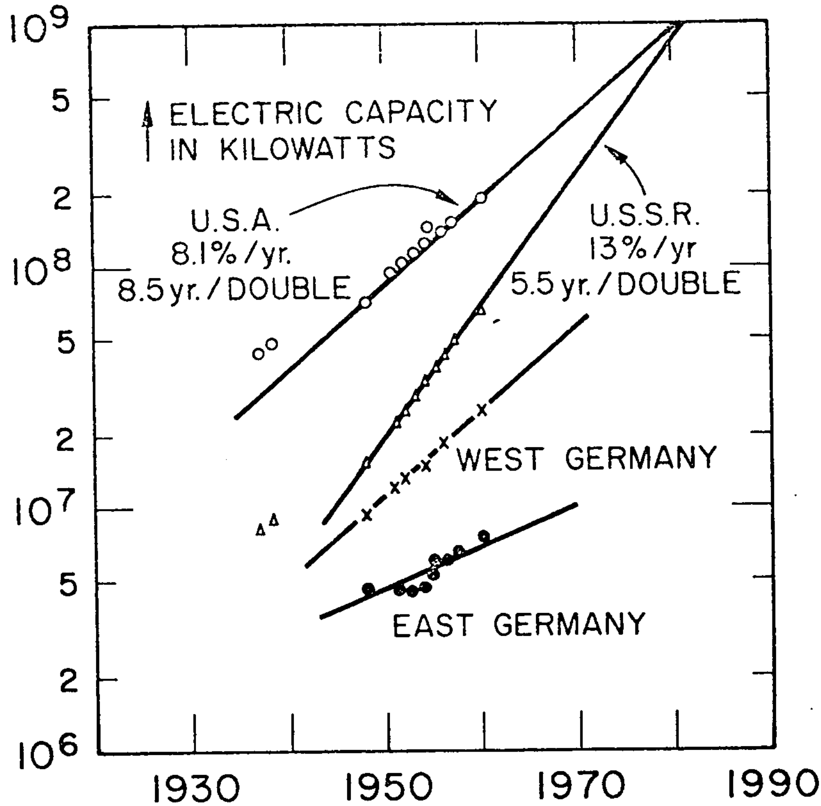

FIGURE 1.—Growth of electrical production capacity in several countries. These data are from the *United Nations Statistical Year Book* for 1958 and *the World Almanac* for 1962.

The best measure of true economic growth that I have found is the measure of improved standard of living given by the increase in "real wages." Real wages may be described in a simplified form as follows: In 1890 an industrial laborer earned about \$.15 an hour, and eggs cost \$.20 a dozen, so that a laborer could buy 0.7 dozen eggs for an hour of wages. In 1957 the corresponding values[2] were \$2.00 an hour and \$.57 a dozen; consequently, in 1957 the laborer could buy 3.6 dozen eggs per hour, so that "real egg-wages" went up by a factor of 5. Figure 2 represents real wages based on a far more representative cross section of items than simply dozens of eggs. The unit used is real wages in 1914 dollars, and a curve has been constructed on the basis of data obtained by Stanley Lebergott.[3]

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[2] Data furnished by Professor Albert Rees of the Department of Economics, University of Chicago. I am also indebted to Professor Rees for putting me in touch with Professor Stanley Lebergott.

[3] Data furnished by Professor Stanley Lebergott, Wesleyan University. See his *Manpower in Economic Growth, the American Record Since 1800* (McGraw-Hill, to be published).

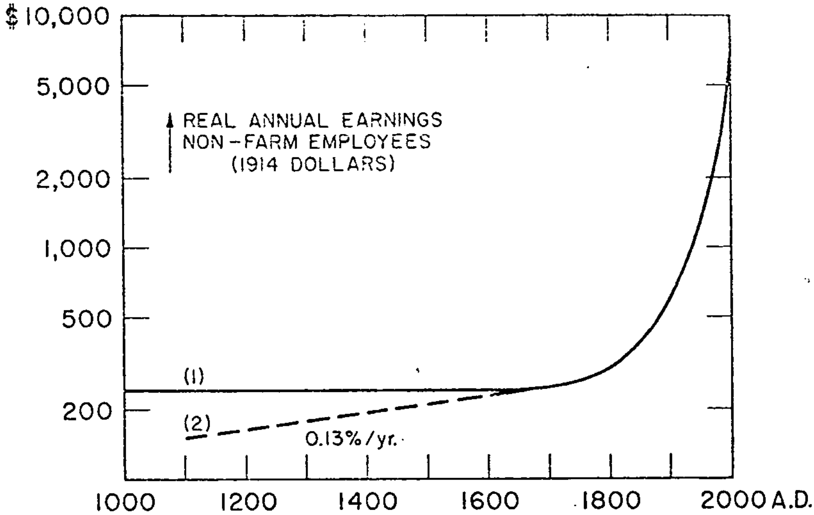

FIGURE 2.—Real annual wages estimated back to approximately 1000 A.D.

I shall discuss the part of the curve from 1860 to the present in more detail below, but first I should comment that the earlier part of the curve is based on a qualitative judgment[4] together with the fact that it is hard to see how a laborer could have supported his family on an income of less han $150 (1914 dollars) per year. If this value corresponded to the year 1100 as shown on the chart, then the rate of increase of real wages in the Middle Ages was only 0.13 per cent per year, so that approximately 500 years are required for real wages to double.

Figure 3 shows the actual data on which the curve of Figure 2 was based.[5] This curve has been fitted by a simple analytic formula based on the concept of the *engineer multiplier*, which I shall discuss below. The analytic curve is actually simply an "exponential," which is the mathematician's name for the growth associated with compound interest or a geometric series. What this means is that the rate of increase of real wages itself increases exponentially, so that the real wages themselves are the exponential of an exponential. This is indeed a very rapid rate of growth. What has produced such striking increase during the last century? Why, from 1100 to 1800, did real wages increase so little?

There can be little doubt in the minds of technologically competent analysts that the major cause of the growth of real wages is the exploitation of basic science by engineers. Further evidence that this is indeed the case is found by comparing the doubling time for the rate of growth of real wages, shown in Figure 3, and the rate of growth of engineers in this country. It is found that the time of forty-nine years required for the fraction of the population with engineering training to double[6] matches with a high degree of accuracy the years required to double the rate of increase of real wages in Figure 3. In fact, the rates of increase of Figure 3 are generally consistent with the value 40 for the *engineer multiplier* factor discussed below.

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[4] Professor Lebergott has expressed the view that almost no increase in real wages occurred between 1000 and 1700.

[5] Stanley Lebergott, *op. cit.*

[6] This doubling time of forty-nine years is deduced from nineteen years to double the rate of production of engineers (see *Investing in Scientific Progress*, National Science Foundation, 1961) and from thirty-one years for the doubling time of the population as a whole.

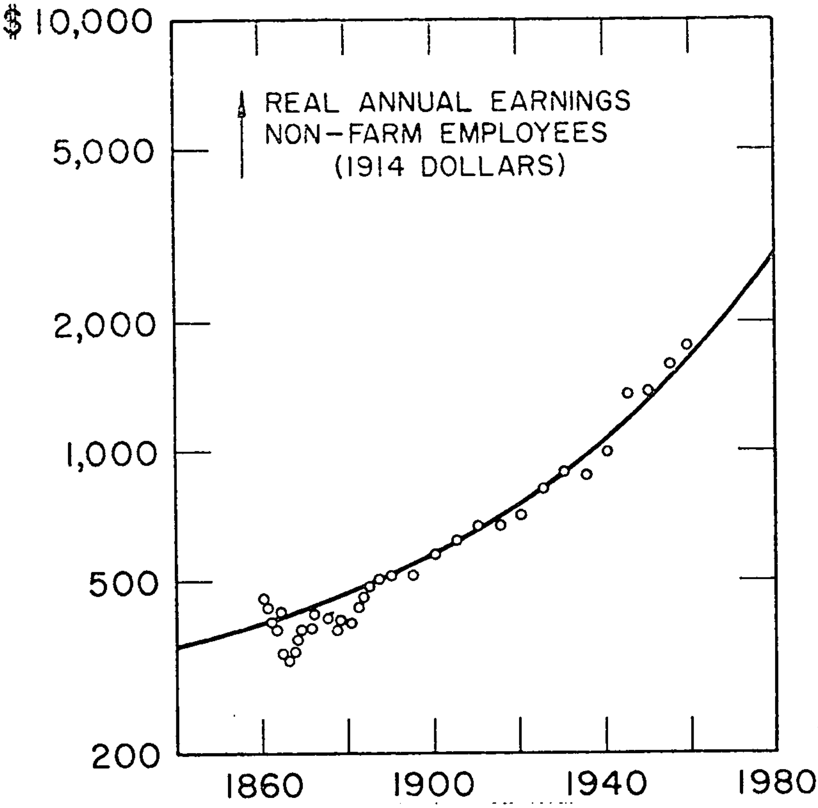

FIGURE 3.—Real annual wages in 1914 dollars from Stanley Lebergott's table.

It can of course be argued that greatly increased production of trained engineers is only an effect rather than the chief cause of economic growth. In fact, some economists argue that the chief cause of economic growth is simply the accumulation of capital. This argument appears to me to be a ridiculously untenable view if one considers the flatness from about 1100 to 1700 shown in Figure 2. It takes a fantastic naiveté to assert that during these countries the economic balance happened to be so perfect that the availability of capital investment remained so precisely balanced with depreciation over this long span of time that real wages changed only 0.13 per cent per year. Instead, I believed that the cause of the flatness was that there were simply no scientific discoveries and technological applications of sufficient importance to enable man's labors to be used more effectively; without technological inventions, like the steam engine, more capital could add little.

As a concrete example of the way in which real wages have increased because of technological progress—whereas they could not have increased significantly without it—let us consider real wages in terms of telephone calls. This is shown in Figure 4.[7] In this case two numbers are compared, the hourly earnings of

[7] I am indebted to Gordon N. Thayer, Vice President of American Telephone and Telegraph, for furnishing me the data shown in Figure 4. To the best of my knowledge, these data have not previously been published, certainly not in the form presented here.

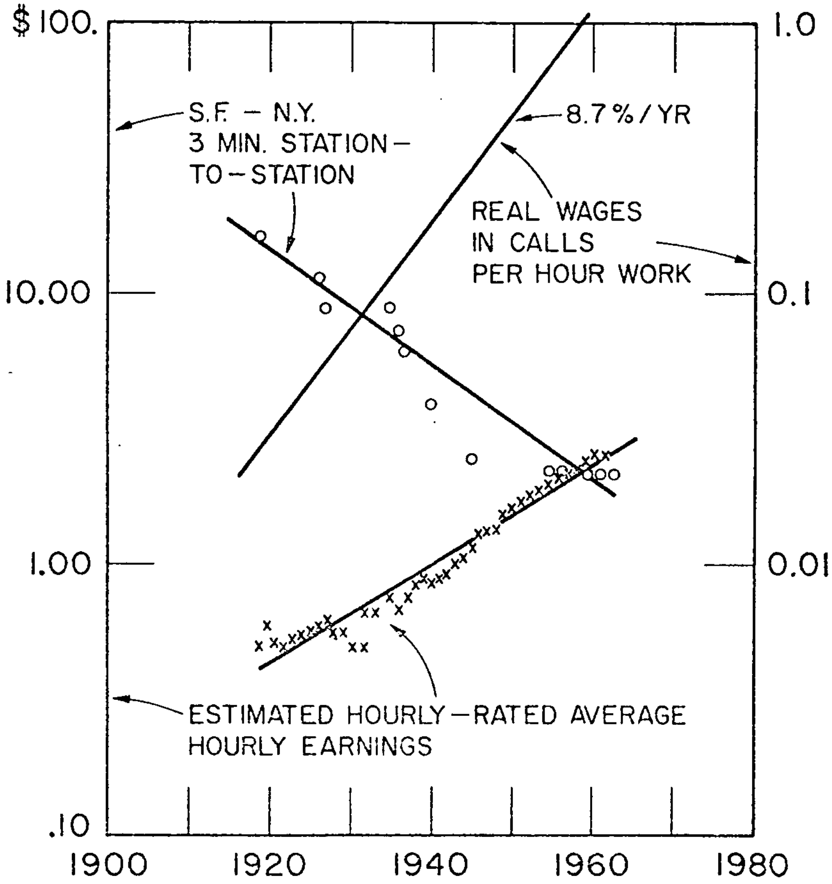

FIGURE 4.—Growth of real wages in terms of long-distance telephone calls.

"hourly-rated" Western Electric workers who manufacture telephone equipment and the cost of a three-minute, transcontinental, station-to-station telephone call. It is seen that in about 1920 a worker could buy only 0.02 telephone calls per hour of work; in other words, a week's wages would be sufficient to buy only one such transcontinental telephone call. On the other hand, by 1960 he could buy more than one such telephone call per hour of work. There can be no doubt that this fiftyfold increase in real wages in terms of phone calls resulted from improved technology with reduced costs of telephone service. Without this improved technology capital investment could not have produced anything like the same effects.

In discussing Figure 3, I indicated that the results were consistent with the concept of the *engineer multiplier*. I arrived at the concept of the *engineer multiplier* in connection with the activities of a committee concerned with technical education, working for the President's Science Advisory Committee.[8] The problem that I felt should be studied in connection with these activities was to find out what the optimum number of people to have engineering training should be. The *engineer multiplier* study has not answered this question except to suggest that there is no sign that overproduction of engineers is close. In order to attack this problem, I ascertained the increases in the gross national product per year for about half a dozen countries and divided these by the number of engineers graduating per year; the result turned out to be approximately the same—roughly $400,000 per year per engineer graduated, within about $100,000,

[8] *Meeting Manpower Needs in Science and Technology*, Report of the President's Science Advisory Committee (1962).

for Belgium, France, Italy, Germany, the Netherlands, Norway, Sweden, Canada, and the United States. I believe that this surprisingly small variation from country to country must in part be due to some accidental chance. However, it led to the concept of the *engineer multiplier,* which can be stated as follows: If we assume that during his career the engineer may be regarded as making the same contribution to the gross national product as if a certain number of people had been added to the labor force together with their necessary production equipment, then we conclude that the value of $400,000 per year engineer corresponds to engineers affecting the economy as if during their career they added to the labor force the equivalent of about forty production workers plus their production facilities.

The number 40 is the *engineer multiplier.* It means that producting an engineer has (only on the average, of course) the effect of adding throughout future history the effect of forty industrial workers. Of course, the basic science must be in existence for the engineer to exploit for the benefit of greater real wages, so that scientists are needed also.

An *engineer multiplier* of 40 corresponds to a rate of about two production workers per year per practicing engineer during his career. The value obtained in this way is quite consistent with the results shown in Figure 3, thus adding some confidence to the conclusion that applied technology is a major cause of improved standards of living.

On the basis of the conclusions presented above it is evident that the fruits of research and development are the major source of economic growth. Furthermore, as is well known now to thoughtful taxpayers, the principal resources for carrying out research and development are the tax dollars as administered by government agencies. How well are these funds administered? How much better could they be administered? These questions must be answered in terms of the quality and competence of the government agencies involved in handling the technical and administrative aspects of these research and development programs.

In order to attack the problem of quality of technical personnel, we must consider a measure of scientific productivity and the extremely large variations from one individual to another.[9] In a previously published study I have found that this distribution can be well expressed by what is called a "log-normal distribution." This technical phrase can be illustrated with the aid of Figure 5, which shows the scientific publications appearing in *Science Abstracts A* (this is devoted to physics) for ninety-five members of Brookhaven research staff (who practice physics) for a four-year period ending in 1955; one finds that the least productive 10 per cent published on the average a quarter of one paper per person during the four-year period. In contrast, the top 10 per cent published as creative in this regard as the bottom 10 per cent. This confirms the well-known result that a small percentage of the technical workers do most of the creative work.[10]

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[9] Shockley, W., "On the Statistics of Individual Variations of Productivity in Research Laboratories." *Proceedings of the Institute of Radio Engineers,* New York, N.Y.: Institute of Radio Engineers, 45 : 279 (Jan. 1957).

[10] Readers who are particularly opposed to this conclusion that published papers are a valid statistical measure of research output are referred to a discussion in W. Shockley, *op. cit.*

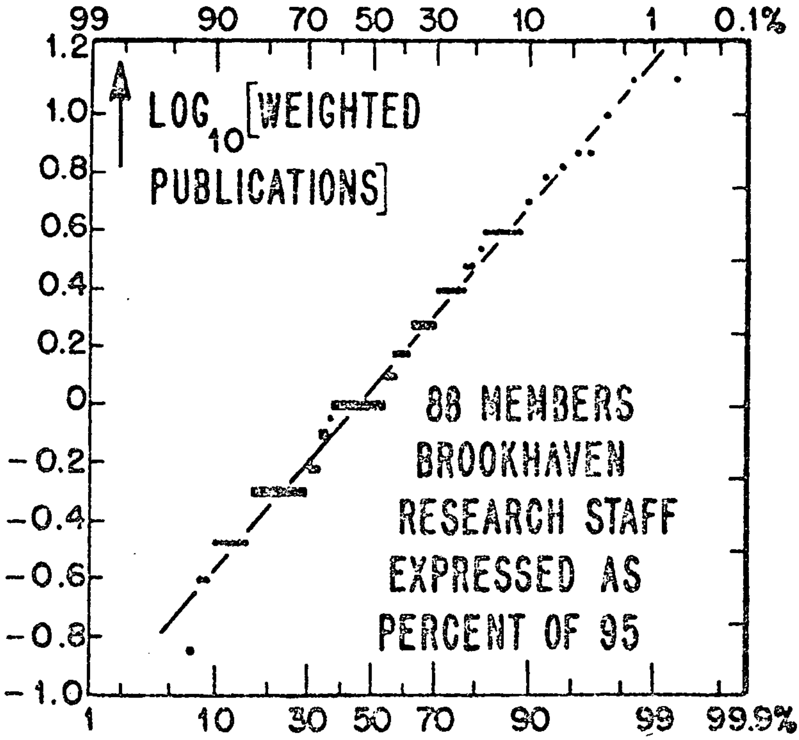

FIGURE 5.—Distribution of the logatrithm of "weighted" rate of publications over a four-year period at Brookhaven National Laboratory.

To relate this to the problem of technical administration of research work, we have several other questions to consider. In particular, are the most effective people so expensive that use of others is cheaper? To study this, consider the relationship of salary to this log-normal distribution of personnel: Figure 6 represents a plot for a different laboratory, namely, the National Bureau of Standards. This shows the age of individuals and their salaries as of 1954. As is represented on the diagram, this population is divided into four quartiles, the top quartile containing those individuals in the top 25 per cent of the salary range for their age group. Similar curves can be and have been constructed for a number of other laboratories.

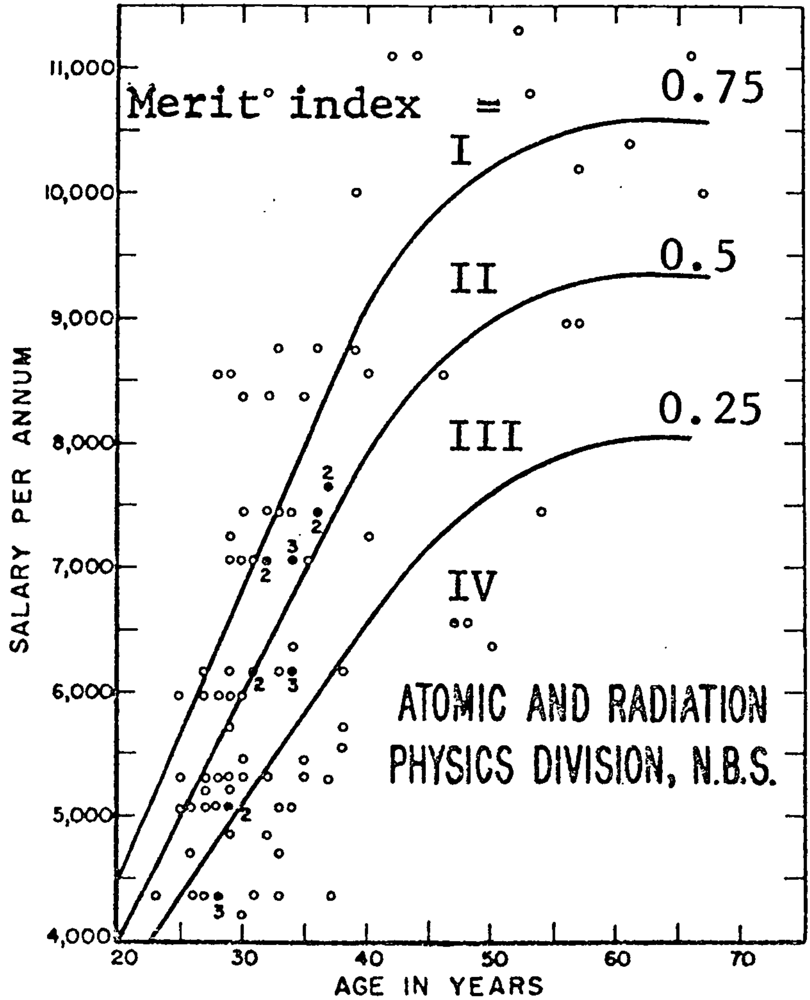

FIGURE 6.—Salary versus age for Atomic and Radiation Physics Division, National Bureau of Standards, together with "merit quartile" divisions.

On the basis of quartile divisions like those shown in Figure 6, groups of people have been found for a number of cases; these are shown in Figure 7. What is seen here is that the scientific output increases markedly from quartile to

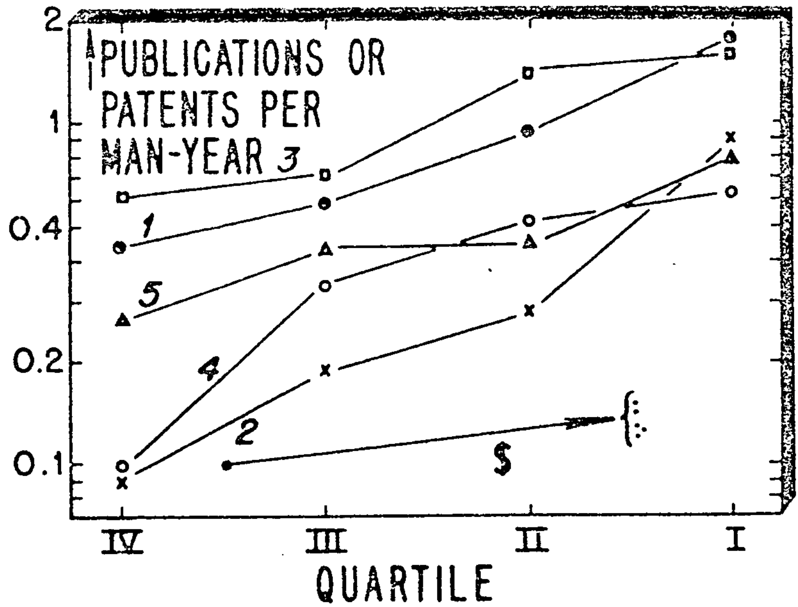

FIGURE 7.—Relationship between productivity and quartile number and salary quartile number for several laboratories.

quartile. In contrast, we see that the relative salary in dollars increases by a small percentage between the dividing lines between quartiles 3 and 4 and the dividing line between quartiles 1 and 2 at the age thirty-five. The general conclusion from this is that for a man to get about a 10 per cent salary increase he probably has to increase his scientific output by 30 to 50 per cent. Conversely, if one can buy a better man by paying 10 per cent more, the chances are one will receive 30 to 50 per cent greater output. This profit factor of 30 to 50 per cent more output for 10 per cent more cost is a statistical effect, and if one can actually pick some of the top producers from the distribution represented in the log-normal distribution of Figure 5, then increases in effectiveness of many hundreds of per cent can be achieved. Those experienced in research and development are sure that equally large effects occur for research and development management as for personal scientific creativity.

How well does Civil Service succeed in getting and keeping top producers? Particularly top research and development managers? To find out the degree to which the really creative people have been retained in government to aid in administering the vast and important research and development programs, consider the concept of the "merit index" and the merit index history of an individual. On Figure 6 a merit index variable has been introduced. A man who comes just at the bottom of the first, or best-paid, quartile has a merit index of 0.75; a median man has a merit index of 0.5. In a Civil Service laboratory salaries may not advance as rapidly as in industry, but an outstanding man still will be pushed ahead of his contemporaries and will acquire a merit index approaching unity (the top salary for his age) at a relatively early age. Do the Civil Service laboratories succeed in retaining these outstanding people? What types of individuals are actually found in key administrative positions? There is the general fear that good people who rise to the top of the salary structure then leave to look for better paying opportunities at the time they get ready to have their children go to college.

In 1957 I introduced the merit index as a tool in an objective way of studying the problem of quality losses in Civil Services laboratories and in Civil Service generally. The study was actually carried out for a period at the National

Bureau of Standards.[11] There are two parts to the study. In one part the losses are analyzed in terms of their distribution for the various merit quartiles. It was found at the Bureau of Standards that the top quartile had approximately 50 per cent more losses than its proper share. The inevitable consequence of this, if continued over a long time and not balanced by suitable additional recruiting into this top quartile, is that the best quality leaves, so that finally the remaining population is composed of the individuals originally less wanted by the organization. In point of fact, little recruiting is done into the top quartiles, and it seems highly probable that on a statistical basis there is a gradual deterioration of quality.

It does not, of course, follow from the foreging observations that any individual who has risen to a key position in a government laboratory is necessarily a person of inferior ability who has simply risen to this position through lack of competition due to more able people leaving. He may prefer the real advantages of work in a government laboratory. He may have developed his abilities over a long period.

The second part of the analysis studies the merit index of individuals throughout their careers. If the general trend is for top level people of high merit index in the mature age range of forty-five to fifty-five to have a history in which they have gradually risen from lower merit indexes, then it is extremely probable that in most cases this is due simply to lack of competition, and, in effect, mediocrity is rising to the top. To see how this may occur, consider Figure 8. This compares what may happen in an industrial laboratory and a Civil Service laboratory. Part (a) of Figure 8 represents man A who comes into an industrial laboratory at age twenty-five with a merit index of 0.8. This means that only 20

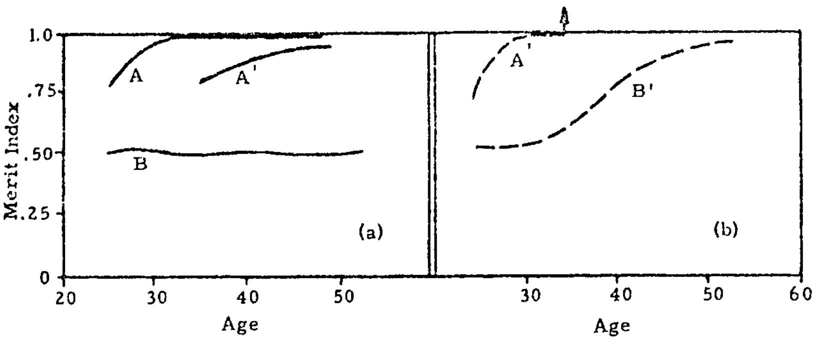

FIGURE 8.—The merit index history of individuals as a method of analyzing quality losses.

Part (a)—Hypothetical data for an industrial laboratory.
Part (b)—Hypothetical data for a Civil Service laboratory.

per cent of his contemporaries got more salary at the time. This man A went right up to the top within a few years, and that is where he is now at age forty-eight.

Now consider Dr. A′ the identical twin of Dr. A. He went to a Civil Service laboratory, and his history is shown in Part (b) of Figure 8. He was also early recognized as exceptional, and his supervisors moved him up quickly in the salary scale at the cost, no doubt, of great effort and ingenuity in writing job descriptions extending his responsibility, and the like. Thus his merit index also rose quickly to the neighborhood of 1.0.

[11] *Aviation Week and Space Technology,* New York, N.Y.: McGraw-Hill. Vol. 76, No. 3 (Jan. 15, 1962) and *The Competition for Quality,* Federal Council for Science and Technology (Jan. 1962).

At age thirty-five, however, Dr. A′ decided he could no longer put up with the red-tape headaches of Civil Service while getting a salary less than half of his brother's, Dr. A. and he accepted a position in the laboratory of Part (a) of the figure at an increase of about 50 per cent in salary, thus starting at merit index 0.75. His outstanding ability was put to effective service as he became familiar with the organization; he was given greater responsibility; and at age fifty he has nearly caught up with his brother and has a merit index of about 0.95.

Now consider two brothers, B and B′, who are responsible but average workers. In industry, B runs along at a median merit index for his entire career. On the other and, B′ in a Civil Service laboratory finds the competition thinning out. By age forty-five almost all the more able people have left and B′ has achieved a merit index of 0.95. The rise of B′ to this high merit index is gradual, of course, since it is largely determined by the departure rate of the more able people.

It is of course possible that an individual with a merit index in a government laboratory like B′ may actually have been an individual who matured slowly and is really one of the outstanding individuals whom the country is very fortunate to have in this position. However, if the situation is statistically in general as represented in Figure 8, then there can be little doubt that the key government administrators who are controlling the taxpayers' resources in research and development are not the outstanding people coming from the top of the "log-normal" distribution curve, who might make most creative use of the resources that they influence. Instead, they may all too frequently represent mediocrity that has risen to the top by default. The cost to technical progress of their lack of ability must be enormous if this unfortunate situation actually does exist.

It is to be hoped that an adequate study may be carried out along the lines discussed, actually comparing industrial and government laboratories and giving a correct and representative picture of the existing situation. In the unlikely event that the fears expressed here are unfounded, this result will help relieve worries on the part of many scientists and engineers. But if, as appears much more likely, the facts are as bad or worse than those conjectured, then the discovery of these facts, it is to be hoped, will provoke the invention of remedial measures which will greatly increase the rate of growth of our national technological resources and real wages of our citizens and people throughout the world.

In a democracy the truth shall set us free. Let us hope that objective discoveries in the problem areas of growth in California and research and development management in government will be found. They will help set us free to enjoy a more rewarding existence.

NATIONAL BUREAU OF STANDARDS SPECIAL PUBLICATION 388. PROCEEDINGS OF CONFERENCE ON THE PUBLIC NEED AND THE ROLE OF THE INVENTOR, JUNE 11-14, 1973, MONTEREY, CALIF. (Issued MAY 1974)

The Invention of the Transistor—"An Example of Creative-Failure Methodology"*

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Presents a general historical perspective on the invention of the transistor, from the standpoint of the patents issued and the men involved in the inventions.

Five applications for patents on transistor devices were filed prior to the first public announcement of the transistor on June 30, 1948. The development of the essential inventive ideas for these five patents is described.

Several attempts to make semiconductor amplifiers failed. These failures were used creatively by the team involved—an example of effective research, of which one of the principal elements is the "will to think." The day-to-day development of important ideas and the interaction between them are presented.

The story is continued through the invention and realization of the junction transistor.

Key words: Creative-failure methodology; invention of transistor; junction transistor; patents; semiconductor amplifiers; will-to-think.

1. Introduction

1.1. Three Men and Five Patents

The invention of the transistor can be described from many different viewpoints. I shall use several in this lecture. For the framework upon which to assemble and interrelate the many specific incidents that form the pattern of the story, I have chosen one of the fundamental features of inventions in present-day society—patents.

Five applications for patents on transistor devices were filed prior to the first public announcement of the transistor on June 30, 1948. I shall describe how the essential inventive ideas for these five patents developed. In the story I shall use these five inventions as landmarks on the path to creativity. Thus from the point of view of specific technical contributions, the story of the invention of the transistor might be appropriately called a tale of five patents.

But in justice the story might also be told as a tale of many men. An attempt to partition appropriate credit to every individual whose contributions were significant to the final achievements is an impossible task for at least two reasons: too many individuals were involved and satisfactory criteria for judging the relative merit of the contributions do not exist. Therefore I shall follow the decision of the Nobel committee that awarded the physics prize for 1956 jointly to John Bardeen, Walter Housner Brattain and me. Furthermore, we are the three who did contribute what has proved to be of most enduring value in the five patents.

The social consequences of the application of the transistor invention are, of course, what is really of most significance. However, they are not a part of the story of the invention. There is, however, another significant aspect that it is appropriate to discuss here—the aspect that is part of the title of this lecture—"Creative-Failure Methodology." The transistor was not invented until after several attempts to make semiconductor amplifiers had failed. How these failures were used creatively by the team involved and under the stimulating conditions for research at Bell Laboratories is an important feature of the history. Several phrases give meaningful insights about effective research. Most important, and most difficult to create, is "the will to think"—the theme that runs through "the magic month" when the point-contact transistor was invented and the following 5 weeks that led to the invention of minority carrier injection in the junction transistor.

The relationship of patents to motivation has a long history in the United States. The key concept was embodied in the U.S. Constitution in section 8, Powers of Congress. The first power delegated to Congress concerns taxes. The relevant power is the eighth. It concerns patents and reads:

"The Congress shall have power to promote the progress of science and useful arts, by securing for limited times to authors and inventors the exclusive right to their respective writings and discoveries."

This "exclusive rights" paragraph has, indeed, contributed forcefully to our Nation's technological progress. I regard it as an outstanding example of the practical conservative wisdom of the framers of the Constitution. It draws into the service of the Nation, in addition to those idealistic or compulsive inventors whose motivations are either altruistic or instinctive, also those profit-motivated inventors who would regard it as naively improvident to fail to realize gains from the fruits of their efforts. The patent law, by offering the competitive advantage of legal protection of exclusive rights, appeals even to selfish motivations and induces those who would otherwise be as secretive as possible about their discoveries to share their knowledge and contribute to the progress of others—even to the progress of potential competitors. To be granted the patent that ensures the exclusive rights, the inventor is required to make his discoveries available in understandable written form in the specification of his patent application. Furthermore, his teaching must stand the test of being adequate for one versed in the related practical art to be able to achieve the results claimed in the invention.

A closely related aspect of the patent law is frequently misunderstood by young workers in industrial research laboratories. Many young scientists resent being restrained by their organizations from telling the world about their accomplishments until after patents have been filed. They come to the erroneous conclusion that it is the patent system that prevents them from claiming scientific recognition for their work. Actually, the opposite is true. If it were not for the rights that their organizations acquire by obtaining patents, then the effective way for the organizations to receive returns from investments in research would be to maintain secrecy. In other words the patent system permits, rather than prevents, publications.

A vital input to the preparation of this contribution came as a by-product of procedures at Bell Laboratories designed to optimize the advantages offered by the "exclusive rights" paragraph in the Powers of Congress. In determining which of two competing inventors should be granted the patent, priority of conception and diligence are weighted heavily. The date of conception of the invention is usually established by the record of when it was clearly disclosed to and understood by a coworker. Usually such endorsements by witnesses are made on the pages of laboratory notebooks. These records are of vital importance in establishing facts in patent litigation. Consequently, information on the issuance and status of such notebooks is carefully maintained in organizations like Bell Laboratories.

These carefully preserved records were essential in the research that I undertook 25 years after the birth of the transistor that led me to identify the "magic month" and to reconstruct the sequence of events and their interactions during that period. This research is discussed in detail in sections 3 and 4.

1.2. Can Creativity Be Taught?

One of my principal purposes in selecting the format for this presentation is to choose one that might encourage creativity on the part of the readers. A basic truth that the history of the invention of the transistor reveals is that the foundations of transistor electronics were created by making errors and following hunches that failed to give what was expected. Pure inspiration contributed less to progress than did perseverance and the willingness to try again after an experiment or an idea had failed to deliver the wanted result—evidence for the truth of the saying that "inspiration is ninety percent perspiration." "Creative-Failure Methodology" is a phrase designed to characterize the importance of making constructive responses to failures and errors. The emphasis on this aspect of transistor history in my presentation is intended to stimulate the creativity of receptive readers who may learn from our experience that failures need not be accepted simply as causes for low self-esteem but can instead be recognized as opportunities to learn so that these failures become stepping stones on the path to creativity.

Can creativity be taught by experiences that illustrate how progress can be made by floundering around while looking for the hunch that will produce a breakthrough? My experience in some educational experiments makes me believe that creativity can be enhanced by teaching students to live more comfortably with their own limitations. This can be accomplished through experiences involving searching unsuccessfully for hidden key attributes in problem situations that they then do discover largely on their own and thus learn that persistence does pay off.

The teaching method uses a diagram called the creative search pattern.[1] This is illustrated in figure 1. In general it is not intended to be a map to tell how to do research but instead to understand the natural blundering process of finding one's way when unfamiliar material is dealt with. Thus, it indicates that a number of poor hunches will occur that serve to increase one's familiarity with the subject matter involved. If one has stored in his memory a collection of patterns that are logically similar to the one that is finally found in the problem, then a payoff hunch is likely to occur in which the benefits of the previous experience are transferred to the new situation—a relationship established by associative memory.

This type of teaching has been tried experimentally in high school, undergraduate college situations and in my own experience with graduate students and in my own research. The result of an experiment involving 3 successive years of Stanford freshmen students is shown in figure 2. The data there was gathered 4 years after the first teaching experiment occurred. The students who entered by freshman seminar entitled "Mental Tools for Scientific Think-

[1] For details see "Mechanics" by William Shockley and Walter A. Gong. (Charles E. Merrill Books, Inc., Columbus, Ohio, 1966).

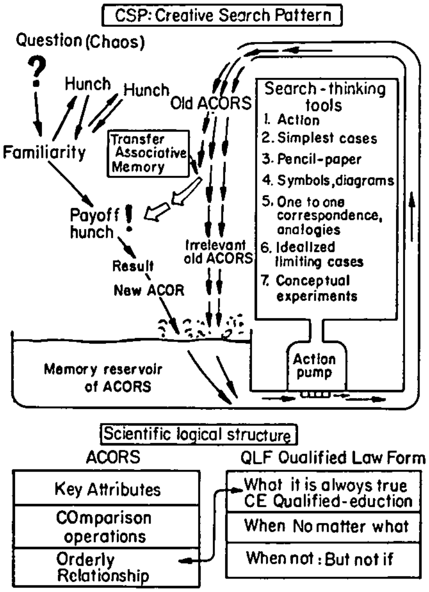


FIGURE 1. *The Creative Search Pattern (CSP), including seven search-thinking tools, and the scientific logical structure based on ACOR, QLF and QE that are four basic science-thinking tools.*

The hunches that do not pay off are examples of creative-failure methodology.

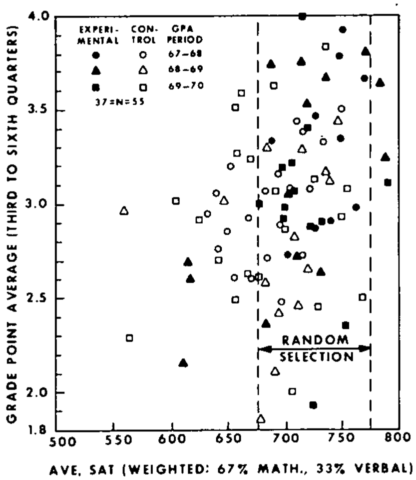


FIGURE 2. *Results of a controlled experiment on randomly selected applicants for a Stanford University freshman seminar on mental tools for scientific thinking.*

In the four quarters subsequent to the two in which the seminar was taken, the "experimental" students outperformed the controls at a significance level of better than 0·05.

ing" took the seminar for the first two quarters of their freshman year. The seminar was over-subscribed and I selected the experimental students who took it by constructing matched groups of students, based on their scholastic aptitude tests for verbal and mathematical ability (with math rated twice as heavily as verbal). From these matched groups I selected by chance approximately half of the students in the range of scores shown on the figure. The follow-up study of the effectiveness of the seminar on the students' intellectual performance consisted of determining the cumulative grade point average for the following four quarters, comprising the final quarter of the freshman year and the three quarters of the sophomore year. Inspection of figure 2 shows that on the average, students who took the seminar obtained better grades for the following four quarters than those who did not although both groups were matched in terms of their potential when they entered Stanford and their interest in studying my seminar. The result is significant in the sense that if the points shown in the random selection region of figure 2 had been assigned randomly to the experimental and control group, then there is only about 1 chance in 20 that the experimental group would have ended up as much ahead as they actually did.

Somewhat similar results were obtained with ninth grade students and with freshmen students at San Jose State College. It is on the basis of this background that I hope a description of the failures and confusions that went into the creating of the transistor will contribute towards the creativity of readers of this article.

1.3. Format of This Presentation

This presentation is organized by presenting in section 2 a general historical perspective on the invention of the transistor starting with some of the factors that motivated the workers as early as 1939, 8 years before the invention of the transistor. This treatment hits the high points and does not delve deeply into the interactions that occurred among the three men and led to the five patents. For convenience in following this discussion, table 1 presents certain key information on the five patents. (The reader with sophistication may consider a table with at least two and perhaps four patents for transistors to be an inconsistency in an article hav-

TABLE 1. *The Five Transistor Device Patents filed before the Public Announcement on 30 June 1947*

No.	Patent dates		Inventors [c]	Invention [d]	Conception	Reduction to practice	Div. C.I.P.
	Filed	Issued [b]					
1........	26 Feb 48	3 Oct 50	WHB RBG	Electrolyte F.E.T....................	20 Nov 47	21 Nov 47	0
2........	26 Feb 48	3 Oct 50	JB	Inver. Layer IGFET................	23 Nov 47	?	0
3........	26 Feb 48	17 Jul 51	RBG	Elect-Form Inver. Layer...........	Dec 47 ?	Dec 47 ?	0
4........	17 Jun 48 [a]	3 Oct 50	JB WHB	Point-Contact Transistor...........	15 Dec 47	23 Dec 47	3
5........	26 Jun 48	25 Sep 51	WS	Junction Transistor..................	23 Jan 48 [e]	Apr 50 [e]	3 [f]

[a] Originally filed 26 Feb 48; abandoned and refiled to include current gain at collector.

[b] Patent numbers: 2,524,034; 2,524,033; 2,560,792; 2,524,035; 2,569,347.

[c] John Bardeen, Walter Houser Brattain, Robert Bernard Gibney, William (Bradford) Shockley.

[d] Modern terminology is used: IGFET = insulated-gate, field-effect transistor. (1) is inversion-layer channel FET with electrolyte gate. (2) is IGFET with inversion layer channel. (3) is electrolytic processing to form inversion layer. (4) and (5) are the basic point contact and junction transistor patents.

[e] The dates are late November 1947 for the $p-n$ junction with drop of electrolyte, see discussion of Claim 29 under 4 Dec 47 in the "Magic Month," subsection III A.

[f] Includes "divisions" and "continuations in part" of these patent applications.

[g] One C.I.P. on negative resistance from transit time, conceived on 24 Jan 48, had in turn two additional C.I.P.'s.

ing in its title the phrase "*the* invention of *the* transistor." Perhaps it was a compromise. Consider an alternative attempt to be more precise such as "the inventions of the point-contact and the junction transistor together with several precursors.")

Table 2 contains a listing of amplifying principles that had been conceived prior to the public announcement of the transistor. These are discussed in the following sections.

TABLE 2. *Amplification concepts conceived before public announcement on 30 June 1948*

Field Effect
Schottky-Barrier and Depletion '39
Insulated-Gate Thin Layer ('32?) '45
Inversion-Layer Electrolyte '47
Inversion-Layer Insulated Gate '47
Junction Field-Effect Transistor '47
Point-contact collector current gain '47–('48)
voltage gain from reverse bias p–n junction '47
injection across p–n junction '48
negative resistance from transit time-effects '48

New research was carried out in preparing this lecture. In September of 1972, about 2 months earlier than the 25th Anniversary of the invention of the transistor, I undertook research on just what had gone on at the time of the invention of the transistor. Probably no one had previously taken all of the laboratory notebooks of the three men principally involved and of their colleagues and endeavored to trace out the day by day development of important ideas and the interactions between them. The details of this history have been organized in section 3 entitled Creative-Failure Methodology and "The Will to Think," the section that follows the historical perspective.

Section 4 extends into 1951 so as to cover the conversion of the junction transistor from a patent application to the device that launched the solid-state era about 1951.

2. Historical Perspective on the Invention of the Transistor

2.1. Pre-World War II

One of the most important motivations in the entire transistor program at Bell Telephone Laboratories was imparted to me by Mervin J. Kelly shortly after he had been promoted to Director of Research from his position as Head of the Vacuum Tube Development Department. My personal contact with Kelly began in 1936, when he recruited me as a fresh Ph. D. from MIT to work with C. J. Davisson in the Physical Research Department. Kelly arranged for me to have an initial indoctrination experience with high frequency vacuum tubes. This included spending some months of 1937 in the Vacuum Tube Department. During that time Kelly gave me an eloquent pep talk—one that had a long-lasting influence on my own motivations. He pointed out that relays in telephone exchanges caused problems and were expensive to maintain. He felt that electronics should contribute to telephone exchanges in addition to making long distance transmission possible.

After I left the Vacuum Tube Department and returned to Physical Research, I kept Kelly's objective in mind. The move put me in contact with Walter Brattain and his interest with copper oxide rectifiers and with the theories of their mechanisms of rectification then recently developed. On the 29th of December 1939 I wrote a disclosure of what in principle was a sound concept of a semiconductor amplifier. The leisurely time scale of the ensuing events is interesting to compare with the accelerated developments that occurred 8 years later during the "magic month" discussed in the next section. Research on my notebook entries show that experiments based on the 1939 disclosure were carried out before 6 February 1940. However, my disclosure waited nearly 2 months, until 27 February 1940, before it was witnessed by J. A. Becker, Walter Brattain's supervisor. Two days later, on leap year day of 1940, Walter Brattain and I both signed a modification of the earlier disclosure. This disclosure, shown in figure 3, shows a more or less standard copper oxide varistor unit with two lines of metal forming electrodes on the surface of the oxide. It would today be called a Schottky-barrier, field-effect transistor.

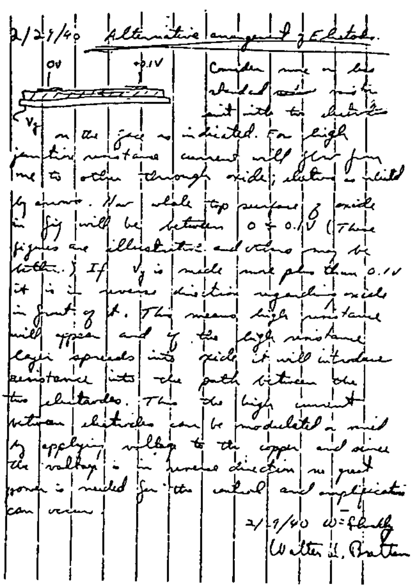


FIGURE 3. *The disclosure of a theoretically-sound, Schottky-barrier, field-effect transistor, signed by W. Shockley and W. H. Brattain on 29 February 1940.*

It was prophetic of developments that were to come 20 years later as parts of integrated circuits using field-effect transistors.

The early results that Brattain and I obtained in experiments related to these disclosures were not encouraging. Shortly after, we quite willingly responded to calls to apply ourselves to different areas of research and development related to America's entry into World War II. Eight years elapsed after the 1940 disclosure before significant field effects in copper oxide were achieved. These 1948 results were reported in one of the three letters to the editor in the Physical Review published simultaneously with the public announcement of the transistor by Bell Telephone Laboratories.

Perspective on the hiatus in research on semiconductor physics is clearly presented in laboratory notebook 18194 which Walter Brattain used both before and after his period of war related activities. His last prewar entry on page 39 is dated 7 November 1941—one month before Japan was to bomb Pearl Harbor. A gap of more than four and one half years elapsed before Brattain made his next entry on 23 April 1946. Page 40 starts with one sentence: "The war is over." The remainder of page 40 describes an experiment with a point contact on germanium: "All points rectify n-type." It was a prophetic combination of structure and semiconductor. Eighteen months later on 16 December 1947 on page 193 of the same notebook Brattain was to report that the addition of a second gold point contact on a piece of n-type germanium achieved power gain.

In 1945, during the last months of the war, Kelly once again took actions that indirectly contributed to the transistor program. He invited me to make a series of return visits from the Pentagon back to Bell Telephone Laboratories. I returned full time immediately after the explosion of the atomic bombs that ended the war with Japan.

Kelly's invitation to visit was preparation for my post war position at Bell Telephone Laboratories as Co-Supervisor of a solid-state physics research group. A visit to the Laboratories' Holmdel facility had reawakened my interest in semiconductor amplifiers. At Holmdel Kelly had arranged a demonstration of a radio set lacking vacuum tubes. In this radio the amplification was accomplished by point-contact detectors. These semiconductor devices acted as negative resistances, a phenomenon probably due to heating effects that lowered their resistance as occurs for thermistors. These devices indicated that semiconductors held exciting potentials but they themselves had many shortcomings—so many that they were failures in pointing the way to make semiconductor amplifiers—but creative-failures in stimulating the will to think of better approaches.

During my visits in 1945, a series of ideas led me again to the concept of a field-effect amplifier. I do not now recall, or for that matter did the research that I did on my laboratory notebooks while preparing this paper suggest, that in 1945 I saw clearly how closely these field effect ideas were related to the copper-oxide concepts of 1939 and 1940. The sequence of ideas in 1945 had a different starting point than the Schottky barrier of 1939.

The basic field-effect concept of 1945 is shown in figure 4. Here a thin layer of n-type semiconductor is represented. This layer forms one plate of a paral-

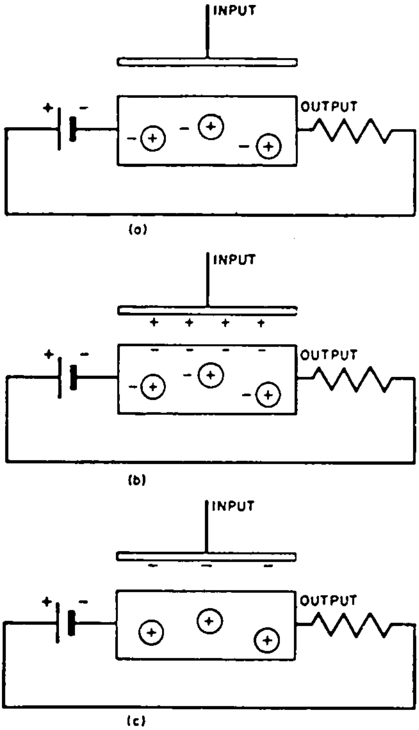


FIGURE 4. *The theory of a field-effect transistor using a thin layer of semi-conductor: (a) The structure of the transistor with no control voltage applied. (b) The situation prevailing when a positive charge is placed on a control plate so as to increase the conductance of the semiconductor. (c) The situation when a negative charge is put on the capacitor plate so as to reduce the conductance of the semiconductor.*

lel plate capacitor, the other plate being a sheet of metal. Charging the capacitor alters the number of electrons on the semiconductor. This modulates the conductance of the layer much as did the reverse bias on the copper plate in the concept of figure 3.

A number of experiments on the field-effect structure of figure 4 were undertaken. These had become possible in 1945 because of the great advances in semiconductor technology that had taken place in point-contact, or "cat's-whisker," radar detectors during World War II. The work at Holmdel had played a key role in this and so had also the metallurgical work at the Murray Hill Laboratory. Silicon and germanium, both elements of the fourth column of the periodic table, had become two of the technologically best-controlled semiconductors in existence. Methods of using impurities from the third and fifth columns of the periodic table to act as acceptors and donors had been developed. The designations p-type and n-type were in common use. Compensation of donors and acceptors had been used at Murray Hill to control or adjust resistivity and, indeed, had been patented by J. H. Scaff and H. C. Theuerer. A particularly important form of high purity, n-type germanium had been developed. In point-contact rectifiers it would stand high reverse voltages, also called "back" voltages, and was referred to as "high back-voltage germanium." This germanium was to play a vital role in the point-contact transistor of patent 4 in table 1.

Thin films of silicon that had been deposited, largely by the work of Gordon Teal, seemed ideally adapted to field-effect experiments. My calculations showed that very substantial modulation of the resistance should occur. None was observed. On 23 June 1945, I wrote that the effects were at least 1,500 times smaller than what I predicted should have been observable.

I brought my calculations to the attention of John Bardeen. In October of 1945, he had joined the Laboratories as a member of the solid-state group. He was a theoretical physicist with a background in solid-state physics founded on research carried out while he was obtaining his Ph. D. under Professor Eugene Wigner of Princeton University. When I did research for the 25th Anniversary of the transistor, I found an entry in Bardeen's laboratory notebook recording my request to him to check my calculations. On November 7, 1945, 2 weeks later, he verified my formula.

2.2. Scientific Aspects of the Failure of the Field-Effect

Attempts to observe the field-effect and use it for amplification failed during 1945 and early 1946. But this failure became a stepping stone on the path to creativity. On 19 March 1946 John Bardeen recorded in his notebook his explanation for the failure: the electrons drawn to the surface of the semiconductor, when it was negatively charged as represented in figure 3, were not free to move as were the electrons in the interior. Instead, they were trapped in surface states, as represented in figure 5, so that they were immobile. Thus, in effect, the surface states trapped the induced charge of electrons and thereby shielded the interior of the semiconductor from the influence of the positively charged control plate. The externally applied field was thus blocked at the surface and did not penetrate.

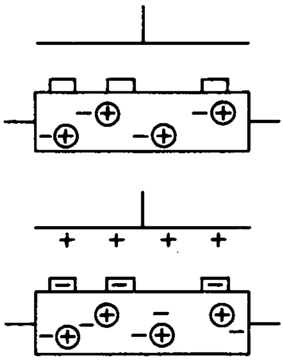


FIGURE 5. *Bardeen's explanation of the failure of the field effect structure through the presence of surface states.*

(a) The structure with no applied voltage. (b) The capture in surface states of the electrons induced by the surface charge.

"Creative-Failure Methodology" is the phrase that I formulated some two decades later to describe what we intuitively put into action when frustrated by the failure of the field-effect experiments. Bardeen's concept of surface states as shielding the interior from external fields gave a practical significance to what had before been largely a theoretical concept. Both Igor Tamm in Russia (co-winner of the 1958 Nobel prize in physics for work on Cerenkov radiation) and I had done theoretical calculations showing that surface states should exist. However, no significant observable implications of these had been proposed. Indeed our surface states were of a highly mathematical nature and related to perfect crystalline surfaces. In contrast, Bardeen's concept of surface states was more empirical. His states were undefined in terms of their quantum mechanical origin and allowed for the possibility that defects on the surface might contribute to their existence—concepts in harmony with knowledge about the states due to donors and acceptors in the interior of the semiconductor.

Bardeen quickly recognized the value of his proposal. His surface states had broad implications and their use was not restricted solely to explaining the field-effect failure. His surface states also resolved a number of mysteries about semiconductor surfaces including their rectifying characteristics when contacted either by metal points or else by other semiconductors—a dramatic example of creative-failure methodology in action.

Our semiconductor research team abandoned efforts to make a field-effect transistor and instead emphasized research on new science related to Bardeen's surface states. The course of action that we intuitively put into action is what I have subsequently analyzed as an effective feature of creative-failure methodology valuable for research related to applications:

I have referred to this creative principle as *respect for the scientific aspects of practical problems.* Figure 6 represents how this principle works in general with the interpretation of its vital role in creating the transistor indicated by the phrases in parentheses: The attempt to make a semiconductor

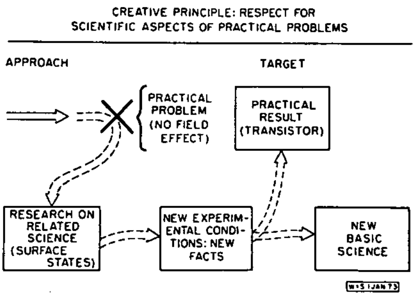


FIGURE 6. *Respect for the scientific aspects of practical problems—a feature of the creative failure methodology which led to the invention of the transistor.*

amplifier was blocked by a practical problem. But how we responded to this failure to reach the practical result was creative. The failure stimulated Bardeen's creative suggestion of the surface states. Our research on the science related to the surface states led to new experimental conditions and to new observations. As discussed below under the "magic month" heading of section 3, the new experimental conditions produced a particularly revealing observation that suggested that at long last the blocking effect of the surface states had been overcome. This new possibility motivated "the will to think" phenomenon that led to the phenomenal degree of creativity that followed immediately thereafter.

There may be today, 25 years after the invention of the transistor, an ironic aspect of the emphasis that I have given to "respect for the scientific aspects of practical problems" as an important creative principle in industrial research. By assigning so much emphasis to this feature of creative-failure methodology—a feature that I helped to establish—I may have become out-dated and be reflecting attitudes that are more appropriate to the experiences that I recall of the 1940's than they are to industrial research today.

In 1946 when the semiconductor research group focussed emphasis on the basic science, I recall that leaders of some other groups of the Research Department suggested, emphatically, that we should put more stress on practical semiconductor problems—those related closely to the difficulties in the telephone plant. Our group was of one mind and we followed the wise course of working, not upon such practical but messy semiconductors as selenium, copper oxide and nickel oxide, but instead on the best understood semiconductors of all—silicon and germanium. For these semiconductors, not all of the theoretical concepts, developed largely during World War II, had been experimentally verified; accordingly, we elected to concentrate upon the resulting gaps in this branch of science, among them the recently proposed surface states. We felt that it was better to understand these two simplest, elemental semiconductors in depth rather than to attempt to add piecemeal contributions to a variety of other materials. Thus we assigned highest priority to the primarily scientific aspects. But in our selection of emphasis, we did choose those scientific aspects that were related to the problems that blocked our approach to the long-range, practical goal—the creation of a semiconductor amplifier, later to be called the transistor.

My feeling of being out of date occurred after I had stressed the creative principle of "respect for scientific aspects of practical problems" at Bell Laboratories during a rehearsal in February 1973 of a lecture version of this presentation. During a subsequent luncheon conference, a young scientist told me that he was puzzled about my reasons for according such emphasis to the "respect for the scientific aspects of practical problems." The approach that I endorsed so vehemently seemed so natural to him that it scarcely called for any emphasis at all. I felt separated from his appraisal by a "generation gap": What in 1947 had been, in the eyes of at least some colleagues, a pioneering advance from the Edisonian methods of trial and error to achieve practical goals was now, 25 years later, to be taken for granted. And the transistor story had probably helped to bring this about. By giving such a strong sales pitch to what today did not need selling, I had made my words become an echo of the past.

2.3. The Invention of the Point-Contact Transistor

The research on silicon and germanium and their surface states led to a series of experiments. As described on figure 6, these created new experimental conditions and led to the discovery of new scientific facts. A detailed description and interpretation of the sequence of events is the subject of section 3. As will be described there, a period that I have called "the magic month" began with a breakthrough observation on 17 November 1947 and culminated with what I shall take as the starting point here—the reduction to practice of the point-contact transistor—an achievement recorded, as discussed above, on page 193 of Brattain's notebook. The famous prototype structure of the point-contact transistor came into existence on 16 December 1947—the date at which Walter Brattain used a wedge-shaped piece of plastic to press two narrowly-spaced, parallel-line, gold contacts against a block of high-back voltage germanium. This prototype is shown in figure 7. The observation that it amplified and oscillated is recorded in that very famous notebook entry—the one written by Walter Brattain in 1947 on the day before Christmas. It appears in figure 8. The date of 23 December cited in figure 8

FIGURE 7. *The original point-contact transistor structure comprising the plate of* n-*type germanium and two line-contacts of gold supported on a plastic wedge.*

and shown as the date of "reduction to practice" in table 1 is the generally accepted date for the birth of the transistor. However, 15 and 16 December 1947, are equally significant because, as discussed under those dates in section 3, these are the dates of conception and of first successful amplification. Actually, amplification was obtained even earlier with semiconductor devices using electrolytes. But these earlier devices had such poor frequency response that they failed to hold promise of developing into useful amplifiers. But they were creative-failures and did directly stimulate the research that led to the point-contact transistor. One such device was covered by the Brattain-Gibney invention, patent 1 of table 1. Another was incorporated in patent 5, the junction transistor.

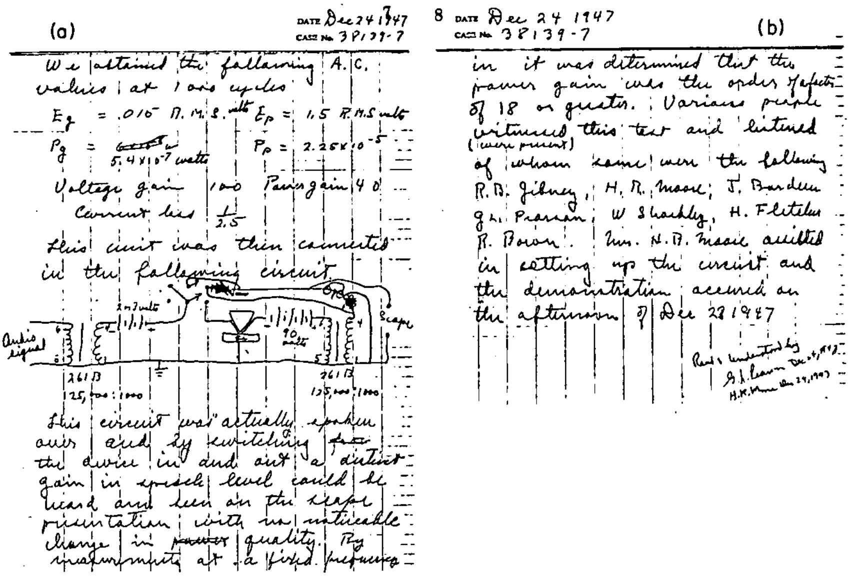
The point-contact transistor, patent 4 of table 1 was originally filed, not on 17 June 1948 as shown in table 1, but on February 26, 1948, simultaneously with patents 1, 2 and 3. The issued version of patent 4 states in its first paragraph that it is a continuation-in-part of the earlier application that had actually been abandoned. The reason for abandoning and refiling (I was told by R. J. Guenther shortly before he retired as General Patent Attorney almost exactly on the date of the 25th Anniversary of the transistor) was that the original filing failed to mention current gain at the collector—an important feature in obtaining power gain and in simplifying the design of circuits. (Current gain is covered in the issued patent in col. 10, line 35 and col. 16, line 26.)

Patent 3, of table 1, involved treatments of semiconductor surfaces electrolytically—concepts important to the theory of the point-contact transistor. Both versions of patent 4 depended heavily upon the concept of an inversion layer on the semiconductor surface caused by surface charges, related to Bardeen's surface states. The most appropriate surface conditions for these charges were produced by electrolytic treatments, specifically those treatments that were the subject of patent number 3 of table 1; indeed an example of the treatment specified in patent 3 is described in detail in a paragraph of the point contact transistor patent (col. 8, lines 8 to 44).

The concept of surface inversion layers was an essential stepping stone along the path of development of transistor technology. It was also prophetic. As discussed further below, in actuality inversion layers were probably not important for the point-contact transistors in pilot production in 1948 nor for the junction transistors or junction field-effect transistors that came somewhat later. However, the surface-state concepts of inversion layers disclosed in patents 2 and 4 are probably now playing an essential role in MOS (metal-oxide-semiconductor) field-effect transistors in integrated circuits.

The importance attributed to the inversion layer in early 1948 may be appreciated by reading the claims of the point-contact transistor patent; for example, claim 1:

"1. A circuit element which comprises a block of semiconductive material at which the body is of one conductivity type and a thin surface layer is of the opposite conductivity type, and an emitter electrode making contact with said layer, a collector or electrode making contact with said layer disposed to collect current spreading from said emitter electrode, and a base electrode making contact with the body of the block."


(a)

DATE Dec 24 1947
CASE No. 38139-7

We obtained the following A.C. values at 1000 cycles

E_g = .015 R.M.S. volts E_p = 1.5 R.M.S. volts

P_g = ~~[illegible]~~ 5.4×10^{-7} watts P_p = 2.25×10^{-5}

Voltage gain 100 Power gain 40

Current loss $\frac{1}{2.5}$

This unit was then connected in the following circuit.

This circuit was actually spoken over and by switching ~~[illegible]~~ the device in and out a distinct gain in speech level could be heard and seen on the scope presentation with no noticeable change in ~~power~~ quality. By measurements at a fixed frequency

(b)

8 DATE Dec 24 1947
CASE No. 38139-7

in it was determined that the power gain was the order of a factor of 18 or greater. Various people witnessed this test and listened (were present) of whom some were the following R.B. Gibney, H.R. Moore, J. Bardeen, G.L. Pearson, W. Shockley, H. Fletcher, R. Bown. Mr. H.R. Moore assisted in setting up the circuit and the demonstration occurred on the afternoon of Dec 23 1947

Read & understood by
G.L. Pearson Dec 24, 1947
H.R. Moore Dec 24, 1947

Figure 8. *Notebook entry of 24 December 1947 describing the point-contact transistor demonstration of 23 December 1947 at Bell Telephone Laboratories.*

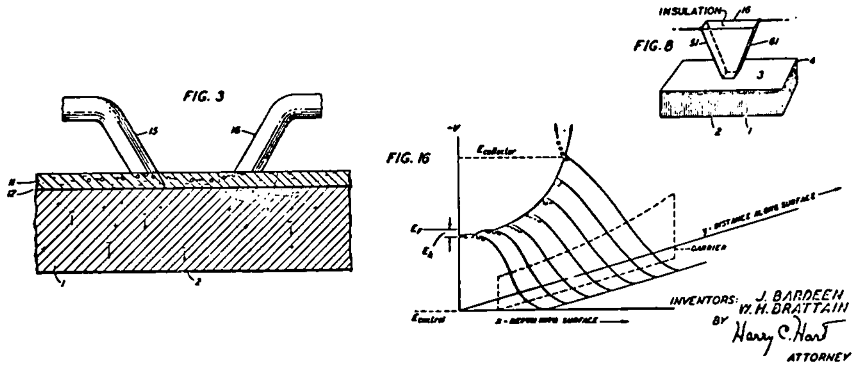


Figure 9. *Patent figures for the point-contact transistor (patent 4 of table 1).*

Figure 8 shows the wedge structure of figure 7. Figure 16 represents the potential profile which traps holes at the surface. The importance of the inversion layer on the surface is emphasized in figure 3 (copied from patent 3 where details are clearer than in the corresponding figure of patent 4).

These concepts are clearly illustrated in figure 9. Figure 3 (taken from patent 3 is almost identical with a more congested figure of patent 4) shows the inversion layer and the emitter and collector electrodes. Figure 8 from patent 4 shows the drawing of the wedge structure discussed for figure 7. Figure 16 from patent 4 shows a potential energy diagram for the p-type channel of the inversion layer, the holes being represented as small circles.

Means of forming the inversion layer were a central topic in patent 3. Indeed, the final claim 9 of the issued patent read directly upon the inversion layer shown in figure 4.

"9. A block of high back voltage germanium material of which the main body is of n-conductivity type, having on a surface thereof a thin layer of the order of 10^{-5} cm in thickness whose characteristics are of the p-type, separated from the body of the block by a high resistance barrier."

Thus it is evident that patent 3 of table 2 contains a claim essential to important general concepts of the first four patents. (The significance of this feature was put in perspective by Bardeen in his Nobel lecture as quoted below, see 9-12 December 1947 in sec. 3.)

The prominence of Gibney's name on the five patents and in the notebooks raised questions in my mind as I did the research for this lecture. What had happened to Gibney? Why had he left? I wondered if lack of recognition in respect to his significant contributions might have provoked him to go away mad during the following 6 months or so. My speculation was unsound. I learned the truth by telephoning to him at Los Alamos. He had indeed left with a dislike for his situation in New Jersey—but not with his situation at Bell Telephone Laboratories. He was mad at the New Jersey weather. The New Jersey winters had contributed to strep throats, colds, and other health problems for all of his family including his children. The problems had been most severe for the preceding winter of 1946-1947. Furthermore, it turned out that both he and Mrs. Gibney, although they had not known this when they married, had years before fallen in love with New Mexico. When in the fall of 1947 Gibney saw an advertisement from the Los Alamos Laboratories seeking a man with his scientific background, he applied. Actually, during part of the magic month, he was at Los Alamos for interviews. Since the spring of 1948 he has been at Los Alamos. For 20 years he was in charge of a group doing physical metallurgy and graphite research.

I had recruited Gibney into the group shortly after it was formed to add otherwise lacking expertise in physical chemistry. The great loss that we suffered when he left for Los Alamos, prior to the announcement of the transistor, was later balanced by the acquisition of Morgan Sparks. Sparks played, as I shall describe in section 4, an essential role in bringing useful junction transistors into existence.

Speculations and Anecdotes Related to the Public Announcement of the Transistor

What was the scientific atmosphere at the time the transistor was announced? What was the press reaction? Would, in any event, the transistor have soon been invented somewhere else? What were some of the key scientific matters left undecided at the time of the announcement? I shall endeavor to put these questions in perspective by describing my own recollections of the events that occurred at approximately the same time—close to the first of July 1948 when news stories of the invention of the transistor first appeared. Some of the important scientific questions concern the role of the inversion layer on the surface and the phenomena associated with the emitter point.

Figure 10 is a chart that was used at the public presentation to describe the region of interaction that surrounds the emitter point. Power gain occurs when the collector point is placed in this region. This diagram suggests how the transistor might have

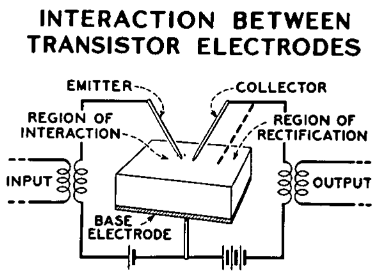


FIGURE 10. *A chart used at the time of the public announcement of the transistor illustrating the concept of the region of interaction surrounding the emitter electrode.*

been discovered elsewhere by a study of the effect upon the voltage and conductance of a probe point when placed in the region of the interaction surrounding another current carrying point. Such research would have led to observing phenomena that, one would think, could easily have suggested the amplification mechanism of the point-contact transistor. That this did perhaps come very near to happening at Purdue University in mid-1948 is suggested by the following facts.

An important motivation to the Bell Laboratories group resulted from competition with the outstanding semiconductor group at Purdue University, directed by Professor Karl Lark-Horowitz. Interest in making semiconductor amplifiers must have existed at Purdue University. The idea that semiconductor amplifiers could probably be realized had already had a long history. (One example is discussed below in connection with table 2.) At a meeting of the American Physical Society in May of 1947 a doctoral candidate at Purdue, Ralph Bray, had undertaken research on what was equivalent to a forward biased emitter point, although not so recognized then. What Bray concluded, and reported in more detail in a paper read in 1948 two months before the announcement of the transistor, was an explanation of how the "spreading resistance" of a point contact depended upon the emitter current. He used large forward currents that produced electric fields of 100 volts per centimeter. Bray concluded that these high electric fields caused the resistivity of the germanium to decrease. ("Hot electron" experiments carried out at Bell Laboratories later showed that high electric fields actually tend to increase the resistance.) It is hard to guess how long it would have taken for Bray or his colleagues at Purdue to discover that the effect that they were analyzing was in actuality the injection of minority carriers into the germanium if transistor related research at Bell Laboratories had not already published the answer in January of 1949.

Before the announcement of the transistor, I had proposed injection as a key feature of the junction transistor, patent 5 of table 1. Experiments, described in more detail in the next section, had also indicated that injection might be an important mechanism for point-contact transistors. Several programs to resolve the question were undertaken of which the best known is the Haynes-Shockley experiment that measures "drift mobility" of injected minority carriers. The first definitive result to be published was submitted to the Physical Review on 1 December 1948 by E. J. Ryder and me and published in the 15 January 1949 issue. Figure 11 shows the essential features. A circular slice of high-back voltage n-type germanium was cut into the shape of a piece of pie and two metal electrodes were applied, one at the point and the other on the crust, or rim. When we applied a flat-topped voltage pulse across these electrodes with the narrow point negative, we observed a flat-topped current pulse of just the magnitude expected from the specimen's low-voltage resistance. There was no change in resistance although our fields were more than 10 times larger than those Bray had used. However, when we reversed the voltage and made the narrow end positive, we did observe a decreasing resistance and a current pulse that increased with time. By assuming that holes were injected from the positive electrode we interpreted these observations as follows. When the positive electrode was the narrow end, then the high current density caused injection of holes into this restricted region and thereby produced a substantial reduction of the resistance of the specimen with a resultant increase of current. On the other hand, when the large end was positive, the efficiency of hole injection was less, because the current density was less, and the holes injected were injected into a region that contributed much less to the resistance of the specimen so that the increase in current was negligible throughout the duration of the voltage pulse.

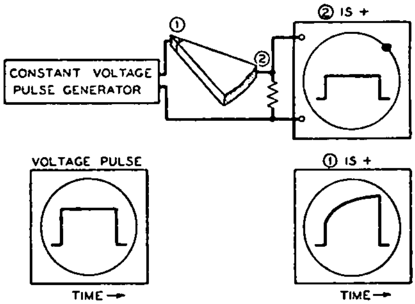


FIGURE 11. *The Ryder-Shockley experiment demonstrating conductivity modulation by hole injection at a positively biased electrode.*

As I pointed out above, this experiment was closely related to the injection concept of the junction transistor. My historical research into photographic files unearthed the presentation of injection shown in figure 12. I speculate that the date of this figure was probably in 1949 or early 1950—a conclusion based on the somewhat wistful expression on my face. During that period the theory of the junction transistor was well developed but the goal of producing one with desirable characteristics was frustratingly out of reach. I was photographed elucidating junction-transistor theory on a blackboard diagram. The proportionality at which I was pointing suggested that the unwanted current of carriers from the base layer to the emitter should vary inversely as the conductivity of the emitter layer.

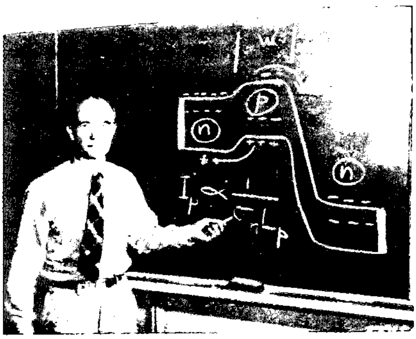


FIGURE 12. *The author presenting the theory of the junction transistor probably in 1949 or early 1950.*

The concept of injection across an emitter junction completed the invention of the junction transistor of patent 5 in table 1 and added one more item to the list of table 2, Amplification Concepts Conceived Before Public Announcement on 30 June 1948. The specification for the application for patent 5 also contained a discussion of negative resistance arising from transit time effects—a consequence of familiarity that I had acquired during the initial portion of the indoctrination period arranged by Dr. Kelly when I was assigned to work with Dr. F. B. Llewellyn. This transit time disclosure was later developed into a continuation in part of patent 5 that resulted finally, as shown in table 1, in three additional issued patents.

In all cases, save one, the dates in table 2 of conception are established within narrow limits from records in laboratory notebooks. The insulated-gate, thin-layer, field-effect concept may have been a significant aspect of the Lilienfeld patents that issued in 1930, 1932, and 1933. Two dates are shown for collector current gain in the point contact transistor. Although gain may have been achieved in 1947, it was not clearly recognized in the 26 February 1948 filing of patent 4. It was, however, clearly expressed in the continuation-in-part filed on 17 June 1948, less than 2 weeks before the public announcement of the transistor.

Do these observations cast any light on the question of when the transistor would have been invented had it not happened at Bell Telephone Laboratories? Sometimes a long time may elapse before a vigorous effort is mounted to overcome an obstacle that blocks progress. However, once "the will to think" is motivated, as discussed in section 3, progress accelerates enormously. In the case of the amplifying principle of injection across a p-n emitter junction, such motivation would have occurred soon at Bell Laboratories if I had not invented injection on the 23d of January 1948. This reasoning is presented in detail in the junction-transistor portion of section 3.

An example of how the existence of established possibilities upon motivations accelerates accomplishments is illustrated by the success of thin film, field-effect experiments like those whose failure was used by Bardeen as a stepping stone to his theory of surface states. Shortly after Bardeen's theoretical proposal in 1946, very feeble field effects were observed by G. L. Pearson at the low temperatures of liquid nitrogen. But room temperature results remained negligible. However, once semiconductor amplification had been achieved, then after only a few months, substantial field effects were observed by Pearson at room temperature, using structures essentially of the form of figure 4. Pearson and I reported on them in one of the first three publications on transistor effects. These publications appeared in the Physical Review and the public announcement was scheduled to coincide with the date of their publication.

Further evidence that the invention of the transistor might have been inevitable and not much longer in coming is illustrated by the following personal anecdote about an experience at the Naval

Research Laboratory. The incident occurred during the week at the end of June 1948 between a private transistor demonstration for members of the Department of Defense and the subsequent public presentation to the press. Ralph Bown, then the Director of Research, and I responded to an invitation to discuss at the Naval Research Laboratory in Washington their proposal that the public announcement of the transistor might be made jointly with them. One of their scientists had been carrying out experiments aimed at producing a semiconductor amplifier. This project was, they thought, far enough along so that these developments should also be recognized. After Bown and I had discussed with them the new scientific interpretations, based largely on the theory of inversion layers, and had stressed to them the fact that their program had not actually achieved power gain, they gracefully withdrew their suggestion. This story shows that other competent laboratories were actively in pursuit of the same goal.

The initial impact of the public announcement of the transistor was disappointing to those of us who were most intimately involved. Figure 13 is representative of the exhibits at the press conference. It shows two typical vacuum tubes compared with a packaged, point-contact transistor. The New York Times report of the press conference was discouraging to us. It occupied four paragraphs of the News and Radio column on the radio page of their July 1st issue:

"A device called the transistor, which has several applications in radio where a vacuum tube ordinarily is employed, was demonstrated for the first time yesterday at Bell Telephone Laboratories, 463 West Street, where it was invented.

"The device was demonstrated in a radio receiver, which contained none of the conventional tubes. It also was shown in a telephone system and in a television unit controlled by a receiver on a lower floor. In each case the transistor was employed as an amplifier, although it is claimed that it also can be used as an oscillator in that it will create and send radio waves.

"In the shape of a small metal cylinder about half an inch long, the transistor contains no vacuum, grid, plate or glass envelope to keep the air away. Its action is instantaneous, there being no warm-up

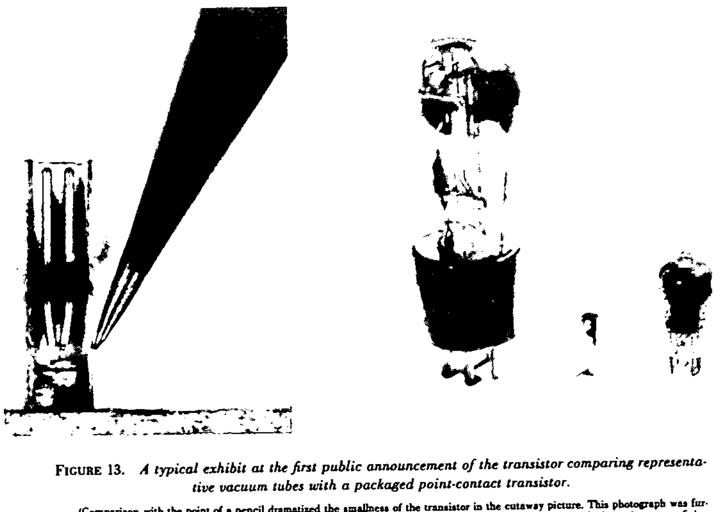


FIGURE 13. *A typical exhibit at the first public announcement of the transistor comparing representative vacuum tubes with a packaged point-contact transistor.*

(Comparison with the point of a pencil dramatized the smallness of the transistor in the cutaway picture. This photograph was furnished by W. G. Pfann whose group made the transistors used in the demonstration. Pfann later achieved fame by inventing zone refining and applying it to germanium—the process that achieved unprecedented chemical purity of crystals.)

delay since no heat is developed as in a vacuum tube.

"The working parts of the device consist solely of two fine wires that run down to a pinhead of solid semiconductive material soldered to a metal base. The substance on the metal base amplifies the current carried to it by one wire and the other wire carries away the amplified current."

The emphasis on "no warm-up delay" does accurately describe the atmosphere at the time. During the next 5 years lecturers who demonstrated transistors found that the almost instantaneous emergence of sound from the loudspeaker after a transistor radio was first turned on usually brought a gasp from the audience who were used to waiting for warm-up times of about 1 minute before a vacuum-tube radio would go into action.

3. The "Magic Month" and the Birth of the Point-Contact Transistor

3.1. "The Will to Think" and Creative-Failure Methodology

"The will to think" is a phrase that properly describes the actions of the semiconductor research group at Bell Telephone Laboratories after a crucial new fact was observed on the 17th of November 1947. I first heard the phrase "the will to think" from Professor Enrico Fermi in 1940 when he was researching atomic energy. Several of us at Bell Telephone Laboratories were also exploring the possible military significance of nuclear fission in response to a request from the National Academy of Sciences. In the course of our research, James B. Fisk (who moved from President to Chairman of the Board of Bell Laboratories on almost exactly the date of the 25th Anniversary of the transistor) and I had invented the "lumped" moderator principle for producing chain reactions and had been calculating optimum dimensions for the periodic structures of nuclear reactors. Fermi's group was undertaking similar investigations at Columbia University. Neither group knew of the other's invention. During the conference when I first heard Fermi use "the will to think" phrase, neither he nor I mentioned the possibility of using "lumps" or uranium in a "pile."

Fermi was designing experiments to study the slowing down of neutrons in graphite. He had confidence that such experiments would be carried out because financial support by the U.S. Government had already been assured to the project. He said that this assurance gave him "the will to think." In these four words he distilled the essence of a very significant insight: A competent thinker will be reluctant to commit himself to the effort that tedious and exacting thinking demands—he will lack the will to think—unless he has conviction that, if by thinking he does create new and significant ideas, then these ideas will be used in worthwhile ways. This, Fermi told me, was why the assurance of financial support gave him the will to think of what would be the optimum design for his experiments.

Many years later Fermi gave me another valuable word. This time it was one related to transistors. I had discussed with him aspects of semiconductor theory related to minority carrier injection at emitter junctions. I defined quasi-Fermi levels and said that these concepts deserved a more compact label. He suggested "imref." I introduced this word into the scientific literature in the first paper on microwatt junction transistors, coauthored by M. Sparks and G. K. Teal. A footnote reported that the most appropriate authority had suggested imref. (If in doubt, spell imref backwards.)

How a significant contribution to "the will to think" had a major impact on the activities of the semiconductor research group is shown in the statistics of figure 14. The data are based on research that I undertook a few days after the transistor's 25th birthday. It shows on a month-by-month basis the number of pages in laboratory notebooks used by Bardeen, Brattain and myself. The "magic month" of November 17 to December 17, 1947 has been extended to 5 weeks so as to include the famous notebook entry of Brattain of Christmas Eve, 1947. For each of three men, a dashed line shows the number of pages used per month on the average for a preceding period of about 1 year.

The abrupt increase in activity shown in figure 14 started during the week of 17 to 24 November for Bardeen's and Brattain's notebooks. During that week Brattain used 15 pages and Bardeen used 10 pages compared respectively to 8 and 0 the week before. Compared to their long-term average weekly rates of filling pages, these 1-week records were more than 4 times faster for Brattain and more than 10 times for Bardeen. Chance statistical fluctuations can be ruled out as the explanation for the high activity of the 17 to 24 November week simply by inspection of figure 14—the persistence of the high level for the magic month through the whole first quarter of 1948 is clear evidence that the 17 to 24 November week initiated a real jump upwards in activity. My own increased notebook activity started 3 weeks later on 8 December. In the following week, I used 5 pages—about 10 times my long-term weekly average—thus starting a pronounced upward trend that lasted many months.

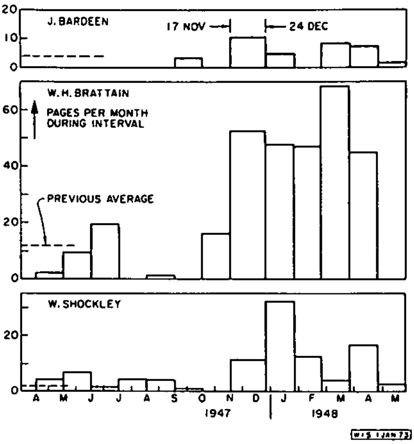


FIGURE 14. *The influence of "the will to think," initiated on 17 November 1947, upon the rate at which notebook entries were made related to the patents of table I.*

(Areas represent total pages and ordinates represent rates. For Brattain's notebook the period of 25 April to 25 May 1948 was not comparable to earlier dates because he shared additional new notebooks with technical assistants.)

What initiated this dramatic step-up of tempo? What stimulated the will to think of items worth entering as permanent records in our laboratory notebooks?

These questions are answered by understanding the "breakthrough observation" which occurred on Monday, November 17. This important event is recorded in the notebook entry of Walter Brattain shown in figure 23. This breakthrough observation led at once to a "breakthrough interpretation": *the discovery of a way to overcome the blocking effect of the surface states* that, so frustratingly, had caused our many failures to make a field-effect amplifier during the previous 2 years.

This breakthrough interpretation was based on the "new facts" resulting from "new experimental conditions" brought about by the "research on the related science (surface states)" in the language used to define the research principle of "respect for the scientific aspects of practical problems"—the chart of figure 6. All the needed facts for the breakthrough interpretation are recorded in figure 15: p-type silicon in the circuit diagram and, in the text, the new fact that "a plus potential as shown increased the effect and the opposite potential reduced the effect to almost zero."

When it was made in November 1947, the breakthrough interpretation was a basic new insight about the science of semiconductor surfaces. Twenty-five years later, the interpretation is simple—perhaps obvious—to an electronic device engineer. Here is the background:

According to Bardeen's surface-states theory, the surface of the semiconductor might be so charged as to repel majority carriers. In the case of the p-type silicon specimen of figure 15, a positive surface-state charge would repel holes. If photons of light fall on the semiconductor and generate hole-electron pairs, then the field just below the surface will separate them, electrons going to the surface and holes to the interior. This separation will cause surface potential to become more negative. Such effects had been observed and were being actively studied by Brattain—a key feature, in the phrasing of figure 6, of the research on the scientific aspects of the practical problem that surface states had prevented the achievement of amplification by using the field-effect.

Brattain used an intermittent flashing light that produced an AC voltage on the silicon surface. This voltage change was detected and measured by using the metal plate parallel to the silicon surface.

The observations reported in figure 15 initiated the burst of creativity evidenced by the statistical effects of figure 14. In the following text, to emphasize the tempo of "the magic month," I have prominently displayed the date including the day of the week at the beginning of many of the paragraphs.

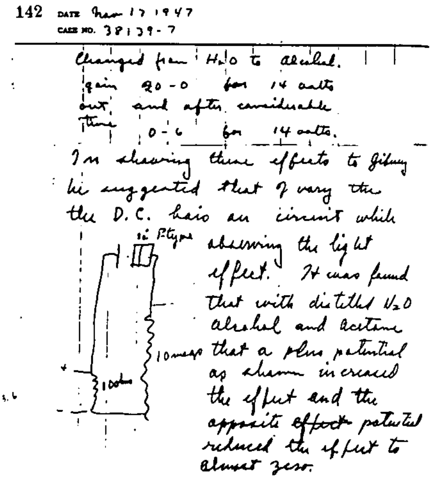


FIGURE 15. *Brattain's notebook entry of 17 November 1947 that motivated the "will to think" that initiated the "magic month" culminating in the invention of the point-contact transistor on 16 December 1947.*

(This figure shows the upper three-quarters of page 142 of Brattain's notebook 18194. In subsequent figures such headings are deleted and in some cases consecutive pages are joined continuously.)

3.2. The Magic Month and the Birth of the Point-Contact Transistor

Monday, 17 November 1947. On this date, as recorded by Brattain in figure 15, Gibney made the key suggestion that voltage be applied between the metal plate and the semiconductor while both were immersed in an electrolyte. By this means a strong electric field was generated perpendicular to the semiconductor surface. The effects observed by Brattain showed that, when the polarity of the applied field was such as to make the silicon surface more positive, there was an increase in the negative photovoltaic change produced on the surface by the light. This new phenomenon could best be explained by assuming that the internal field inside the semiconductor had been increased so as to repel the majority holes further from the surface, and, more important for the experiment, to draw photoelectrons more effectively to the surface and thus to enhance the separation of the hole-electron pairs. This is the significance of Brattain's words "that a plus potential as shown increased the effect." Similar reasoning explains why "the opposite potential reduced the effect to almost zero."

At long last, Brattain and Gibney had overcome the blocking effect of the surface states—the practical problem that had caused our failure to succeed in making a field-effect transistor work.

The phrase "accident favors the prepared mind"—in this case minds prepared to try to make field-effect amplifiers—applies to what happened next. In the phrasing of figure 6, the new experimental conditions and the resulting new facts motivated the will to think of ways to achieve a practical result. Within a week, the inventions of patents 1 and 2 of table 1 were conceived.

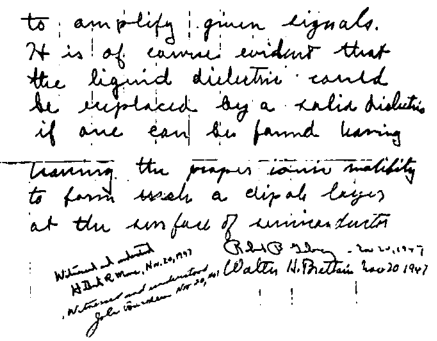
These inventions included at least one field-effect concept that was far ahead of its time. This was the inversion-layer, insulated-gate, field-effect transistor of Bardeen's patent 2 of table 1. The basic concept was clearly expressed by Bardeen, as discussed below, on 23 November 1947. However, more than 10 years had to elapse before the IGFET (insulated-gate-field-effect-transistor) became a useful device. Indeed, the useful IGFET technology was founded on earlier development of silicon junction transistor technology; for example, diffused source and drain regions, not contemplated in Bardeen's patent. These techniques needed even further refinements to produce the silicon dioxide insulating layers suitable for the MOSFET (metal-oxide-semiconductor-field-effect-transistor) of integrated circuits.

The role of the will to think provoked by the breakthrough observation of 17 November 1947 appears clearly in Bardeen's recognition of the role of the inversion layer. His concept of inversion layers had been well developed before. So had his interest in field-effect amplifiers. But the inventive combination was not conceived before the breakthrough observation. Then, only 4 days later, the inversion-layer, electrolyte combination was actually used in a device that controlled current. These observations about inversion layers and field-effect amplifiers are similar to those discussed in section 4 about injection in junction transistors. In each case all the concepts needed for the inventive combination were available to Bardeen or to me long before either he or I conceived of the new inventive combination.

The discussion presented in this section calls for a remark relevant to the influence of the "exclusive rights" paragraph in the Powers of Congress discussed in section 1. The research on history of the magic month, indeed the impact of the breakthrough observation itself, became clearly evident by virtue of the records which were preserved because of the nature of the patent laws.

From these records it thus becomes obvious in retrospect what caused the burst of activity for Bardeen's and Brattain's notebooks in the first week of the magic month.

Thursday, 20 November 1947, Only 3 days after the new facts were observed, Brattain and Gibney wrote a disclosure of the concept of patent 1 on table 1, suggesting that electrolytes could be used to produce field-effect amplifiers. As shown after the final paragraph, reproduced in figure 16, this disclosure was witnessed on the same day by John Bardeen and H. Moore. In this case, the time lag was not—as it was for my 1939 conception—two months between the conception of the invention and the witnessing of the disclosure, nor even the 2 weeks that had elapsed in 1945 for Bardeen's checking of my field-effect calculations. Why did things move so slowly then? At those earlier dates, we were doubtful that thinking would produce worthwhile action. But after 17 November 1947, it was different. The discovery that surface-states could be overcome aroused in minds conditioned to search for semiconductor amplifiers, the will to think and to act.


to amplify given signals. It is of course evident that the liquid dielectric could be replaced by a solid dielectric if one can be found having having the proper ionic mobility to form such a dipole layer at the surface of semiconductor

Witnessed and understood Nov. 20, 1947 H R Moore
R H Gibney Nov 20, 1947
Walter H. Brattain Nov 20 1947
Witnessed and understood Nov 20, 1947 John Bardeen

FIGURE 16. *The disclosure by Brattain and Gibney of the conception of patent 1 of table 1 and the signatures of Bardeen and Moore who witnessed it on the same day, 20 November 1947.*

Friday, 21 November 1947. One day after its conception, a device covered by both patent 1 and patent 2 of table 1 was reduced to practice. The arrangement that was used is shown in figure 17 and may be understood in terms of the figures from the issued patents 1 and 2 shown in figure 18. A point contact to a p-type surface layer on n-type germanium is surrounded by a drop of electrolyte that is insulated from it by a layer of wax (9 and 27 in fig. 18). Contact to the electrolyte is made by a circle of metal (13 and 26 in fig. 18) that is formed from the heavier wire in figure 17.

FIGURE 17. *The experimental apparatus of patents 1 and 2 consisting of a point-contact passing through a drop of electrolyte with a "grid" electrode in the form of a ring (not visible in the figure) in the electrolyte.*

Actually at least three separate ideas are involved in this device. The use of the electrolyte to produce a high field at the surface is covered by the claims of Brattain-Gibney, patent 1. The layer that insulates the metal point from the electrolyte and also the inversion layer are covered in Bardeen's patent 2.

Saturday, 22 November 1947. On this day Bardeen, who had participated in the conception of figure 18 and in making the observations, filled seven pages of his notebook with a report and an analysis of the data gathered the day before. In it he stressed the role of an inversion layer on the silicon surface.

"As noted by Brattain and Gibney, this effect might be used to control the resistivity of thin films ('Shockley effect'), and thus lead to a control element. It occurred to the writer that the effect might be observed in the thin n-type layer on the surface of

FIG. 3

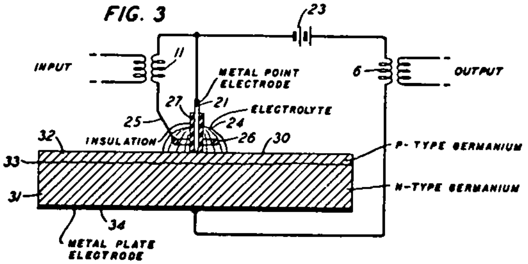
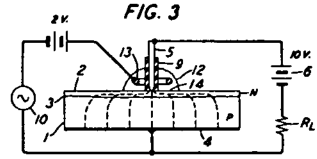


FIG. 3



INVENTORS: W. H. BRATTAIN
R. B. GIBNEY
BY Harry C. Hart
ATTORNEY

INVENTOR
J. BARDEEN
BY Harry C. Hart
ATTORNEY

FIGURE 18. *Corresponding figures from the issued version of patents 1 and 2 showing the structure of the electrolytic semiconductor amplifier.*

a block of p-type Si. It was suggested that this be tried because thin films so far developed did not exhibit normal rectifying characteristics."

The final paragraph of this notebook entry of Bardeen's emphasized the "will to think" relevance of the new experiments to the practical goal of an amplifier: "...these tests show definitely that it is possible to introduce an electrode or grid to control the flow of current in a semiconductor."

Sunday, 23 November 1947. The impact of the new facts stimulated Bardeen's will to think more incisively about amplifiers. Figure 19 shows his discussion of the mechanism of current flow from a point contact made to the inversion layer on the surface. Figure 20 shows how he conceived the invention of patent 2 in table 1 and figure 21 shows the same diagram as printed in the issued patent.

Monday, 24 November 1947. On this date Bardeen disclosed his invention for Walter Brattain's signature as a witness. Two days later, on the Wednesday before Thanksgiving, I also wrote "witnessed and understood" under this disclosure.

Bardeen's patent 2 of table 1 is, in the language developed 25 years later, a form of an insulated-gate, field-effect transistor with an inversion-layer channel. The use of a second electrode contact to the inversion layer, like the second electrode shown in figure 3 proposed in 1940, does not appear to have been included in patent 2. Instead the current that was controlled was considered to flow out through the inversion layer gradually leaking across the barrier into the underlying base. The basic concept of saturating drain currents, so important for field-effect transistors, was first to appear 4 years later in my field-effect paper.

Thursday, 4 December 1947. This is the date of the very informative discussion of three experiments entered in Brattain's notebook shown in figure 22. The experiments designated I and II had both been successfully tried. One of these, experiment I, was the previously discussed concept of patent 1. The third, marked III, anticipates the structure of the point-contact transistor of patent 4. The second experiment, designated II, was based on a suggestion that I had made of placing a drop of electrolyte across a p-n junction. This proposal was probably reduced to practice by G. L. Pearson within a few days of Thanksgiving, 27 November 1947.

A digression on patent-writing art and the junction transistor. Experiment II, like experiment I, was a success as an amplifier at low frequencies—frequencies so low due to the limitations imposed by the response of the electrolyte that experiment II was a failure as a possible telephone amplifier. But Experiment II was a creative failure in several ways. One of these occurred, as discussed below, on 8 December. Another was its significant role in the preparation of the patent application for the junction transistor—indeed the electrolyte across p-n junction is figure 1 in patent 5 of table 1. The dates that I have given for conception and for reduction to practice for patent 5 are not in November 1947 as would be in keeping with Brattain's report that Pearson had been successful before 4 December 1947.

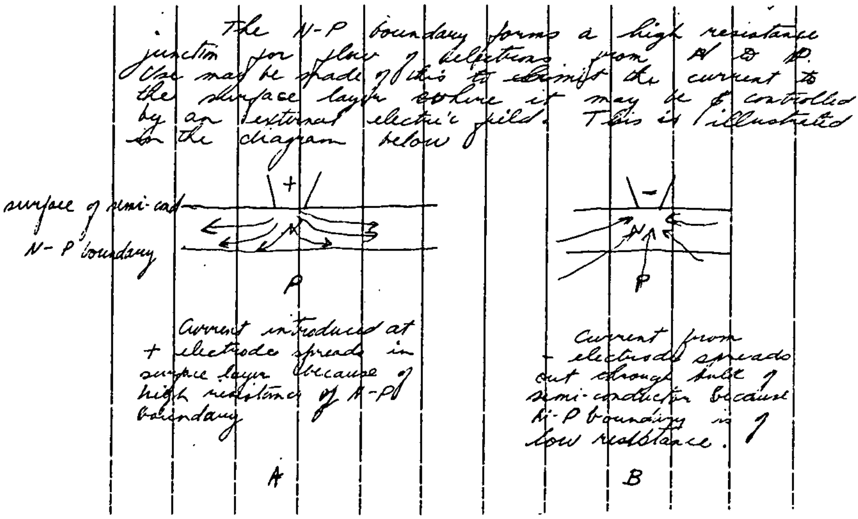


FIGURE 19. *Bardeen's notebook entry of Sunday, 23 November 1947 on the theory of current flow from a point-contact through an inversion layer on a semiconductor circuit.*

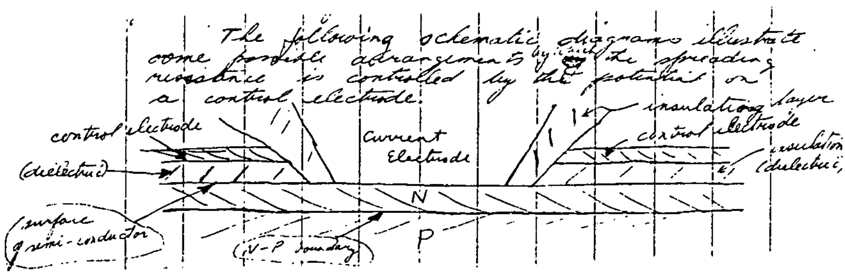


FIGURE 20. *Bardeen's conception of patent 2 in table I that anticipates an insulated-gate field-effect transistor with an inversion layer channel.*

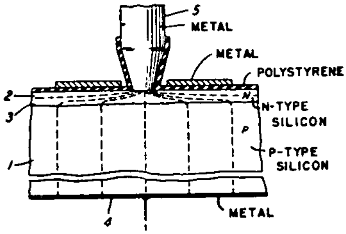


FIGURE 21. *The version of Bardeen's conception of the insulated-gate inversion layer channel structure as it appears in the issued form of patent 2 of tab. I.*

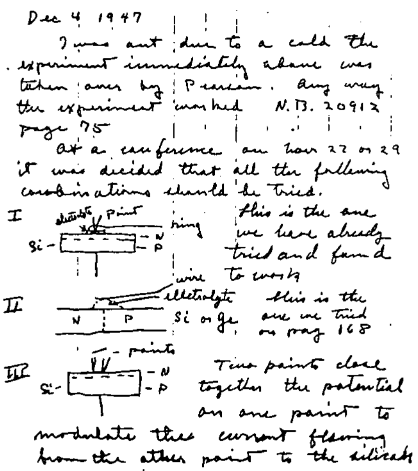


FIGURE 22. *Three key experiments recorded in Brattain's notebook on 4 December 1947.*

Patent 5 teaches in detail how to make "electrolyte on p-n junction" amplifiers. Furthermore, some of the allowed claims in patent 5, the junction transistor patent, read directly on experiment II. The suggested electrotyte, glycol-borate, was then a readily available laboratory commodity, being used in standard electrolytic capacitors. (The customary glycol-borate extraction method required a hammer, a nail, and a vise.) Claim 29 of patent 5, which I shall analyze as an example of the art of patent writing, is a typical example of several similar claims in the junction transistor patent that read on the structure of the device employing glycol-borate across a p-n junction (a structure that the patent specification does indeed clearly teach how to construct):

"29. An electrical translating device comprising a body of semiconductive material having two zones of unlike conductivity type separated by a barrier, means of establishing current flow between said two zones, *and means separate from said first means for controlling the impedance to such current flow introduced by said barrier.*" (Emphasis added here for purposes of exposition.)

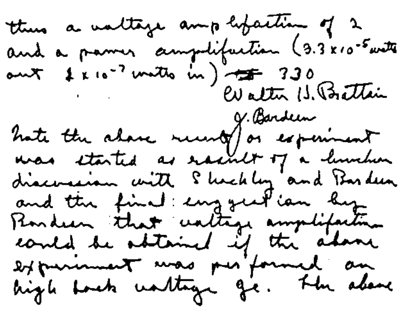
I do not recall now that I was impressed then, when the patent was filed, by the ingenuity of this claim. It was only in the course of the research reported in this article that I really appreciated the quality of patent-writing art displayed by Rudi Guenther who was the attorney. Claim 29 reads directly on the structure of experiment II, figure 22; and yet, this claim is so constructed that it reads equally well upon a three-layer sandwich-type transistor such as the one in figure 12 that I am shown explaining on a blackboard. For the case of the electrolyte across the p-n junction, the "means separate from said first means" of Claim 29 obviously signifies effects produced by voltage applied to the electrode in the drop of electrolyte. But the identical phrase does indeed apply equally well to the case of the three-layer structure of the true junction transistor. For this the three-layer structure the "barrier" is the collector junction and the "means separate from said first means" signifies injection across the emitter junction followed by diffusion through the base layer. Guenther had structured the patent application, including Claim 29, so that the specification clearly taught how to practice the electrolyte embodiment on which Claim 29 read. But at the same time Guenther had kept Claim 29 broad enough to cover the concept of the true junction transistor, which, while described in detail in patent 5, we did not then know how to make. As a matter of fact, however, those "skilled in the art"—or did they subsequently become skilled?—did successfully make them about 2 years later at approximately the same date that patent 5 was issued.

In table 1, I have put a date of 23 January 1947 for the conception of the junction transistor and April 1950 for the reduction to practice. Both dates apply to a proper three-layer, junction-transistor structure. If the drop of electrolyte were accepted as a form of junction transistor, then both the "conception" and the "reduction to practice" dates would fall in November 1947. Actually, none of the electrolyte devices of table 1 turned out to have any practical value. In April 1949, another possible date for "reduction to practice," a germanium transistor was made with a geometry like the original point-contact transistor of figure 9, but with the gold contacts replaced by p-type stripes making p-n junction connections with the n-type base. (The method of fabrication is discussed with figs. 38 and 49.) Power gains as high as 16 were obtained. However, a good 3-layer structure was not achieved until about 1 year later. Thus the dates shown in table 1 for patent 5 are the realistic ones for a true junction transistor.

This discussion of the "electrolyte on p-n junction" devices described in Brattain's note of 8 December 1947 has carried the discussion far ahead. I shall now return to the "magic month."

Monday, 8 December 1947. On this date several significant stepping stones were laid on the path to the point-contact transistor. As shown in figure 23 from Walter Brattain's notebook, observation of significant voltage and power gains were reported over the signatures of Bardeen and Brattain. The amplifying device which they used consisted of the drop of electrolyte and point-contact structure of patent 1. However, a key new feature was the material used—high back-voltage, n-type germanium—later a central feature in achieving the voltage gain of the point-contact transistor. As figure 23 shows, Brattain reported that the key suggestion was made by Bardeen while the three of us had a luncheon discussion. The circumstance of this discussion presents an important illustration of the mixture of cooperation and competition that characterized the interactions within the semiconductor group.

Monday, 8 December 1947. Background for the subject of discussion at the luncheon is given by the last of several of my own notebook entries for the same date—the date that initiated my own stepped-up tempo of notebook pages per month. These entries commence with the disclosure in figure 24 of the junction, field-effect amplifying principle that thus authenticates the 1947 date on table 2. The topic connected with the lunch conference of 8 December appears somewhat lower on the same



thus a voltage amplification of 2 and a power amplification (3.3×10^{-5} watts out 1×10^{-7} watts in) 330
Walter H. Brattain
J. Bardeen
Note the above result of an experiment was started as result of a luncheon discussion with Shockley and Bardeen and the final suggestion by Bardeen that voltage amplification could be obtained if the above experiment was performed on high back voltage Ge. The above

FIGURE 23. *The achievement of voltage gain on 8 December 1947 by Bardeen and Brattain using high back-voltage n-type germanium.*

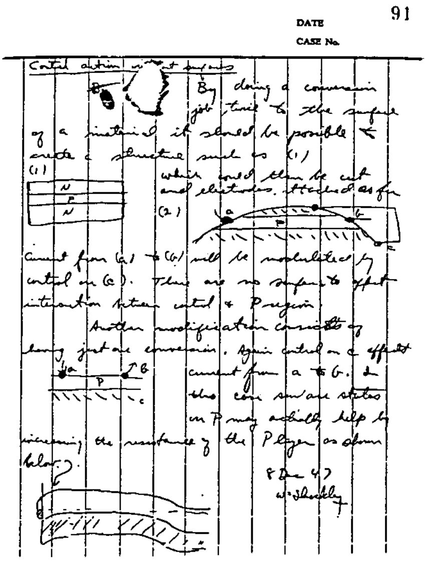
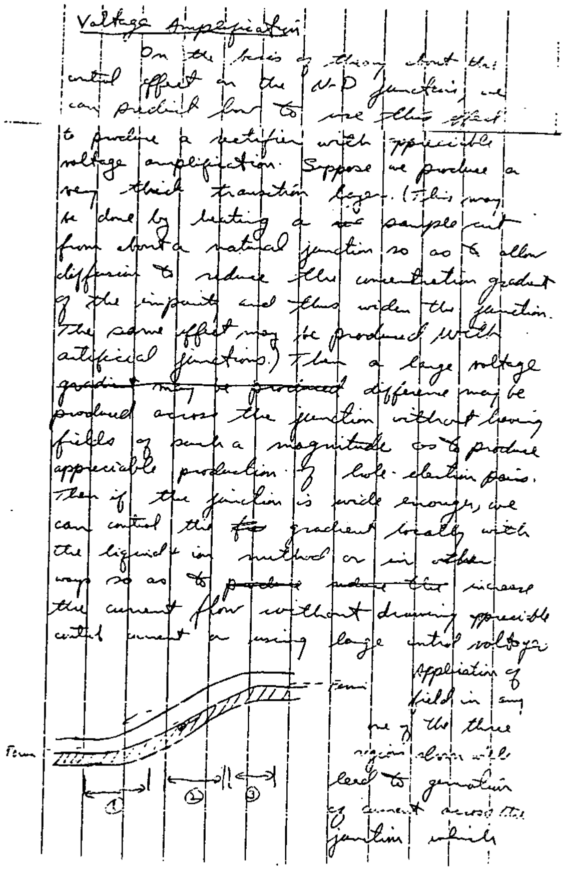


FIGURE 24. *The disclosure of a junction, field-effect transistor structure by Shockley on 8 December 1947.*

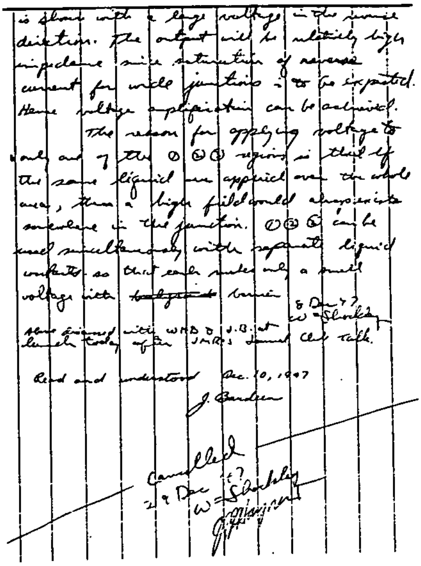
page and is reproduced on figures 25 and 26. What this disclosure proposes is that a wide p-n junction operated at high reverse bias might be used to obtain voltage gain. Placing drops of electrolyte at localized intermediate points on the junction might control reverse current using relatively small voltages applied to the electrolyte and thus produce large voltage changes across the p-n junction.


Voltage Amplification

On the basis of theory about the control effect in the n-p junction, we can predict how to use this effect to produce a rectifier with appreciable voltage amplification. Suppose we produce a very thick transition layer. (This may be done by heating a ~~xx~~ sample cut from about a natural junction so as to allow diffusion to reduce the concentration gradient of the impurity and thus widen the junction. The same effect may be produced with artificial junctions.) Then a large voltage ~~gradient may be produced~~ difference may be produced across the junction without having fields of such a magnitude as to produce appreciable production of hole-electron pairs. Then if the junction is wide enough, we can control the ~~fie~~ gradient locally with the liquid + ion method or in other ways so as to ~~produce reduce the~~ increase the current flow without drawing appreciable control current or using large control voltages.

Application of field in any one of the three region shown will lead to generation of current across the junction which

FIGURE 25. *The proposal of using a wide* p–n *junction with electrolyte to obtain voltage gain from Shockley's notebook on 8 December 1947.*


is shown with a large voltage in the inverse direction. The output will be relatively high impedance since saturation of reverse current for whole junction is to be expected. Hence voltage amplification can be achieved.

The reason for applying voltage to and and 7 thru ① ② ③ regions is that if the same liquid were applied over the whole area, then a high field would appear everywhere in the junction. ① ② ③ can be used simultaneously with separate liquid contacts so that each applies only a small voltage with ~~high~~ time.

8 Dec ?? W Shockley

Above discussed with WHB & J.B. at lunch today after JAB's Journal Club talk.

Read and understood Dec. 10, 1947 J. Bardeen

Cancelled 8 Dec ?? W = Shockley

FIGURE 26. *A continuation from figure 25 of the disclosure of 8 December 1947.*

Typical examples of creative-failure methodology are illustrated by the circumstances related to the suggestion of figures 25 and 26. The idea was unquestionably half-baked. Almost certainly the device employing "electrolyte on the p-n junction" operated in ways that we probably did not conceive of and would not have been effective in the arrangement that I discussed with Bardeen, as recorded in figure 26. The actual working mechanism of experiment II (described in Brattain's notebook on 4 December shown in figure 22) was probably the formation of surface-channels near the junction. These would then increase the reverse current. However, this half-baked, reverse-biased junction idea of 8 December did (as Brattain's reference to Bardeen's suggestion on fig. 23 indicates) serve as a stepping stone in Bardeen's thinking and led him to consider the advantages of using a rectifying contact at high reverse bias and thus to suggest trying high back-voltage germanium. The suggestion paid off. Later the same day, he and Brattain obtained voltage gains with the patent 1 or 2 form of device with which they were experimenting as Brattain reported in the 8 December 1947 entry of figure 23.

This voltage gain was an important step forward on the path to the point-contact transistor. They arrived there 8 days later.

Tuesday to Friday, 9 to 12 December 1947. During the remainder of the week, a series of experiments was tried. Exactly 9 years later in Stockholm—Nobel prizes are awarded on December tenth—John Bardeen discussed these experiments in the first of his two Nobel Lectures. In "Le Prix Nobel" he published the lecture as "Semiconductor Research Leading to the Point Contact Transistor" and wrote as follows:

"It was next decided to try a similar arrangement with a block of n-type germanium. Although we had no prior knowledge of a p-type inversion layer on the surface, the experiments showed definitely that a large part of the reverse current consisted of holes flowing in an inversion layer near the surface. A positive change in voltage on the probe [the ring in the electrolyte of figure 17 and 18] decreased the reverse current. Considerable voltage as well as current and power amplification was observed.

"Because of the long time constants of the electrolyte used, amplification was obtained only at very low frequencies. We next tried to replace the electrolyte by a metal control electrode insulated from the surface by either a thin oxide layer or by a rectifying contact. A surface was prepared by Gibney by anodizing the surface and then evaporating several gold spots on it. Although none made the desired high resistance contact to the block, we decided to see what effects would be obtained...."

The culmination of these efforts, as described in Brattain's notebook, was achieved in the first 2 days of the week starting on 15 December 1947.

Monday, 15 December 1947. On this day Brattain recorded experiments in which the electrolyte had been replaced by evaporated gold. The gold was divided into two areas, spots A and B. Figure 27 shows the last nine lines of Brattain's notebook entries for 15 December 1947 appearing on the bottom of page 191 to the top of 192.

The polarity of the effects with a gold spot are consistent with hole injection: a plus voltage on spot B would inject holes into the n-type germanium that were then collected to increase the positive current

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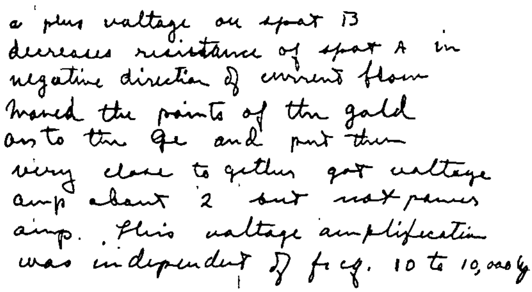

a plus voltage on spot B
decreases resistance of spot A in
negative direction of current flow
moved the points of the gold
on to the Ge and put them
very close together got voltage
amp about 2 but not power
amp. This voltage amplification
was independent of freq. 10 to 10,000 cy

FIGURE 27. *Brattain's notebook entry of 15 December 1947 describing an experiment involving a concept of the point-contact transistor.*

flowing out of A, the directions corresponding to a "negative direction of current flow."

But the crucial circumstance that occurred on 15 December was to move the two point contacts from the gold spots to the germanium—an advance in research technique specified by the two prepositions "off" and "on to." As one more example of the human fallibilities that characterized the creation of the transistor—a trivial example compared to some of my conjectures about making transistors by evaporation—note that on figure 27 the key feature of leaving the gold spots and working on the germanium—the act described by the preposition "off"—indeed, the first feature of the breakway that would create the point contact-transistor within 32 hours—was spelled "of": "of the gold and on to the germanium."

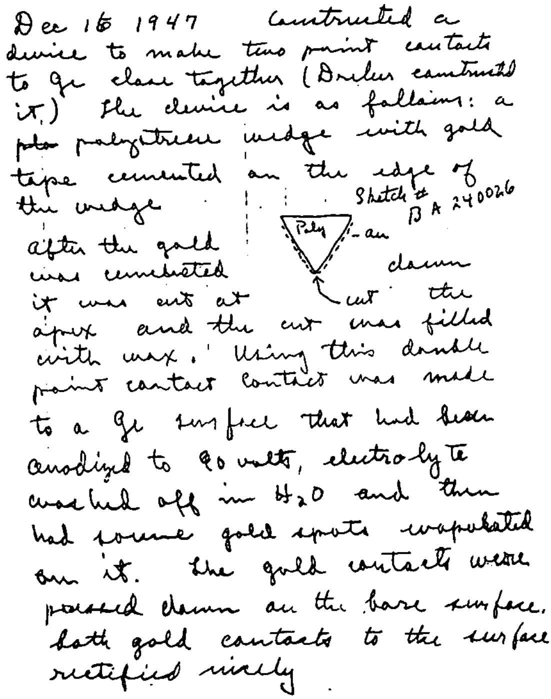
But no matter how it is spelled, the experiment worked. Brattain reported voltage gains with both points on the germanium. This experiment appears to be the first clear conception of the point-contact transistor and accounts for the date of 15 December 1947 for patent 4 in table 1.

Tuesday, 16 December 1947. As Brattain's notebook entries show, this was the date at which the point-contact transistor was reduced to practice in the form shown in figure 7. Figure 28 describes the structure constructed under Brattain's direction by his assistant, E. G. Dreher. In figure 29 Brattain reports both a voltage gain of 15 and a power gain of 1.3. On the next page (not shown here in a figure) he reports probably the first example of current gain for a point-contact transistor—a voltage gain of 4 associated with a greater power gain of 4.5 thus implying a current gain of more than 1.1—a feature that in due course led to the later filing date discussed in table 1.

Clearly, the transistor had been born. But had it been "invented?" This is a question of patent law.

Tuesday, 23 December 1947, was the date of the private demonstration for executives that is described in Brattain's famous notebook entry of Christmas Eve, 1947, figure 8. Although this date of 23 December 1947 has been publicly accepted in some instances as the date for the birth of the transistor, as discussed above, my research on laboratory notebooks documents that 16 December is the first date when a point-contact transistor did amplify. The period of 29 days from the breakthrough observation of 17 November to the point-contact amplification of 16 December thus includes so many creative contributions and starts and finishes with such significant events as to deserve the title "the magic month."

There is an element of obvious, and entertaining, naivete about accepting the day before Christmas Eve of 1947 as the date of the birth of the transistor. The demonstration of 23 December was attended by Harvey Fletcher, the Director of Physical Research, to whom I reported, and Ralph Bown, the Director of

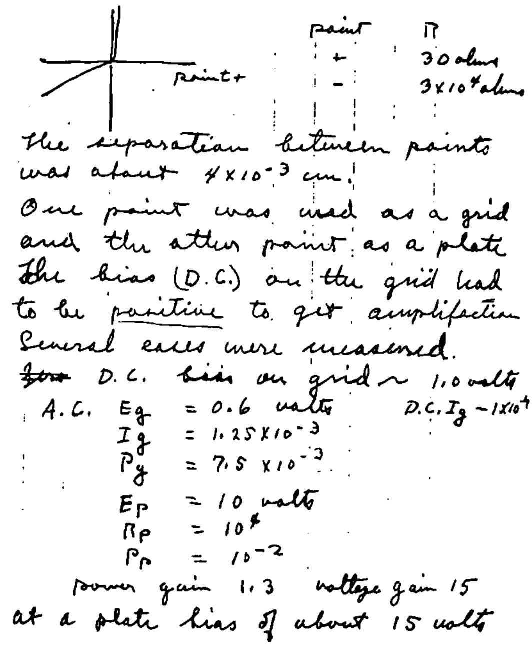

Dec 16 1947 Constructed a device to make two point contacts to Ge close together (Dreher constructed it.) The device is as follows: a polystrene wedge with gold tape cemented on the edge of the wedge. Sketch # BA 240026

after the gold was cemented it was cut at the apex and the cut was filled with wax. Using this double point contact contact was made to a Ge surface that had been anodized to 90 volts, electrolyte washed off in H_2O and then had some gold spots evaporated on it. The gold contacts were pressed down on the bare surface. Both gold contacts to the surface rectified nicely.

FIGURE 28. *Brattain's notebook entry for 16 December 1947 describing the construction of the "wedge" arrangement for bringing two point-contacts close together in a point-contact transistor.*

Research, to whom Harvey Fletcher reported. To believe that such a demonstration occurred on the first day that a transistor had worked—the schedules of these executives—the reliability of the demonstration equipment—a decision by the research scientists that they were sure—what better definition of naivete? But this is an example of creative-failure methodology in public relations. The 24 December 1947 notebook entry is dramatic; hearing speech amplified by the transistor was in the tradition of Alexander Graham Bell's famous "Mr. Watson, come here, I want you!" (Unfortunately, there is no record that these words were transmitted through a transistor during the 23 December 1947 demonstration.) Thus the failure to date correctly the birth of the transistor led to a warm Christmas Eve atmosphere for the first coming of age of a transistor that achieved the goal of aiding people, when separated, to communicate more easily than ever before with each other.

There is no record that Brattain's and Bardeen's experiments of 16-22 December were formally witnessed by others—an important factor of "reduction to practice." The 24 December notebook entry was witnessed by others who confirmed the recorded facts about the 23 December demonstration. Thus the date of 23 December does appear on the records of the Patent Department as the date of "reduction



point+

point	R
+	30 ohms
−	3×10^4 ohms

The separation between points was about 4×10^{-3} cm.
One point was used as a grid and the other point as a plate
The bias (D.C.) on the grid had to be positive to get amplification
Several cases were measured.
~~for~~ D.C. bias on grid ~ 1.0 volts
A.C. E_g = 0.6 volts D.C. $I_g \sim 1\times10^{-4}$
I_g = 1.25×10^{-3}
P_g = 7.5×10^{-3}
E_p = 10 volts
R_p = 10^4
P_p = 10^{-2}
power gain 1.3 voltage gain 15
at a plate bias of about 15 volts

FIGURE 29. *A continuation of the 16 December 1947 notebook entry reporting the first record of power gain with a point-contact transistor.*

to practice." A test of reduction to practice in patent law is whether a businessman would be prepared to invest in development. There was, of course, no doubt about this at the Laboratories—or was there on 23 December?

I have a clear recollection that Harvey Fletcher did raise a significant question to this effect:

"How do you know you really have amplification in the telephone conversation demonstration? It may be simply matching of impedances? Making an oscillator would be a valuable confirmation."

Actually the input and output voltage measurements had already clearly shown true power gain. But it is noteworthy that Brattain's 24 December notebook entry continues after the report of the 23 December demonstration to record that an oscillator was constructed on 24 December and did, indeed, oscillate.

The identification of the exact date of the "invention" of the transistor is a legal matter. If "conception" is taken as the definition of "invention," then 15 December is a date confirmed both by my notebook research and by the Patent Department's records. First observation of amplification was 1 day later. "Reduction to practice" in a legally sound form was 23 December. In any event, it was a wonderful start for a 4-day Christmas weekend.

The Patent 3 Story. Patent 3 was conceived and reduced to practice in connection with the point-contact transistor. The surface treatments devised by Gibney played an essential role in making point-contact transistors at that time. I recall a most trying week somewhere in late December or early January when for some reason the treatments failed and no transistors worked. As discussed earlier in connection with figure 9, Gibney's patent dealt with the electrolytic processes used for preparing the surface and for cleaning-off the oxide products while preparing an inversion layer for use with contacts as shown in figure 9.

4. The Invention and Realization of the Junction Transistor

4.1. Pre-Invention Explorations That Missed the Key Concept of Minority-Carrier Injection

The completion of the invention of the junction transistor was accomplished with conception of an emitter junction to inject minority carriers into a sufficiently thin base layer. These injected carriers would then diffuse to the collector junction. Satisfactory collection efficiency and voltage gain would then result from reverse-bias on the collector junction—a concept already proposed as discussed in connection with figures 25 and 26. All of these ideas had been considered in work that I had done before the point-contact transistor amplified—with one exception—the recognition of the importance of injection. It took exactly one calendar month after the demonstration of the point-contact transistor on 23 December 1947 before the missing piece was inserted to complete the concept of the junction transistor.

An aside is appropriate here. Frankly, Bardeen and Brattain's point-contact transistor provoked conflicting emotions in me. My elation with the group's success was balanced by not being one of the inventors. For the next 5 years, I did my best to try to put the Labs—and myself—in the lead for transistor patents. (Most of my 90-odd issued U.S. patents relate to the transistor.) Such efforts account for much of my "will to think" peak of notebook pages for the month ending on 25 January 1948 as shown in figure 14. One example is worth discussing as an illustration of how easy it was to miss the concept of injection while trying to invent transistors based on p-n junctions.

31 December 1947. On New Year's Eve I was alone in Chicago between two meetings that came so close together that a return to New Jersey seemed impractical. I used this opportunity for uninterrupted efforts to invent new semiconductor amplifying principles. In 2 days I wrote enough to fill a bit more than 19 notebook pages. My notebook was at the Laboratories and I used a pad of paper and mailed the disclosures back to my co-supervisor, S. O. Morgan, who witnessed them and asked Bardeen to do the same. Later these pages were rubber-cemented into my notebook where they remained available for study while writing this article.

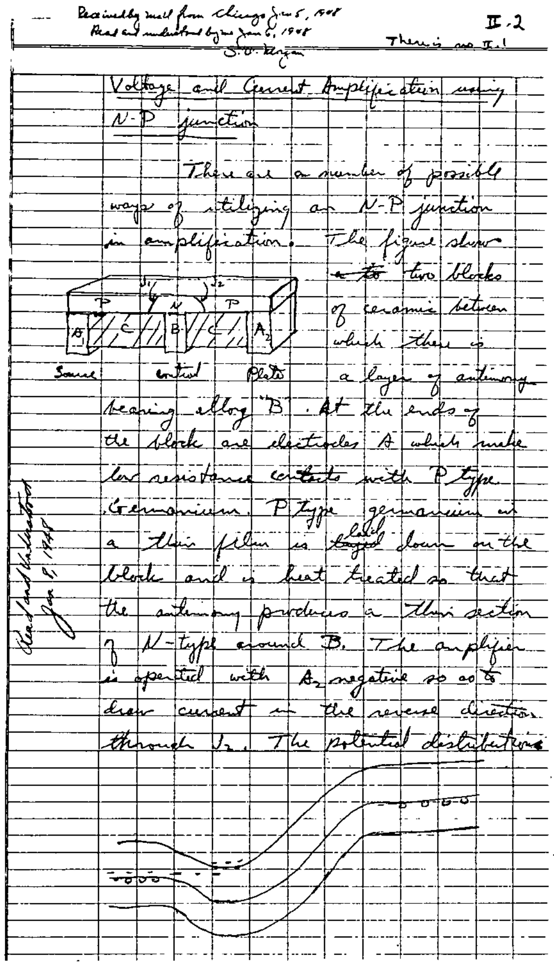
The pages from which figures 30 and 31 were reproduced were three of the five which were written on New Year's Eve. The other 14 were written on New Year's Day.

The structure shown in figure 30 has two p-type regions separated by a strip of n-type formed by heating a thin film of germanium lying upon a plane formed by two ceramic insulators separated by a thin layer of antimony-bearing alloy. The antimony diffuses into the germanium converting a strip into n-type. I shall not consider the naivete of the fabrication scheme. I shall instead focus on a blind spot in the amplification concept. These notebook entries reveal that I missed an obvious opportunity to recognize the possibility of minority carrier injection into a base layer and did so even while considering a device containing a base layer and while drawing energy-band diagrams almost indistinguishable from those for true junction transistors.

Figure 30 shows the structure of the device and also contains an energy band diagram representing the variation along a line extending from the p-type emitter through the n-type base to the p-type collector. It looks precisely like the diagram for a true junction transistor. But that wasn't the idea at all.

How the amplification concept of this disclosure differed from that of a true junction transistor is represented in the lower diagram of figure 31. The antimony was supposed to convert the germanium to strong n-type near the antimony-bearing, metal-contact B, while barely converting it at all near the surface. Thus the barrier for hole flow was low near the surface and high near B, as indicated in the diagram. By applying negative voltage to B, the barrier could be made so low that holes could easily flow over it to reach the reverse-biased collector. The disclosure also suggests using the structure as a junction, field-effect transistor with a superficial p-type layer channel lying over the n-type stripe gate.

What is conspicuously lacking is any suggestion of the possibility that holes might be injected into the n-type material of the stripe itself, thereby becoming minority carriers in the presence of electrons. Why this idea did not occur to me then or even much earlier is baffling. Eight months before in the same notebook I had written an analysis of thermal



Received by mail from Chicago Jan 5, 1948
Read and understood by me Jan 6, 1948
S. O. Morgan

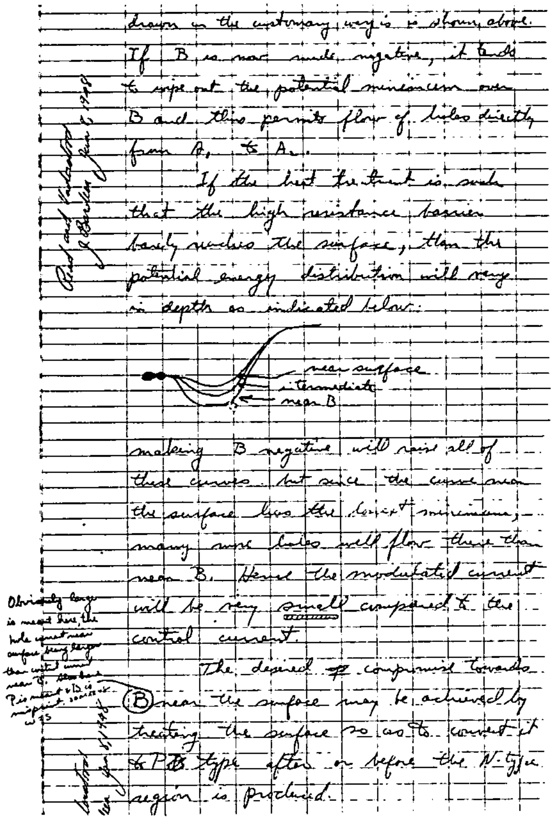
II.2

There is no II.1

Voltage and Current Amplification using N-P junction

There are a number of possible ways of utilizing an N-P junction in amplification. The figure shows ~~a to~~ two blocks of ceramic between which there is a layer of antimony bearing alloy "B". At the ends of the block are electrodes A which make low resistance contacts with P type Germanium. P type germanium in a thin film is ~~[illegible]~~ laid down on the block and is heat treated so that the antimony produces a thin section of N-type around B. The amplifier is operated with A_2 negative so as to draw current in the reverse direction through J_2. The potential distributions

FIGURE 30. *How a disclosure of 31 December 1947 in Shockley's notebook should have, in spite of its practical shortcomings, suggested that minority carrier injection would be important in making transistors.*


drawn in the customary way is shown above.
If B is now made negative, it tends to wipe out the potential minimum over B and thus permits flow of holes directly from A, to A.
If the heat treatment is such that the high resistance barrier barely reaches the surface, then the potential energy distribution will vary in depth as indicated below:

making B negative will raise all of these curves but since the curve near the surface has the lowest minimum, many more holes will flow there than near B. Hence the modulated current will be very small compared to the control current.
The desired … compromise towards (B) near the surface may be achieved by treating the surface so as to convert it to P type after or before the N-type region is produced.

FIGURE 31. *A description of the proposed mechanism of operation of Shockley's 31 December 1947 conception showing how it missed out on minority carrier injection from the emitter.*

generation of minority holes in n-type material and how their contribution to the saturation reverse current of a p-n junction depended on their diffusion length. I then had the bum-hunch that the activation energy for reverse current would measure the energy gap. But in any event, in April 1947, I had in hand all the necessary mathematical machinery needed to derive the p-n junction current-voltage formula that would have included injection from a forward biased emitter junction but had simply done nothing about it.

There had been at least two other opportunities in 1947 for me to think of injection. In April, I had proposed a lightning arrestor composed of a sequence of very thin p-type and n-type layers in series. In a high electric field, the shallow potential hills would be flattened out so that carriers of either sign could go straight through: i.e., a form of electric breakdown would occur. In September, I had proposed a high speed thermistor that also involved minority carriers passing over the potential energy maximum for them in a layer of the opposite conductivity type. But for none of these ideas had the concept of injection of significant densities of minority carriers been singled out as potentially useful.

The completion of the basic junction transistor concept came about as an accident—one for which I had a prepared mind. It had features of respect for the scientific aspect of the practical question of how exactly the point-contact transistor works and also of creative-failure in the sense that what came out of the effort was not really what I had been looking for at the time. It all happened 23 January 1948.

4.2. The 23 January 1948 Disclosure

23 January 1948 was the day when I made notebook entries that disclosed the basic conception of the junction transistor including several closely related inventions. I was not trying, as I was on New Year's Eve, to invent an amplifier. Instead I was trying to devise experiments to determine whether or not an inversion layer really played an important role in the point-contact transistor. I had speculated about placing an n-type contact on top of the assumed p-type inversion layer. I thought that this contact might adjust to the electrons of the underlying n-type body rather than to the potential of the holes in the inversion layer. This led me to think of a structure having three layers of semiconductor of alternating conductivity type. I had considered such structures before but not in terms of a three-terminal amplifying device. As soon as I became really familiar with the three-layer concept and the possibility of current flow between the two outer layers by minority carriers diffusing through the middle layer, my reservoir of orderly logical patterns, as represented on figure 1, came into play and I had the payoff-hunch: I recognized that one of the p-n junctions could be reversed biased, as for my concept of 8 December 1947 of voltage gain, and that the minority carriers could control the reverse current. I recognized the structure for a p-n junction semiconductor amplifier—later to be called the junction transistor.

The resulting disclosure, written on a pad at home and later rubber-cemented in my notebook, is shown in part on figures 32 to 36. Their content is discussed in the captions. They were witnessed 4 days later on January 27 by J. R. Haynes. Figure 37 shows the four corresponding figures from patent 5 including the electrolyte-junction structure, the "near miss" of figure 30, the structure of the original disclosure of figure 32, and finally a more orderly, three-layer-sandwich, junction-transistor structure. I have quoted above Claim 29 of patent 5 and discussed how it was so contrived as to read all the examples of figure 37. In contrast, Claim 1 reads simply and directly on the sandwich structure:

"1. A solid conductive device for controlling electrical energy that comprises a body of semiconductive material having two zones of one conductivity type separated by a zone of the opposite conductivity type, said two zones being contiguous with opposite faces of said zone of opposite conductivity type, and means for making electrical connection to each zone"

Claim 1 is a straightforward definition of the junction transistor. A satisfactory reduction to practice of the Claim 1 structure was not achieved until the middle of 1950. I wonder how Claim 29 would have stood up in a patent suit if the teaching to achieve Claim 1 had been challenged. Actually by the time patent 5 had issued, techniques for fabricating Claim 1 structures were well along.

I have referred above to my conjecture that the junction transistor would probably have been invented later in 1948 by someone else at Bell Laboratories if I had not already disclosed it in January. The key item in my reasoning is an observation made and recorded in his notebook on 13 February 1948 by John N. Shive and subsequently reported by him in a conference that I attended 5 days later. Shive had tried a new way to bring emitter and collector points very close together. He made a thin "sliver" of germanium and placed the points on opposite faces. He obtained good transistor action. This success was hard to explain if transistor action occurred through an inversion which in his arrangement would have required a much longer path over the surface from emitter to collector than would have resulted in power gain if the points had been on the same face of the specimen. Shive's notebook entries between 13 and 18 February included some

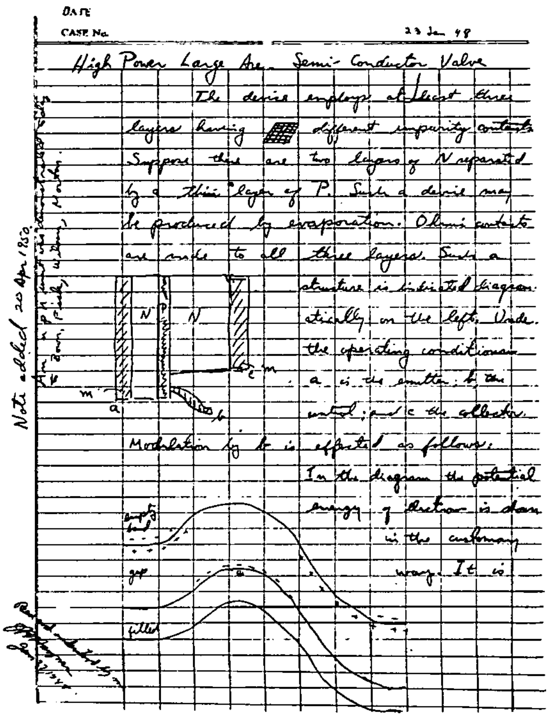
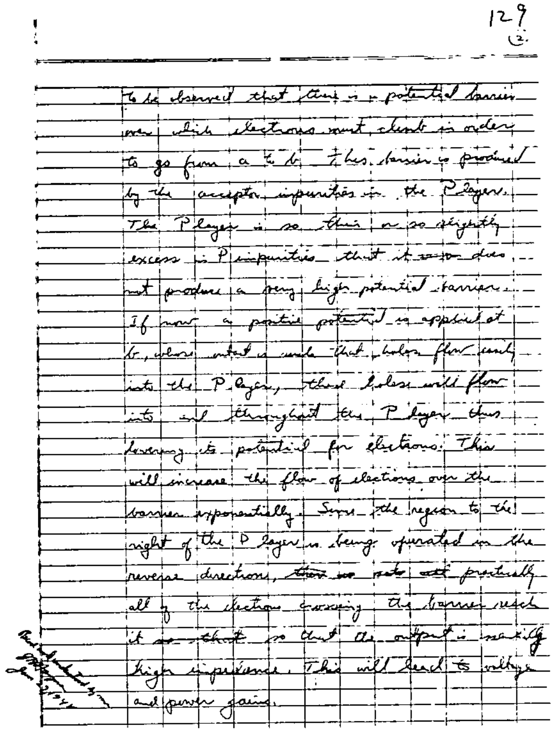


Figure 32. *The first of five pages dated 23 January 1948 from Shockley's notebook containing the record of the conception of the junction transistor.*

It proposes using evaporation for fabrication – a poor idea. But it does lead into the concept of minority carrier injection through a thin base layer.

speculations about mechanisms that would permit holes to flow through the thin specimen from emitter to collector. These mechanisms included the formation of p-type regions and the spreading of wide space charge layers – concept on a par with some of my bum-hunches, for example, figures 30 and 31. However, none of Shive's proposed mechanisms included injection of minority carriers that then diffused through the base layer until collected. Incidentally, it was Shive himself who later introduced the words "minority" and "majority" that have proven to be so convenient to describe such effects.

I recall being startled when Shive presented his findings at the conference on 18 February 1948. My junction transistor concept had been witnessed but had not been generally promulgated. I felt that I did not want to be left behind on this one much as I recall feeling that I had been on the voltage-gain idea of 8 December 1947; Bardeen had then effectively used creative-failure methodology by converting my very half-baked, voltage-gain idea into a big forward step towards the point-contact transistor that he and Walter Brattain achieved 8 days later. At the conference where Shive presented his observations, I


129
(2)

to be observed that there is a potential barrier
over which electrons must climb in order
to go from a to b. This barrier is produced
by the acceptor impurities in the P layer.
The P layer is so thin or so slightly
excess in P impurities that it does
not produce a very high potential barrier.
If now a positive potential is applied at
b, whose contact is made that holes flow easily
into the P layer, these holes will flow
into and throughout the P layer thus
lowering its potential for electrons. This
will increase the flow of electrons over the
barrier exponentially. Since the region to the
right of the P layer is being operated in the
reverse direction, practically
all of the electrons crossing the barrier reach
it so that the output is naturally
high impedance. This will lead to voltage
and power gain.

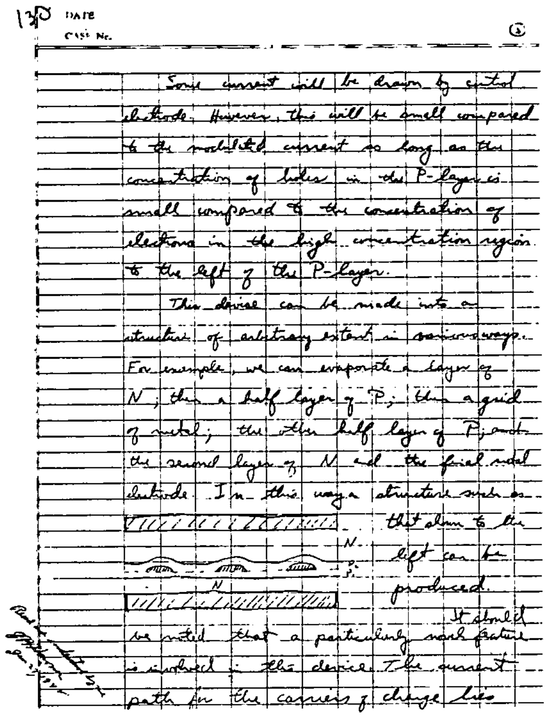
FIGURE 33. *The second page of the 23 January 1948 disclosure of the junction transistor.*

The basic amplifying considerations are clearly explained.

made it a point to explain how they could be interpreted by my carrier-injection model of an emitter junction. If I had not given this interpretation at once, I am sure that Bardeen or someone else would have proposed minority-carrier injection within a very short time—possibly even during the next 5 minutes. From that point on, the concept of using p-n junctions rather than metal point contacts would have been but a small step and the junction transistor would have been invented.

These observations suggest that the proof provided by the success of the point-contact transistor would have so stimulated "the will to think" that, after Shive had tried the radical experiment of points on opposite sides, the thinking needed to complete the invention of the junction transistor would inevitably have occurred and probably at most no more than a few months later.

The junction transistor, in one embodiment or another, was the amplifying device that played so large a role initially in solid-state electronics that it is fair to say that its realization was the development that did effectively launch the solid state era of electronics. It continues to dominate bipolar circuitry.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Some current will be drawn by control electrode. However, this will be small compared to the modulated current so long as the concentration of holes in the P-layer is small compared to the concentration of electrons in the high concentration region to the left of the P-layer.

This device can be made into a structure of arbitrary extent in various ways. For example, we can evaporate a layer of N; then a half layer of P; then a grid of metal; then the other half layer of P and the second layer of N and the final metal electrode. In this way a structure such as that shown to the left can be produced.

It should be noted that a particularly novel feature is involved in this device. The current path for the carriers of charge lies

FIGURE 34. *The third page of the 23 January 1948 disclosure including an appreciation of the importance of heavy doping of the emitter to reduce unwanted base-to-emitter currents.*

A half-baked idea about metal electrodes in the semiconductor to reduce base series resistance is discussed.

But this launching required much more than the concept disclosed in the junction transistor patent, patent 5 of table 1. The specification of that patent did teach those skilled in the art to make junction transistors—indeed before the junction transistor patent issued—but not until other inventions were made that permitted converting the concept of the junction transistor patent into a working reality.

4.3. Making the Junction Transistor a Reality

The tempo of the magic month was followed by a period of more gradual development. New technology needed to be developed to conquer the fabrication problems of the junction transistor. Morgan Sparks undertook a program of trying to make good p-n junctions. The first major effort consisted of dropping molten germanium of one conductivity type onto heated plates of the opposite type.

24 March 1949 dates the notebook entry of figure 38 describing how Robert Mikulyak, working under Sparks' direction, produced a form of junction transistor. A drop of molten p-type germanium fell onto a heated slice of n-type germanium. The solidified droplet was later polished down so as to make a slab structure.

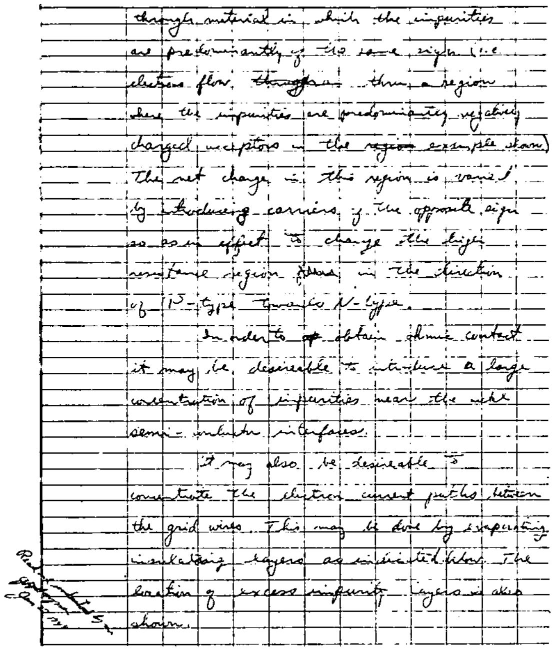

through material in which the impurities
are predominantly of the same sign (i.e.
electrons flow) through then a region
where the impurities are predominantly negatively
charged acceptors in the region example shown)
The net charge in this region is varied
by introducing carriers of the opposite sign
so as in effect to change the high
resistance region from in the direction
of P-type towards N-type.
In order to obtain ohmic contact
it may be desirable to introduce a large
concentration of impurities near the metal
semi-conductor interfaces.
It may also be desirable to
concentrate the electron current paths between
the grid wires. This may be done by separating
insulating layers as indicated below. The
location of excess impurity layers is also
shown.

FIGURE 35. *The fourth page of the 23 January 1948 disclosure containing one idea that became a division of patent 5 of table I: the importance of heavy doping under the metal contacts to reduce contact resistance.*

6 April 1949 dates the notebook entry covering the resulting transistor. It was done as shown on figure 39 by sawing through the p-type drop with a thin wire so as to leave two parallel bars of p-type sitting on top of a large block of n-type. If a junction transistor of proper form is described as a sandwich with a base layer of cheese lying between emitter and collector slices of bread, then Mikulyak's transistor consisted of two fingers of bread lying on top of a cake of cheese. As his notebook entry shows, this device did give power gain. It may be the first example of a transistor with p-n junctions for both emitter and collector. It did not, however, achieve the structure of Claim 1 of patent 5 discussed above.

The essential missing ingredient needed to put the junction transistor across was good crystal growing techniques and the use of compensation of donors and acceptors through the technique of "double doping." The crystal growing story is an important one. Gordon Teal who had worked on semiconductor preparation towards the end of the war was enthusiastic about growing large crystals for purposes of both research and development. He was unsuccessful in selling this program in the chemistry department and to me. My position at the time was that we could do adequate scientific research by cutting specimens from the relatively large crystals that appeared naturally in the polycrystalline ingots resulting from solidified melts, Gordon Teal bootlegged on a shoestring basis a program of preparing a crystal grower without an official authorization. (Indeed, he reminisces about fears imparted to him by manage-

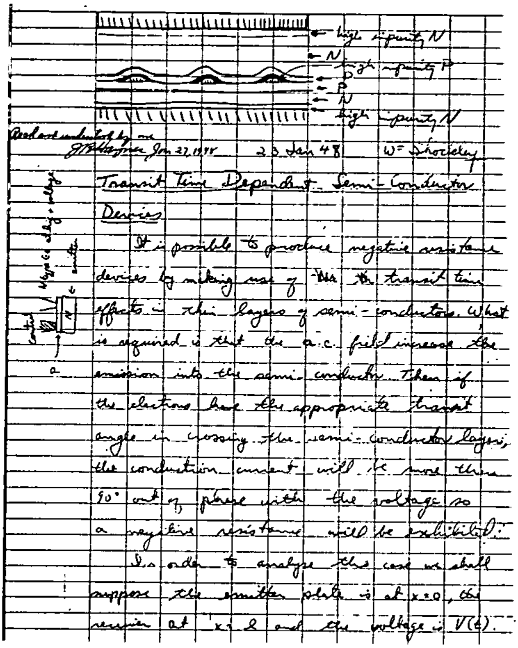


FIGURE 36. *The fifth page of the 23 January 1948 disclosure discussing what might be called "ribs" of high doping in the base. This diagram does not appear to follow what was called for on the fourth page.*

Also on this page is the start of a discussion dated 24 January 1948 of negative-resistance transit-time devices. This topic became a continuation in part of patent 5.

ment that his obstinacy might cause him to lose his job.) Later, J. A. Morton, who had the responsibility of developing transistors for manufacture, bought Teal's program and supported it. Morton wished to avoid having grain boundaries cause variations in point-contact transistors.

Subsequently, the proper way to produce good p-n junctions was discovered by Sparks and Teal to consist of changing the composition of the melt as the crystals grew. Good p-n junctions were produced from apparatus like that of figure 40.

12 April 1950 dates the notebook entry of figure 41 that shows how Morgan Sparks chemically etched a specimen made by double-doping so as to produce the desired three layers. The etch raised the base layer so that it could be contacted by soldering to it.

Figure 42 shows the first, large-area junction transistor that was made by this means in the late spring of 1950. It was embedded in wax and soldered to a large block of copper to provide cooling. It had a thick base layer that produced such long diffusion times that it would not operate above about 20 Kc. However, it did produce 10 or 15 watts of audio power. In the summer of 1950, I discussed it at a semiconductor conference in Reading, England, and described its characteristics. These were in keeping with the theory that I had published in 1949 in the Bell System Technical Journal about p-n junctions

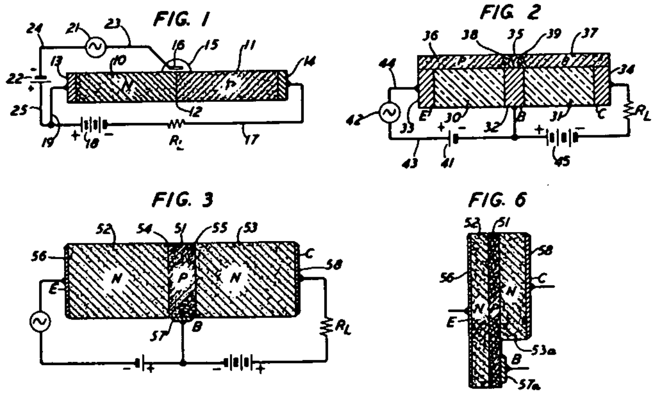


FIGURE 37. *Figures from the junction transistor patent, patent 5 of table I.*

Figure 1 shows the drop of electrolyte across the *p–n* junction, the structure discussed as Experiment II in Brattain's notebook entry of 4 December 1947 reproduced in figure 22. Figure 2 is the conception of 31 December 1947 reproduced in figure 30. Figure 3 is a true junction transistor structure. Figure 6 is a close copy of the diagram of the first theoretically correct junction transistor disclosure as shown in figure 32.

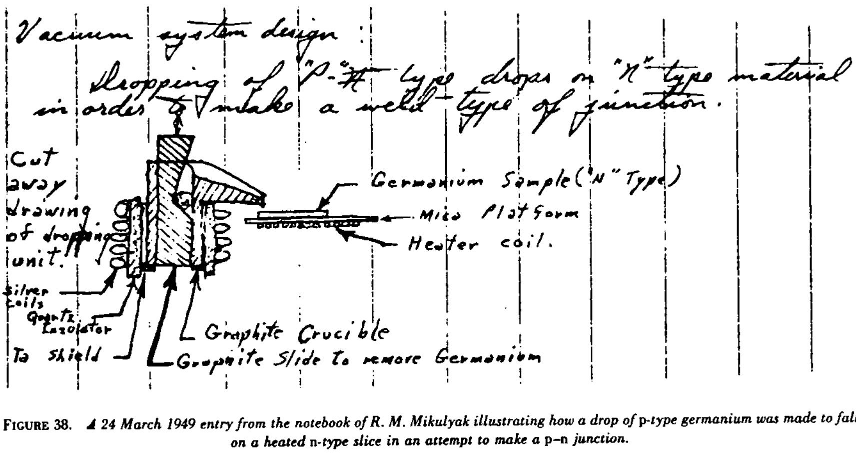


FIGURE 38. *A 24 March 1949 entry from the notebook of R. M. Mikulyak illustrating how a drop of* p-*type germanium was made to fall on a heated* n-*type slice in an attempt to make a* p–n *junction.*

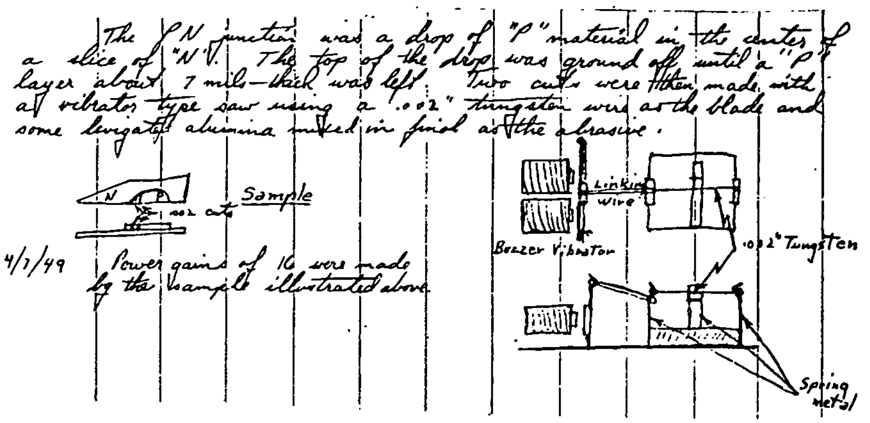


FIGURE 39. *A transistor using p–n junctions made from a specimen prepared by letting a drop of molten p-type germanium fall on a heated n-type base gave power gain on 7 April 1949.*

and junction transistors. This post-deadline paper that I presented at the Reading meeting was omitted from the report of the meeting. While giving lectures in London during the 25th Anniversary celebration of the invention of the transistor—one of the lectures upon which this presentation is based—I remarked on this omission. A member of the audience then reminded me that in 1950 I had been unwilling to reveal how the transistor had been fabricated by double doping. This was probably why it was not included in the reports of the transistor conference at Reading University in 1950.

December 1950 was the date at which significant hastening of the junction transistor development occurred. Because of my experience in operations research during World War II, I had been called in the late fall of 1950 to consult on operations research on our combat forces in Korea. I found that proximity fuses were not in use in mortar shells. Proximity fuses would have been very important in fighting that limited war. After my subsequent contacts with the Quartermaster General as a member of another consulting group, I urged that transistors should be actively considered for use in proximity fuses. I made further inquiries at Bell Laboratories and was convinced by R. L. Wallace, who was an expert on transistor circuitry, that a good, small-area, high-frequency junction transistor would be far superior to point-contact transistors for proximity fuses. No such transistors had then been made.

This stimulated us to take up the double-doping program in which no one had shown any interest. In January of 1951, Morgan Sparks succeeded in growing new crystals with suitably thin base layers. These were cut into rods having small cross sections and which were made into individual transistors. Figure 43 shows an example. True to Wallace's prediction, these had enormous technological impact. The public announcement of these microwatt transistors occurred in the summer of 1951. This is the date at which I feel the transistor era was really finally launched.

When I am asked how we felt about the transistor's future shortly after its invention, I reply by quoting a paragraph written in mid-1950 from my book "Electrons and Holes in Semiconductors":

"It may be appropriate to speculate at this point about the future of transistor electronics. Those who have worked intensively in the field share the author's feeling of great optimism regarding the ultimate potentialities. It appears to most of the workers that an area has been opened up comparable to the entire area of vacuum and gas discharge electronics.

FIGURE 40. *A crystal-growing apparatus of the form developed by G. K. Teal being observed by Mr. E. Buehler, who grew most of the crystals used in semiconductor development and research at Bell Laboratories for many years, and M. Sparks who fabricated the first of the good junction transistors.*

Already several transistor structures have been developed and many others have been explored to the extent of demonstrating their ultimate practicality, and still other ideas have been produced which have yet to be subjected to adequate experimental tests. It seems likely that many inventions unforeseen at present will be made based on the principles of carrier injection, the field effect, the Suhl effect, and the properties of rectifying junctions. It is quite probable that other new physical principles will also be utilized to practical ends as the art develops."

To speculate here about the future of solid-state electronics is not part of a presentation on the invention of the transistor.

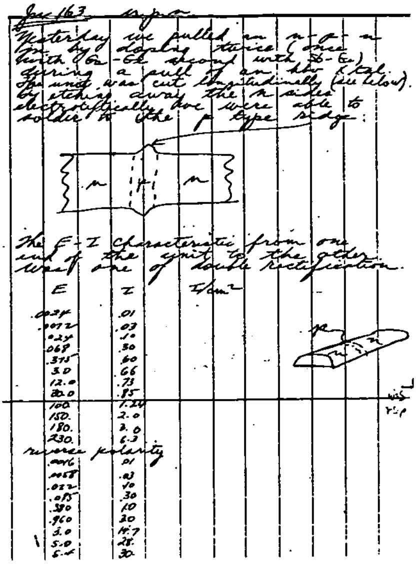


FIGURE 41. *A disclosure of 12 April 1950 by Morgan Sparks of his technique for making a base layer for contact in fabricating a junction transistor.*

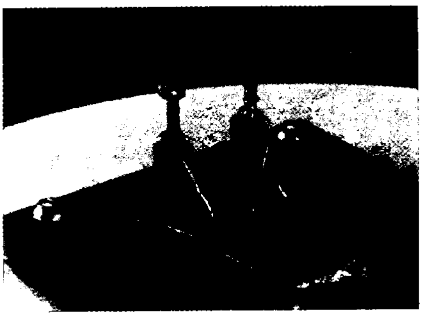


FIGURE 42. *The first successful, sandwich-structure, junction transistor.*

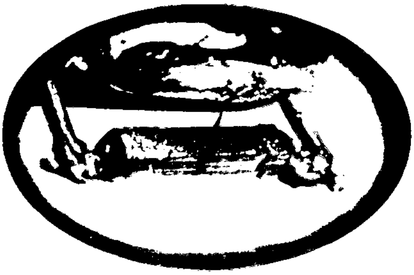


FIGURE 43. *An example of the first micro-watt junction transistor, the device that may be said to have launched the transistor era.*

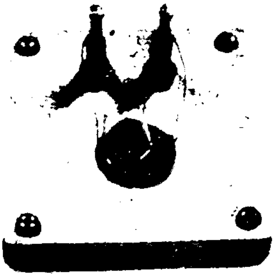


FIGURE 44. *The junction transistor displayed for many years on the concourse at the Murray Hill Laboratory of Bell Telephone Laboratories.*

Was it really the original junction transistor?

4.4. Conclusion

I shall close the junction transistor story with an anecdote—actually still one more example of a form of creative-failure methodology. The original junction transistor of figure 42 would have been lost if Bob Mikulyak, who had helped to make it, had not retrieved it from refuse that had accumulated during a clean-up campaign. Consequently, he was later able to supply it in response to its need to appear in a transistor exhibit for display at the Murray Hill Laboratory. He requested that it be returned to him when the exhibit was taken down.

When the exhibit was opened, Mikulyak was surprised to find the label "replica" on the junction transistor as shown in figure 44. I suggested that the "replica" label was a device to protect this valuable original—a bum hunch as we were to discover.

When new building construction started at Murray Hill in 1972, the exhibit was removed and the transistor again disappeared. Mikulyak once more succeeded in retrieving it from refuse.

I learned this story while preparing lectures for the 25th Anniversary of the transistor. I talked to the Publication Department about preparing a slide showing this transistor being held by Mikulyak who had been involved in its original creation and twice in its subsequent preservation. I was disconcerted to be told that the original junction transistor was carefully preserved and known to be at the moment in the possession of AT&T. Furthermore, it would not be made available to be photographed with Mikulyak because it was being photographed in a movie about the 25th Anniversary.

To determine the fact about the "replica," I asked Mikulyak to give it an ohmmeter test. He called back to report that it was indeed a replica. He had not needed an ohmmeter. A fingernail test sufficed. The "copper block" of figure 44 was plastic painted with copper paint.

This anecdote is my closing example of creative-failure methodology—one of many which occurred in the history of the transistor program. The practical output was my persuading Bob Mikulyak to be photographed, as shown in figure 45, as I awarded him with the replica.

FIGURE 45. *The presentation in September of 1972 to Robert Mikulyak of the junction transistor exhibit at Murray Hill.*

To summarize in a few words the intended impact that I hope this paper may have upon creativity, I use a picture that was given to me after I had rehearsed it at Bell Telephone Laboratories in February of 1973 by a member of the audience. He felt that this picture distills the essence of creative-failure methodology. It read:

"When life gives you lemons, make lemonade."

I have since found this quotation to be an applicable comment for me to make about other research areas in which I have worked as well as to transistor electronics.

Discussion

Question: Who owned the patents?

W. Shockley: Bell Labs owned them in the United States until sometime in 1956 or thereabouts, when I joined up with Beckman to try starting something. We paid them $25,000 for patent rights, and then the consent decree came in and the patents would have been for free anyhow. I don't think I'd like working in a place where your rewards were so dependent upon the value of the patent.

J. Rabinow: I think this is a point that's worth taking up. I don't want to belabor this business of what an inventor deserves when he works for a company, but, Bill, I think that was oversimplified this morning, that if you invent you get so many dollars; I think any intelligent management would weigh all the other factors, like how secretive you are as against how cooperative you are.

As I listened to your story, which is a very fascinating thing, how haphazard inventions are made, even by intelligent people, I was wondering: were you aware of the commercial importance of it, in getting your names on the thing and rushing, as you were, because you knew this was scientifically very important? I don't question the motivation of the invention, but you were obviously very anxious to get it on paper, to document the exact dates. Was that because of commercial importance or because of the fact that you wanted your name on them for future glory? And I don't mean to belittle the glory.

W. Shockley: I think it was a mix of the two, and I'm not sure how well I could balance that out.

J. Rabinow: Did you realize it was going to be commercially very important?

W. Shockley: Well, certainly by 1950, when I wrote a book. I think we felt it would be very important right from the start, simply because we knew we were doing something new and different. As soon as it had worked, the reaction of some of the managerial people at Bell Labs, of course, was very clear. This was really a well-held secret up until this public announcement.

J. Rabinow: When people talk about great inventions, you find that most of them come out of small laboratories; this is a special case, and the interesting thing about the transistor is that you had a ready-made market, once you recognized how important it was. If you were in some little company and came up with a point contact transistor and tried to sell it to a big company, I think you wouldn't sell it. They would say, "Well, wait till it works right, wait till all the problems are licked." But because you had your own customer built in, a tremendous customer, and your own money, I think this is a most unusual combination. Almost no invention is made by the company that is its own user. Usually they sell it to somebody else, and that was a very special case.

I. Kayton: I joined the Labs in 1952, and at that time I was told a story given as the truth—perhaps it was apocryphal—that the patent department and management so fully realized the commercial importance of your contribution that in preparing the patent application they had three of their very best patent attorneys write applications independently of each other, and that they selected the ones that were best. This was told to me by a division patent counsel, and I was wondering if that were true.

W. Shockley: I don't remember that story; I doubt it, but I could be wrong. I wrote most of the disclosure on the junction transistor patent myself, and this may have been the first time I really worked closely with Rudi Guenther, but we did a lot of things otherwise. So I'm not aware of that on the junction transistor patent. The thing that impressed me on coming back to it was the ingenuity on that Claim 29, and some of the other claims.

J. Stephens: Was the generality of that first patent application the work of the attorneys, or the inventors?

W. Shockley: You mean Claim 29? I don't remember having appreciated that; if I'd thought that thing up and realized what it was, I would have had no doubt that I had made a big contribution. It must have been that Guenther did it and I didn't even bother to read it.

A. Ezra: What led you guys to start doing research on semiconductors anyway? Was it a result of a market survey that said, if you invent a transistor you will make a lot of money?

W. Shockley: No, the work on the semiconductors was part of the really very wise and farsighted research policies at Bell Labs. The organization there was a concept of Mervin Kelly's, which he came to toward the end of the War. Brattain was away working on magnetic anomaly detectors, Bardeen had not been hired, and I was doing operations research in the Office of the Secretary of War. He visualized this group and set up two co-heads. This was a solid state physics group; the other fellow was S. O. Morgan, who had one ingenious patent worth mentioning; he was particularly concerned with dielectrics and piezoelectrics, and I think he had the first patent on using heavy hydrogen, deuterium, to alter critical temperature of a piezoelectric. There were several areas of this, all of which had to do with the properties of crystalline material which were relevant to phenomena that might be used to control electrical signals. And that's as closely defined as the work was. That's typical of the type of research emphasis they would have. Ferromagnetism was one, dielectrics was another; piezoelectrics was another, semiconductors. I think there were five; I seem to have left one out.

I remember at another time, myself, trying to sell Bell Labs on doing something in the nuclear physics fields, of getting some Van de Graaff generators. They weren't so interested in the nuclear pile business, because these were things in which other organizations might well have a much larger fraction of their interest, than the Bell system. But in terms of these materials, in influencing or controlling signals, which are so central to the whole communication business, there this was a very logical business. In that, the sort of freedom and support we had was high.

I remember one other thing that we were wise about; it has to do with respect for the scientific aspects of practical problems. I recall some people coming around and saying, "Well, you're working on silicon and germanium; those are really pretty well under control, why aren't you working on selenium or copper oxide or nickel oxide—those are the things we are using in the telephone plant?" Well, these we knew were relatively messy things; besides that, there were some gaps in the science of silicon and germanium, and they were pretty well understood. That, I think, is another sound research principle, that if you are trying to extend a field, sometimes one of the best ways of doing it is to find a place in which you are maybe most advanced, but there's another step, you don't know where to go. Well, the surface state was exactly that sort of thing. I remember before we had any low-temperature stuff, Gerald Pearson and I went down to the Bureau of Standards and borrowed low-temperature equipment and cooled off silicon and germanium specimens; and later on Bardeen and Pearson wrote papers on this. So the freedom to work on this basic stuff was very high. It fitted into a research principle that I haven't tried to quote for a long time that came from Ralph Bown, who was at that time Director of Research and later Vice President for Research, and was a very thoughtful, eloquent man. He wrote the foreword to my book on *Electrons and Holes in Semiconductors.* He talked about several freedoms: one was the freedom to do things, and another one was the freedom to refuse to do things. And we didn't have anything to do with these suggestions that we should look hard at these practical semiconductor materials.

J. Rabinow: I once heard Dr. Kelly speak, and he said that one of the things that he did when he became the Director of Bell Labs, was to shake the place up—that he reorganized it. He said that every laboratory should have a boss that comes in once in awhile and reorganizes and changes things, just because it's necessary to do it for the good of society.

LECTURE DELIVERED AT THE BANQUET OF THE THIRD NAVY MICROELECTRONICS PROGRAM CONFERENCE ON APRIL 5, 1965

(By William Shockley)

After Dr. Shockley turned the tables on the Master of Ceremonies and gave an impromptu humorous bit of background information on Dr. Arnold Shostak, Office of Naval Research, he began his off-the-cuff talk, amidst silent expectations of the assembled dinner guests. The theme of the evening's delivery turned out to be a two-fold premise which went somewhat like this:

1. Advocate respect for the scientific nature of practical problems, and
2. Recognize the inadequacies in the "law of excluded optimum", as far as Government Agencies are concerned.

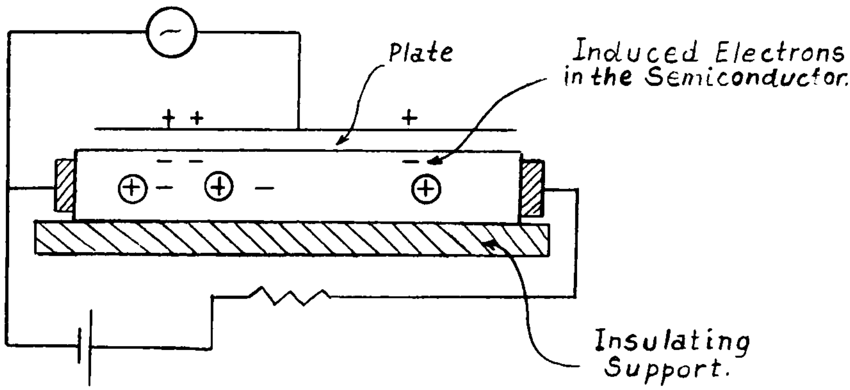
Dr. Shockley proceeded then to illuminate his point by making reference to the chronology which led ultimately to the discovery of the transistor and subsequently to our present-day microelectronics. A page from Dr. Shockley's own notebook entry was projected on the screen, revealing that back in 1939 (i.e., at 4:15 a.m., 12–29–39) he was proposing to Bell Telephone Laboratories the realization of a semiconductor amplifier to replace vacuum tubes. After crediting Schottky's investigations, a second part of the same Notebook entry of Dr. Shockley's was shown, in his neat, legible, scientific handwriting. It was dated "Friday, at home in Gillette", and was witnessed on February 27, 1940. It pertained to his suggesting a "Semiconductor triode or amplifier", to be made with copper oxide and with grid-like wire, much like Lee de Forest did with his audion tube. Dr. Shockley then foresaw its use in amplification and modulation, truly like the modern field-effect devices of today.

The question may justifiably be asked: what is the relationship between these old pieces of history and microelectronics? Was the discovery of the transistor—

1. The result of a planned industrial research program, or
2. Was it sheer lucky accident, as part of basic research?

In all fairness, one must concede, the above two questions are lousy alternatives. It was neither, or it was both, as after the five-years interruption of World War II it took the Bell Laboratories' research team from 1945 to 1948 to come up with the new device, the transistor.

Dr. Shockley drew up his earliest post war concept on the blackboard, representing a theoretical-existence proof of the possibility of making an amplifier. It was a piece of insulator, with a film of semiconductor material on it. He applied voltage, as per sketch below, and postulated charges that would momentarily increase the conductivity at will, like the response to grid bias in a vacuum tube.



This approach, however, did not work; the anticipated effects were not detectable. Why not? The investigators shifted position and asked themselves: what's wrong with our physics? Could it be that those extra charges were not like ordinary, movable charges? Indeed, they were not, but rather the harbingers of a then unexploited concept, the "surface states" (investigated by Bardeen). This conceptual model, introduced by Bardeen to explain any observable effects, focused attention on the scientific aspects of the practical problem of achieving amplification. Bardeen and Brattain then developed the foundations of semiconductor surface state science.

As an aside, Dr. Shockley mentioned other aspects of research on practical problems by discussing degradation of germanium merely from repeated handling. This goes back to the minute amounts of copper adhering to human fingers, causing degradation to Ge in the presence of heat. The investigator's approach was to "attempt to enhance the problem, in the hope of contributing materially towards establishing its nature". In this case, by introducing Cu deliberately.

In their surface-state research, Barden and Brattain used an electrolytic solution and utilized the bulk material with point contacts to produce an amplifier. The transistor actually was named by John Pierce.

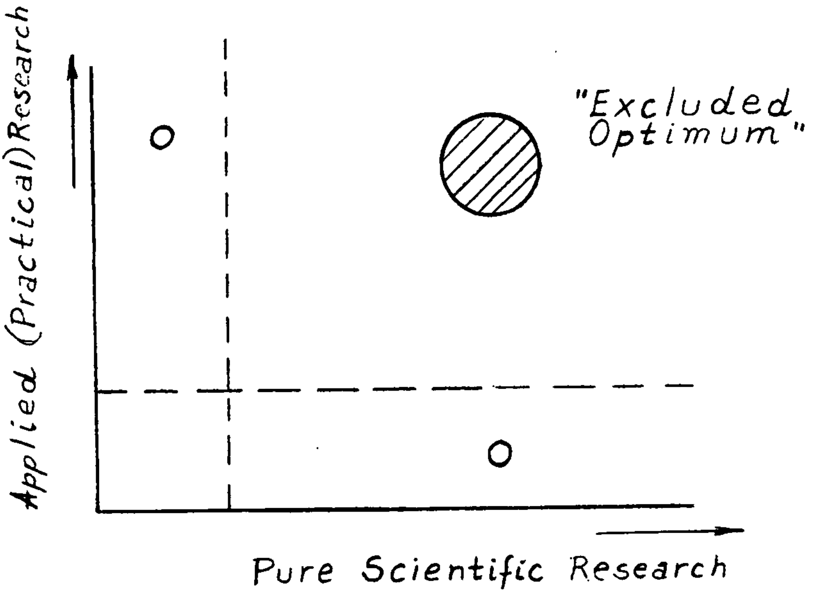
One might be tempted again to ask now:

1. Was it a well-planned industrial program? Or
2. Was it a lucky accident as happens in research?

The answer is: neither! It was simply to illustrate the variety of ways one could show "scientific respect for the practical problems" and come up with a practical payoff. At the time, the investigators did not understand semiconductor behavior in the presence of outside fields. Subsequent studies in surface states, in ion-drift phenomena, and in avalanche breakdown followed.

The lecture thus far served well to emphasize premise #1, namely, the importance of "respect for the scientific nature of practical problems". Let's see our second premise now.

When Dr. Shockley or his associates want to exploit some of the newly established scientific territories of focal interest, they try to commit some Government funds. When they go to some typical Government agency or office and start talking about, say, diffusion of donors along dislocations in grain boundaries as a possible basis for improved transistor technology, the reaction is usually the same, almost mathematically predictable! Each office will want to hew close to their primary assigned areas, thereby giving rise to the law of the "Excluded Optimum"! The sketch below will illustrate this point concerning our stated second premise:



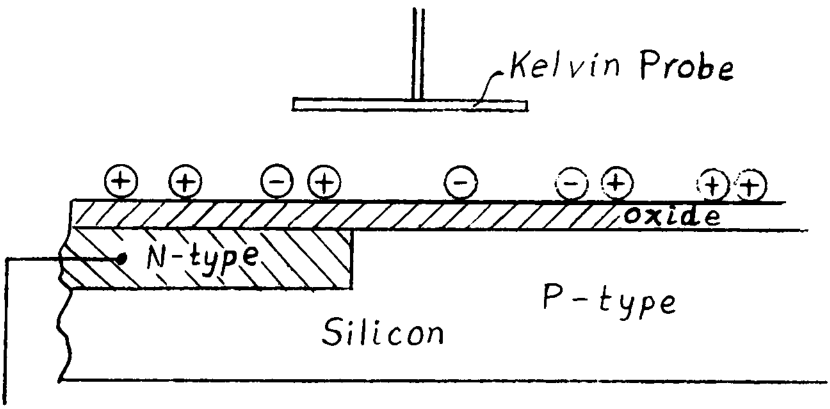
On this basis, work on semiconductor surface states which led to the transistor would have been rejected by the "basic" agencies because it had a practical motivation and by the "practical" agencies because it required developing a science. Indeed, the optimum program that led to the transistor and to its extension into microelectronics, could easily be excluded by present-day contracting procedures.

In the course of his talk Dr. Shockley stressed the relevance of the future of microelectronics to oceanic research and problems of the Navy. Taking, e.g., disposable sensors, microelectronics will make it possible to pack intelligence into small, self-contained packages so that exploration and reporting at considerable distances from a ship or submarine becomes a reality. Disposable sensors, considered so because of the low cost with which functions are then to be carried in an expandable device, are foreseen to emerge from the deep, after having activated themselves, and to surface so that they can be played back, for obtaining scientific information.

The development of single crystals at Bell Telephone Laboratories was provoked by practical requirements and the first really good single crystals came about at the instigation of the semiconductor group, not due to the Pure Research people, as might be anticipated. Dr. Shockley considered his failure to urge a strong single-crystal program as a serious mistake. He then added the remark with which he used to encourage his graduate students, namely: "I do not make progress by always avoiding mistakes or errors, I think of ways to correct them."

The last part of the lecture was more in an educational vein.

A prominent example of scientific investigation prompted by practical problems was Atalla's work on ion drift in semiconductor devices. What goes wrong with p-n junctions in the presence of applied potentials and under conditions of humidity? The industry was baffled by device failures: If a reverse voltage was applied to a p-n junction, it deteriorated until it failed catastrophically. The cross-section shown below was sketched up by Dr. Shockley:



Electric field reaches through the oxide, creating an ion flow. The ion flow will tend to make the surface equipotential. The oxide, with the ions on it, will act like a condenser. Thereby, changes in contact potentials of the order of 50 volts would come about, i.e., changes about 1,000 times larger than ordinarily studied.

To measure Atalla's model quantitatively, a so-called Kelvin-probe was used, based on a technique developed for testing contact-difference of potential. It was ascertained that all the charges building up on the oxide were due to ions sitting on the outside and not due to ions inside the bulk. Focussing attention on phenomena associated with ion drift and related avalanche-breakdown problems, set up a situation in which it was natural to think of the influence that surface conditions would have on avalanche breakdown and thus of possible utilization of avalanche-breakdown control in making amplifying devices. This line of thought led to the Surface Controlled Avalanche Transistor, or SCAT, because of a combined interest in both the theoretical aspects and a motivation to make new-type ampliyfying devices.

The Air Force's "Physics of Failure" program, initiated and funded from Rome, New York, is a good way to get into and to explore those areas of investigation which were up-to-then portions of the "Excluded Optimum".

The possibility of achieving extreme high power gain has been recently opened up.

Magnetic Bubble Technology at Bell Laboratories—An Example of Innovation Through Diversified Skills and Facilities

Bell Laboratories has been interested in developing magnetic bubble technology because of its potential for low-cost, highly reliable memory devices which could operate with very low power. The devices promised to be simple to fabricate and able to store millions of bits of information in a very small area. Their reliability and nonvolatility would make them particularly attractive for storing vital data in telephone company central offices. In the home of a telephone customer, they could be used as a low-cost memory system powered directly through the telephone line.

While you were directly involved in the initial conceptions of magnetic bubble development, a great deal of additional effort was required to progress from the theoretical concepts to practical devices suitable for telephone company applications. Solving the technical problems took five years and required the direct participation of many people representing a diversity of disciplines. And their work drew heavily upon other research work being conducted at Bell Labs during this period. Currently, some 60 members of our staff are involved in the development program.

From the initial discovery that these small magnetic domains, embedded in a medium of opposite magnetic polarity, could be moved rapidly within the magnetic material and thus record and store data, much research and development work was required to bring the potential capabilities to fruition. Some phases of this activity are described below:

DEVELOPMENT OF EFFECTIVE MATERIALS SYSTEMS

To be practical, the magnetic material had to contain a high density of bubbles per unit area, and these bubbles had to have high mobility. The material also had to be mechanically, chemically, and thermally stable as well as easy to produce in a mass-production facility.

The phenomenon of mobile magnetic bubbles was originally investigated in rare-earth orthoferrite crystals. As a commercially viable medium for storing data, however, orthoferrite had serious technical and economic limitations. The bubbles could not easily be made small enough for sufficient density of information storage, and it was difficult to produce detect-free, large, thin slices of orthoferrite using normal flux-growth methods. Growing a magnetic film of orthoferrite as an epitaxial layer matching the crystal lattice of a nonmagnetic substrate required matching lattices in two directions and a high degree of crystal perfection.

Although there seemed to be no fundamental physical obstacle to producing commercially suitable systems, a considerable effort was necessary to realize them. In the course of these studies, however, we developed growing and measuring techniques and methods of propagating, manipulating and detecting bubbles. This work also contributed to our understanding of the magnetic characteristics and our ability to control such properties as bubble size.

In the course of our investigation, stable bubbles were observed in synthetic garnet crystals, our primary materials vehicle. Garnet had long been known to possess properties that could easily be modified, but they were thought to have several equivalent directions of magnetization, making them unsuitable for producing bubbles. Occasional departures from this behavior were explained as effects of strain on the crystal lattice. We were able to demonstrate that their ability to be magnetized in one unique direction was a result of growth-induced ordering of rare-earth metals in the direction of crystal growth.

While this discovery was welcome, the problem of achieving a commercially acceptable growing method for these synethetic crystals still remained. Fortunately, the growth-related properties of the gasnet seemed suited to straight forward expitaxial techniques, anad nonmagnetic garnets were available as substrates. Formidable obstacles still remained. Both substrate and epitaxial layer had to be virtually free of imperfections, the epitaxial film had to be uniform and magnetized in the proper direction, and appropriate solvents and thermal and geometric conditions had still to be found. We began developing growth methods for the substrate and for the magnetic garnet film. Using precisely controlled film compositions, we were able to achieve close substrate-film matching and produce low-defect films. Eventually, we were able to grow reproducibly uniform films, an essential requirement for widespread application.

HARD BUBBLES IN THE NEW MATERIALS SYSTEMS

Mowever, the development of epitaxial garnets as a new materials system for magnetic bubbles was not problem-free. Some garnet bubbles behaved erratically, in a way that would seriously limit their operation at high speed. These so-called "hard bubbles" moved at a sharp angle to the desired path. Understanding the basic physical cause of hard bubbles was a complicated task drawing on a variety of scientific and engineering disciplines.

The problem was traced to magnetic differences in the bubble wall. When magnetic bubble material is placed in a magnetic field, the magnetization inside the bubble wall is in the opposite direction from that in the surrounding material. In a normal bubble wall, the direction of magnetization changes uniformly, clockwise in some bubbles, counterclockwise in others. With a hard bubble, however, the direction of magnetization jumps back and forth from clockwise to counterclockwise, producing a complicated magnetic structure that causes the erratic, unreliable motion when a magnetic field is applied.

Of the three practical means for avoiding hard bubble problems, on implantation has proved to be the most useful. In ion implantation—a technique that originated in semiconductor processing—material is bombarded with charged atoms to change the material's characteristics. In this way, we can alter the direction of magnetization of a thin layer near the surface of the film so that it is parallel rather than perpendicular to the surface, and this magnetic "lid" prevents hard bubbles from forming.

PROCESSING TECHNIQUES FOR MAGNETIC BUBBLES TECHNOLOGY

In addition to understanding the basic phenomena of magnetic bubbles and to developing a workable, problem-free materials system, we had to develop techniques for controlling their movement in economically producible devices. To move bubbles of this size required processing techniques with considerable precision of control.

To manipulate the bubbles in the garnet film, a very thin layer of magnetic metal is deposited on top of the bubble material using photolithographic techniques. The pattern for this metal layer is produced on top of the thin metal layer, and the parts of the pattern left exposed are etched away by ion-milling techniques. The pattern used to form the metal layer is produced by a technique known as electron-beam exposure.

The photolithographic processes and the electron-beam exposure system were developed in our in-house semiconductor integrated circuit processing facility. These are very expensive and sophisticated facilities—had we not been in the integrated circuit business for some time, these processes might not have been available to solve the problems of magnetic bubble technology.

In the past, Bell Laboratories had designed thin-film magnetic memories for telephone technology. These memories used in a thin layer of magnetic metal of Permalloy. Indeed, it was our rather extensive knowledge of Permalloy's properies and of techniques for depositing it that led us to apply to it to manipulating bubbles. Thus we were able to install the necessary facilities for processing magnetic bubbles rather quickly.

The electron-beam exposure system and the ion-milling techniques are examples of cross-fertilization in the process of technological innovation. Both were developed primarily as production tools for silicon integrated circuits, not for magnetic bubbles, but both were readily applied to practical problems in magnetic bubble technology.

In the course of our work with integrated circuits, we recognized that pattern delineation is a process basic to much of modern technology. We thus pursued research in ion milling to improve our capabilities. Ion milling is a process whereby low-energy beams are focused on the material being machined. The beam mills away the material to the desired configuration, etching very precisely, atom by atom. Fortunately, our work in ion milling was conducted at the same time—though independently of—our magnetic bubble program. And just when we needed to make very finely featured bubble film patterns, the necessary knowledge and equipment were available.

The electron-beam exposure system (EBES) designed by Bell Laboratories was actually a key feature in the development of silicon integrated circuits. It was a complex program, whose ultimate success has justified its cost. The technique makes high-quality masks, or circuit patterns, with features one micron in size. The method produces integrated circuits with improved performance and a greater number of circuit elements per unit area, an important economic benefit. And the photo-resist Bell Labs developed for the process has been made available to other industries as well.

TESTING OF MAGNETIC BUBBLES TECHNOLOGY

But finding effective, flexible materials and workable production methods for magnetic bubble devices was not the end of the development problems. Theoretical design is not enough—practical application requires performance testing, specific design and development of manufacturing methods. Because of the requirements for high reliability and the sheer numbers involved in memory testing, this is an important and time-consuming step in the development process. Profiting from our experience with other memory systems, we initiated a bubble test program some time ago. To speed ultimate production, we developed test equipment and strategy in collaboration with Western Electric. The equipment is very versatile, since it is all computer-controlled, and we expect to use the same equipment for different device designs, and for diagnostics and basic development in addition to production. Even with our considerable experience with computers, this has been a complicated procedure; our hardware is essentially complete, but a few software bugs remain.

DESIGN FOR MANUFACTURE

To be competitive with the many available memory technologies, magnetic bubbles must exhibit some really practical advantages. Perhaps the most important of these are greater reliability and lower cost, which involve not olny the fundamental device but also its actual design for manufacture. The Bell System decided to form a joint Bell Laboratories/Western Electric task force on magnetic bubbles to develop a reliable memory package that could also be manufactured at low cost. In this way, the packaged magnetic bubble devices used to demonstrate reliability could also be put into manufacture without redesign. The joint task force, headed by a Western Electric Assistant Manager from a production location, consisted of production engineers from Western Electric, bonding and packaging experts from the Western Electric Engineering Research Center, plastics experts from the Research Area of Bell Laboratories, and magnetics and reliability experts from the Electronics Technology Area of Bell Laboratories. This diversified group, which covered the entire development span from fundamental research to production, designed the bubble memory package. The design was so successful in both reliability and cost that the device is now in pilot production at Western Electric and is expected to go into full production next year. It is clear that the assembly of this task force with all its diversified talent from Bell Labs and Western Electric greatly expedited development of these devices.

TELEPHONE APPLICATIONS FOR MAGNETIC BUBBLE TECHNOLOGY

We would not, of course, maintain a research and development program as large as our magnetic bubbles effort if we did not see significant benefits to the telephone system. We are now concentrating on three applications.

One is the repertory telephone, which stores frequently called numbers in its memory and dials them at the press of a single button. This application is designed for household use, so keeping cost low is of primary concern. The task force concentrated primarily on this cost-sensitive application, and we would not have pursued its development had they not projected attractive figures. Another challenging aspect of this task is our intention to power the magnetic bubble memory with the very small amount of power available directly from the telephone line, saving the cost of additional power on the customer's premises. Development of this general-purpose application has had other benefits, since it drew our attention to a number of practical aspects—making sure the device would operate even when dropped, for example.

A second interesting telephone application is voice-message recording. Here, magnetic bubbles must compete with the magnetic tape market. In the telephone central office, many of the voice messages stored as instructions to our customers are rather short, making the overhead costs of magnetic tape systems excessive. In addition, such mechanical systems are not very reliable under the continual use they receive in the central office. In the proposed magnetic bubble voice-message recording system, voice is encoded into digital format and is stored in a quarter-of-a-million bit bubble device. Each device contains four 68-kilobit chips and can store the equivalent of twelve seconds of voice. The system is modular in design, and additional messages can be stored by simply adding magnetic bubble devices.

A third application for magnetic bubble technology is, of course, to replace the very large disk-file memories in our electronic switching systems. Since these memories store the executive—or overall control—program, extreme reliability is necessary. Clearly, one of our problems is to prove that our memory will operate for decades without malfunctioning—we cannot afford comparable time to test this in advance. We are now developing methods of accelerated aging, borrowing techniques from our semiconductor people. But we must develop additional procedures. Although we are beginning to see progress, more work is needed before we can convince our electronic central office systems designers that our device will meet their rigid operating standards.

It is worth pointing out that all of the discoveries described above in connection with the magnetic bubble devices have been made at Bell Laboratories in addition to our many contributions to the techniques for fabricating them. This work so far has resulted in the award to Bell Labs inventors of two prizes (one by the Franklin Institute and one by the IEEE) and the granting of 140 patents.

[From the Electronic News, Jan. 11, 1971]

SILICON VALLEY-U.S.A.

(This is the first of a three-part series on the history of the semiconductor industry in the Bay Area, a behind-the-scenes report of the men, money, and litigation which spawned 23 companies—from the fledgling rebels of Shockley Transistor to the present day.)

(By Don C. Hoefler)

It was not a vintage year for semiconductor start-ups. Yet the 1970 year-end box score on the San Francisco Peninsula and Santa Clara Valley of California found four more new entries in the IC sweeps, one more than in 1969.

The pace has been so frantic that even hardened veterans of the semiconductor wars find it hard to realize that the Bay Area story covers an era of only 15 years. And only 23 years have passed since the invention of the transistor, which made it all possible.

For the story really begins on the day before Christmas Eve, Dec. 23, 1947. That was the day, at Bell Telephone Laboratories in Murray Hill, N.J., three distinguished scientists, Dr. John Bardeen, Dr. Walter Brattain and Dr. William Shockley, demonstrated the first successful transistor. It was made of germanium, a point-contact device that looked something like a crystal detector, complete with cat's whiskers.

The three inventors won the Nobel Prize for their efforts, but only one of them, Dr. Shockley, was determined to capitalize on the transistor commercially. In him lies the genesis of the San Francisco silicon story.

It was only by a quirk of fate, however, coupled with lack of management foresight, that Boston failed to become the major semiconductor center San Francisco is today. When Dr. Shockley left Bell Labs in 1954, he headed first for New England to become a consultant to Raytheon Co., with a view toward establishing a semiconductor firm there under its auspices.

His financial plan called for a guarantee to him of $1 million over a 3-year period—hardly unreasonable by today's standards. But the Raytheon management 16 years ago couldn't see it, so Dr. Shockley left the company after only 1 month.

When Raytheon closed the door, it inadvertently set in motion a whole new cycle which was ultimately to result in the complex of nearly 25 chip makers in the San Francisco Bay Area.

RETURNS HOME

For Dr. Shockley was a native of Palo Alto and a Stanford alumnus, and his next move was to return home. Thus, in 1955, with the backing of Beckman Instruments, in Palo Alto was born Shockley Transistor Corp., the direct antecedent of nearly every semiconductor firm in the Area today.

Despite an uncanny genius for spotting and recruiting talent ("hero worship," admitted one of his early employees), he was less adroit in managing that talent. Within 2 years there was much internal unrest at Shockley Transistor, much of it centered on Dr. Shockley's decision to concentrate on four-layer diodes rather than the product in the company name. A group of palace revolutionaries even went to Dr. Arnold Beckman, president of the parent company, with a plan which would move Dr. Shockley to emeritus status and relieve him of operating control. Dr. Beckman was agreeable, but Dr. Shockley held veto power, and he exercised it.

The young revolutionaries thereupon gave up on Shockley Transistor, and sought backing for a company of their own. The father of one of them had a contact with the brokerage house of Hayden, Stone, which in turn found a willing backer in Fairchild Camera & Instrument Corp., then of Syosset, N.Y. Thus, in September, 1957, only 2 years after Dr. Shockley came to the Coast, was born the most fecund of all the semiconductor firms, Fairchild Semiconductor.

The Young Turks who founded the company—now all departed—were: Dr. Robert Noyce; Dr. Gordon Moore; Dr. Jean Hoerni; Dr. Jay Last; Mr. Victor Grinich; Dr. Sheldon Roberts; Julius Blank and Eugene Kleiner. Dr. Shockley called them "the traitorous eight."

Shockley Transistor never recovered from the blow, although the company staggered along, through three owners, until mid-1968. Beckman gave up and sold the company to Clevite, which gave up and sold it to ITT. ITT gave up, couldn't sell it, and shut it down.

ONLY FACILITY REMAINS

All that remains of Shockley Transistor Corp. Today is an elaborate facility, empty for 2.5 years, at the corner of Page Mill Road and Foothill Expressway in Palo Alto. It still carries the sign, "ITT Semiconductors," but in Stanford Industrial Park, where real estate often commands triple the prices of surrounding property, it is a white elephant no semiconductor firm can afford.

Meanwhile, Fairchild had gone through a trauma of its own. Since all eight of the founders were technologists rather than businessmen, the FC&I management in New York deemed it essential to import a professional manager to run the plant.

Still another UCE-Solitron spinout was James Paris, also ex-Fairchild, who moved east in 1970 to Trevose, Pa., to found Unisem with United Aircraft backing.

BIRTH OF SIGNETICS

Returning to 1961, when Fairchild lost four of its eight founders, another significant spinout that year was Signetics, involving such key people as Dr. David James, David Allison and Orville Baker. Later they brought in F. Joseph Van Poppelen, Jr., from Motorola, first to head marketing, and later as general manager. Original financing was by Lehman Bros., but the New York bankers couldn't stand California-style losses, so they sold control to Corning Glass works.

Corning later sent in James F. Riley to head the company, and his arrival was greeted with a notable lack of enthusiasm on the part of Mr. Van Poppelen and the founding troops. A few months later they organized a cabal which was designed to Deep-Six him, but their security was sloppy. They didn't reckon with paper-thin walls, nor the fact that Mr. Riley was in an adjacent room, overhearing the details of his intended demise.

But he survived, and Mr. Van Poppelen departed, with the comment: "Only a few more months—a few lousy months—and we never would have needed that Corning money."

Signetics has been remarkably free of spinouts, although the firm backed an unusual spinoff in 1969. Orville Baker and David Allison wanted a company of their own, and Mr. Riley was able to convince them that they could do as well remaining under the Corning banner. The result was Signetics Memory Systems, a Signetics Corp. subsidiary with interlocking directors.

There was to have been a third founder, Zeev Drori, then with Fairchild and earlier with IBM. But when he came to the signing to close the deal, he discovered that Corning had inserted a new clause without prior discussion. This had to do with rights to re-acquire founder's stock, and Mr. Drori regarded it as inimical to his interests.

He therefore backed out of the deal at the last minute. Instead he went out and found other backing early in 1970, forming Monolithic Memories with other founders from National and Texas Instruments. Monolithic Memories is confining its efforts to bipolar memories, since one of its backers is Electronic Arrays, an MOS-only house.

While the founding of Signetics Memory Systems kept two of Signetics' most valuable properties in-house, it may have created as many problems for Corning as it solved. The party line is that Signetics is a device company and Signetics Memory is a systems company, and never the twain shall compete. But talk to the marketing troops of the respective companies, and you get quite a different story.

That first general manager was Dr. Ewart M. (Ed) Baldwin, who was recruited from Hughes. Although not one of the founders, he was given a founder's share, plus other incentives whereby he might have made more money than any of the founders.

(Ironically, the success story of the Fairchild founders—which has been probably the largest single factor in stimulating spinouts—was not nearly as successful as is generally believed. Dr. Noyce recently remarked that his total capital-gains taken out of Fairchild amounted to $240,000. Even more ironic, the founders of that spectacular loser, General Micro-Electronics, each walked away with $300,000.)

But whatever Dr. Baldwin might have taken out of Fairchild, he threw it all over in March, 1959, when Fairchild was only 1.5 years old. With backing from Rheem Manufacturing, he pulled out to form Rheem Semiconductor, taking 10 key Fairchild people with him.

Dr. Noyce, the 32-year-old scientist with no management experience, reluctantly agreed to take the top job temporarily, and that temporary job lasted 9 years.

The Fairchild management was furious with Dr. Baldwin, and they sued him and Rheem, claiming, among other things, that he had stolen their cook book. Even more mysterious, they claimed, the process manual was later returned in the mail, in plain envelope, with return address.

COOK BOOK STORY "TRUE"

Digging into the case recently, this writer asked one of the key Rheem people of that day if the story of the cook book were true. "Sure it was," he said. "But the thing that got us off was the doctrine of clean hands. When we told what they did to Shockley, the court was not so impressed with what Rheem supposedly did to them."

Not that Rheem got off scot-free. The case was settled out of court, and while the parties agreed to keep the terms secret, it can now be revealed that Rheem paid Fairchild $70,000 and agreed to refrain from using one of Fairchild's proprietary process steps.

Although the settlement was cheap, the suit crippled Rheem in other ways, and after 2.5 years in business, the company caved in and was sold to Raytheon. So in 1961, Raytheon acquired the first spinout from Fairchild, which was the first spinout from Shockley, which they could have had in the first place, 7 years before.

There was bad blood between the companies for years, but time has healed the wounds, and now Fairchild and Raytheon are peaceful next-door neighbors on Ellis Street in Mountain View.

Dr. Baldwin faded from the scene soon thereafter and, when last heard from, he was starting a hybrid IC firm in the Conejo Valley in southern California.

Meanwhile at Fairchild, while things were going swimmingly in the marketplace, relations between Mountain View and Syosset headquarters were becoming increasingly strained. New York tried to impose eastern working conditions on the California plant; the company's stock-option plan for key employes was penurious; and too much of the cream being generated in California was skimmed off to finance ill-advised acquisitions by New York.

The operators of the profit center in Mountain View became convinced they were being bilked. And thereby were sowed the seeds of discontent, which created troubled Fairchild has not yet outlived.

HALF PULL OUT IN 1961

These conditions led directly to half of the Fairchild founders pulling out in 1961. Dr. Hoerni, Dr. Last, Dr. Roberts and Mr. Kleiner left to form Amelco, a cornerstone in the later huge Teledyne complex. Although Amelso (now Teledyne Semiconductor) has never been a major challenger to Fairchild in the marketplace, the move proved to make sound economic sense for the defectors. This despite the fact that only one of the four, Dr. Last, remained with the firm. Today, up in the corporate headquarters of Teledyne, he is far removed from Amleco.

Dr. Hoerni left Amelco in 1964, along with Robert Freund, another ex-Fairchilder, to set up a semiconductor department for Union Carbide Electronics (UCE). In 1967 he left UCE to found Intersil. Mr. Freund retired shortly thereafter, and UCE Semiconductor was taken over by still another ex-Fairchilder, David Beadling.

Mr. Beadling moved UCE south to San Diego, out of the Bay orbit, and in 1969 the operation was sold to Solitron. About the same time, Mr. Beadling left to help found another company in the San Diego area, Garrett Micro-Circuits, a second source to American Micro-Systems. The parent company is Garrett Corp., and AMI is also heavily involved. Still another San Diego entry upcoming is Burroughs Corp., also setting up with AMI know-how.

That makes three semiconductor firms in the San Diego area, all Fairchild-descended. Even though Fairchild itself has abandoned its plans for a major facility there, San Diego may yet become the next semiconductor center of the world. But that is another story.

[From the Electronic News, Jan. 18, 1971]

SILICON VALLEY-U.S.A., PART II

(This is the second of a three-part series on the origin of the semiconductor business in the Bay Area. The first instalment traced the startup of Shockley Transistor, ending with the creation of Signetics.)

(By Don C. Hoefler)

Soon after the formation of Signetics in late 1961, James Nall left Fairchild to form Molectro Corp. This was one of the very few Fairchild spinouts which failed, but it did serve later as the partial nucleus of the revitalization of National, to be discussed presently.

The biggest loser ever to spin out from Fairchild was begun in 1963, with a retired Marine Colonel named Arthur Lowell, and a Fairchild group including James P. (Phil) Ferguson, Robert Norman and Howard Bobb. The company was General Micro-Electronics, the first specifically formed to exploit the MOS (metal-oxide-silicon) technology.

Colonel Lowell was a high-flying promoter, and when word was circulated that GM-E was backed by General Motors, he just smiled and said nothing. Finally GM took notice of the rumor, and issued a press release denying it.

He was also involved, in August, 1963, in a legendary interview with Alfred D. Cook, then editor of Electronic News. "The colonel said we were going to have 24 plants in 12 months," recalls Howard Bobb. "I wanted to jump in and say, 'No, it's 12 plants in 24 months.' Afterwards I asked him if he was counting outhouses."

GM-E was the seventh spinout from Fairchild (including two equipment companies not shown in the map above). The brass in Syosset were getting uptight about the defections, and ordered Mountain View to sue.

Like the Rheem case, the GM-E suit was settled out of court; and, as in the Rheem case, both parties agreed to keep the terms secret.

It can now be revealed, however, that no money changed hands. "But to get them off our backs, we had to come up with somthing face-saving," a former GM-E insider recalls. "So we found a process in which we had no interest, and then promised not to use it."

The founding and failure of GM-E has an interesting parallel movement starting back in New Jersey in the early 1950s. There, young Dr. William Hugle was being forced out of the synthetic gem business by Union Carbide. He was looking for a new venture when he came upon a list of the first 10 licensees of the Bell Labs semiconductor patents through Western Electric.

The list included all of the giants of electronics, such as RCA, GE, Westinghouse, Philco, Sylvania and Rayethon. But down at the bottom were two little-known names: Texas Instruments, and Baldwin Piano. Dr. Hugle wrote the two dark horses, offering his services, and those of his wife, Dr. Frances Hugle.

TI never answered the letter, but Baldwin did. Its interest in transistors was for electronic organs, and possibly military and industrial electronics. The two Drs. Hugle moved to Cincinnati to join Baldwin, forging a friendly relationship which continues to this day.

The Hugles later left Baldwin Piana for Westinghouse, first in Pittsburgh, and later in Newbury Park, Calif. Later they migrated to the Bay Area. In 1962, Mr. William Hugle formed Hugle & Lee, with Dr. Richard E. Lee of Texas Instruments. The partnership operated initially as a consultant firm, but ultimately evolved into Siliconix, with backing from Baldwin, plus Electronic Engineering Co. of California.

Soon there were policy differences, however, and the Hugles pulled out of Siliconix the following year. Thus it was that in early 1963, both Dr. Hugle and Colonel Lowell were looking for backing for new semiconductor companies.

Dr. Hugle was closing in on Pyle-National in Chicago, and Colonel Lowell was closing in on Stewart-Warner, also in Chicago. The two companies are in fact next-door neighbors, and Dr. Hugle and Colonel Lowell frequently bumped into each other in those days.

Both of the potential sponsors were adamant that the new semiconductor firms were to be set up in Chicago. Dr. Hugle was equally adamant in insisting to Pyle-National that manpower problems would make that impossible. Colonel Lowell was presenting the same argument to Stewart-Warner. As a result, both negotiations broke down.

This had a settling effect on both companies. They reopened negotiations, willing to start up in California. But they apparently used a DPDT switch to turn things on again, and the dancers had changed partners. Thus it happened that Pyle-National wound up blacking Colonel Lowell in GM-E, and Stewart-Warner set up Stewart-Warner Microcruits under Hugle.

At GM-E, Colonel Lowell had ideas so grandiose that they nearly wiped out little Pyle-National. But Philco-Ford came to their rescue in 1966, buying the company and putting the name to rest forever. Thus the company that was once rumored to be a GM model found itself in the Ford garage.

But Philco soon found that trying to tame GM-E was like trying to grasp an eel in a barrel of water. Like most eastern companies, it thought stock options were kind of funny, while the GM-E troops who found themselves working strictly for salary again, didn't find it funny at all.

Philco's operating losses for the division were exceeding $1 million a month, and key people were planning mass defection. Philco finally gave up in 1968, closed the operation and headed back home for Pennsylvania.

The first significant spinout from Philco—GM-E was American Micro-Systems, headed by Howard Bobb, in 1966. This group accomplished what GM-E was never able to do—make a profit in MOS. Many others have since tried to emulate AMI's performance, but the company still controls close to 50 per cent of the market.

AMI soon was followed by Dr. James McMullen, Earl Gregory and others, who formed McMullen Associates, which ultimately became Electronic Arrays. This group thought they were set with Sangamo Electric, but Sangamo also balked at McMullen's key-employe stock option plan.

Sangamo was quite willing to provide a nice ownership package for the founders, but could not be convinced that additional options would be required for recruiting. It finally backed off, and the McMullen group had to start over again looing for money. They found it, mostly among some wealthy individuals in Beverly Hills, plus a nice infusion from the Dutch Phillips Company, and Electronic Arrays finally opened its doors in 1967.

The year 1967 also saw the reorganization of National Semiconductor Corp. as it exists today.

The company had been formed in Danbury, Conn., in 1959, when Dr. Bernard Rothlein led a group out of Sperry Semiconductor. Sperry didn't take too kindly to that, and sued. This action, however, made Fairchild's suit against Rheem in the same year look like a church social. Sperry was going for the jugular, and found it.

Sperry's Exhibit A in the court room was a large blow-up of its organization chart before Dr. Rothlein and his group left. Then the Sperry barrister slowly and dramatically placed large black squares, one by one, over each box which represented a defector. It was a hokey performance which probably would be laughed out of court in high-turnover California, but it did the job and brought National to its knees.

By 1967, National was nearly out of business in Danbury, and the same was true of Molectro in Santa Clara. Then along came a new group of investors, headed by Peter Sprague, who put it all together. They acquired the National and Molectro shells, dropped the Molectro name, and moved the National headquarters to Santa Clara. They also made a few management changes.

Charles E. Sporck, general manager at Fairchild, became president of National, bringing with him a group which included Fred Bialek, Pierre Lamond, Roger Smullen and, later, Don Valentine. Coming from the back of the pack, this group in 3 years brought National to a neck-and-neck fight with Signetics for fourth place.

It may be wondered why, since this was potentially the most damaging spinout yet. Fairchild didn't sue National, as it had Rheem and GM-E. One obvious reason is Mr. Sporck's strong friendship with the Fairchild top line, plus the many real contributions he had made to that company.

But another reason may be that a number of Fairchild's insiders made substantial capital gains in National stock, buying it at very depressed prices before the Sporck move was made public.

Spinouts from GM-E continued on into the following year, with the formation by Nortec Electronics by Robert Norman. This was originally a small custom house, funded with his own money and that of other individuals. The firm has been bootstrapped wisely, and Mr. Norman will be in an excellent position to take it public, which he plans to do.

Another GM–E spinout that year was Integrated Systems Technology, a service company headed by Don Farina and recently acquired by Varadyne.

Also formed in 1968 was Qualidyne, originally headed by David Hilbiber, from Fairchild by way of Hewlett-Packard Associates. He subsequently resigned, and the president now is Ward Gebhardt, also from Fairchild, by way of Intersil.

[From the Electronic News, Jan. 25, 1971]

SILICON VALLEY-U.S.A., PART III

(This is the last of three articles on the history of the Bay Area semiconductor industry. The previous article related the startup of General Microelectronics and the rejuvenation of National Semiconductor with Fairchild plasma.)

(By Don C. Hoefler)

In July, 1968, it came—the shot heard "round the world." Dr. Noyce was leaving Fairchild, and so were Drs. Gordon Moore and Andrew Grove.

The story being told was that Dr. Moore, Fairchild's R. & D. director, had determined to start a company, and later persuaded Dr. Noyce to accept the presidency. Although Dr. Moore had never been known as a promoter, the story was just implausible enough to be believable. This writer bought it after having been told by both Drs. Noyce and Moore that it was so.

Dr. Noyce recently admitted, however, that the story was merely a cover to avoid legal entanglements with Fairchild, and that he himself was indeed the prime mover behind Intel Corp.

That move set up a series of chain reactions which has not fully subsided yet. Sherman Fairchild came to realize that Mountain View had to become the headquarters of FC & I, not the satellite tail that wagged the dog. So he set out to find, not a group vice-president, but a new president.

After weeks of searching, and some near-misses, Mr. Fairchild found his man: Dr. C. Lester Hogan, head of Fairchild's arch-enemy, Motorola Semiconductor.

In January–February of that year, Dr. Hogan had been in negotiation with Genreal Instrument, for himself and a group of Motorola executives—later to be known as "Hogan's Heroes"—to join that company. It has since been speculated that in making the Fairchild deal, Dr. Hogan has at least as much concern with his moral obligation to the "heroes" as to the needs of Fairchild. He brought the group with him to Fairchild, but a year later publicly admitted it had been a mistake.

The choice was traumatic for many Fairchild veterans, and the pace of spinouts increased at once. The first came out of Fairchild R. & D. Precision Monolithics was formed by Marvin Rudin and Dr. Garth Wilson, with 40 per cent of the backing from Bourns, Inc., which has an option to acquire control in 1974.

Another 1968 spinout was Computer Microtechnology, with a founding group all ex-Fairchild except the president, Francis Megan. He hails from ITT Semiconductors' facility in West Palm Beach, Fla.

Having a more variegated lineage is Advanced Memory Systems, with president Robert Lloyd out of IBM, and other founders from Motorola, Collins and Fairchild.

In late 1968, Dr. Hogan removed Jerry Sanders from marketing responsibility at Fairchild, and offered him instead a contrived vice-presidency with a vague charter. Mr. Sanders decided to think that one over on the sands of Malibu, when Jack Gifford of Fairchild called him to suggest he join still another group of ex-Fairchilders who were planning to start a company.

That group became Advanced Micro Devices early in 1969, with eight founders from Fairchild. Thus Fairchild, which was founded by eight people from Shockley, gave up eight people for the founding of AMD.

The last Fairchild spinout of 1969 was Communication Transistor Corp. Although the president, Thomas Ciochetti, came from National, he had previously been at Fairchild and ITT, and all of the other founders are from Fairchild. CTC, an affiliate of Varian-Eimac, specializes in high-frequency devices.

THE RILEY BOMBSHELL

The Noyce-Hogan moves of July–August, 1968, also set in motion a series of events which culminated in another major upheaval, in August, 1970.

Shortly after Dr. Hogan arrived at Fairchild, a group of marketing people from there headed by Marshall Cox, and a group of engineering people headed by Joseph Rizzi, moved next door to Raytheon. Immediately, Raytheon's prospects zoomed as never before.

The relationship didn't take, however, largely because it was based on salaries rather than capital gains. So early in 1970, both the Cox and Rizzi groups, along with Roger Smullen and Kenneth Moyle of National, joined Dr. Hoerni at Interstil, forming a second company, Intersil Memory Corp. Suddenly Interstil looked like it would be the happening that Raytheon almost was.

But Dr. Hoerni didn't stop there. Since the first of the year he and Fred Adler, New York financier, had been trying to entice James Riley away from Signetics. Mr. Riley had been having the same sort of troubles with Corning that the Fairchild management had earlier encountered with Syosset. Mr. Riley's departure become widely rumored in the industry, but before matters got out of hand, he and Corning closed ranks to deny them, and all seemed quiet again.

Dr. Hoerni never gave up his recruiting, however, nor was he particularly inconspicuous about it.

All summer long, Dr. Hoerni and Mr. Cox continued to work on Mr. Riley. Mr. Cox with the hard sell, and Mr. Hoerni with the soft sell. Then for a change of pace, Mr. Adler would come in from New York with more blandishments.

Finally on Sunday evening, Aug. 30, the officers and directors of Signetics were summoned hurriedly to a secret meeting, where they were told that Mr. Riley's resignation would be announced the following day, and that Charles C. Harwood would be his successor.

Mr. Riley was to become president of Intersil, Inc., a position that Dr. Hoerni conveniently had left open since February.

Two more spinouts appeared on the scene at 1970 year-end. Richard Bader and Thomas Darby, both ex-Fairchild, formed Integrated Electronics with backing from Western Microwave Laboratories and Instrument Systems Corp.

(Ironically, Dr. Moore had insisted on the founding of Intel that a name be chosen which conveyed the idea "Integrated Electronics.")

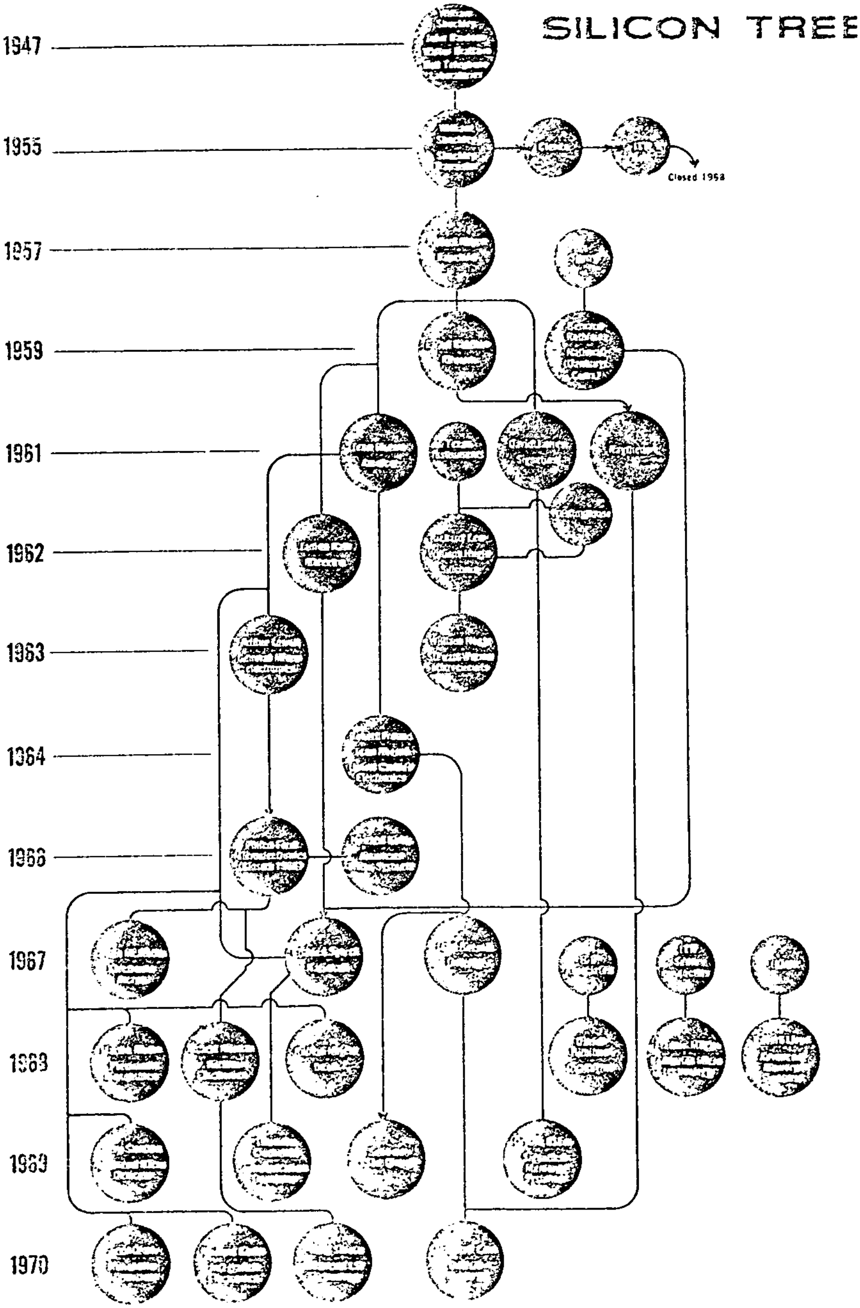
Philip Shiota, formerly of Nortec, started advanced LSI technology, with debt financing provided by the Bank of Tokyo.

The latest additions bring to 23 the number of monolithic semiconductor firms in the San Francisco Bay Area. All, except for the two Hugle companies—Siliconix and Stewart-Warner—have blood lines going back to Fairchild, and hence to Shockley.

This common ancestry makes the semiconductor community there a tightly-knit group. Wherever they go, ex-Fairchilders retain an awesome respect and emotional attachment to their Alma Mater. The wives all know each other and remain on the friendliest terms.

The men eat at the same restaurants; drink at the same bars, and go to the same parties. Despite their fierce competition during business hours, away from the office they remain the greatest friends.

And on a rare occasion, when one bends an elbow and lifts a glass, he remembers that it all began with Shockley—only 15 shorts years ago.



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CRYSTAL-DATE WATCH CALENDAR CO.,
A DIVISION OF MERRICK INDUSTRIES, INC.,
Santa Clara, Calif., August 26, 1975.

Subject: Government infringement on privately-owned patents.

Prof. WILLIAM SHOCKLEY,
Stanford University,
Stanford, Calif.

DEAR DR. SHOCKLEY: Having complained to the Department of Defense regarding their policy of promoting the infringement of patents which are privately owned, I was told by the people at the Pentagon that the "government has a long-standing policy" and that their position is "quite firm". Their policy will continue to be that the "Authorization and Consent Clause" will be included in every supplies contract, regardless of the nature of the product, the dollar amount of the contract, or who owns any patents which are infringed in the performance of the contract.

What this means is that the government is actually encouraging their low bidder contracts to infringe any and all patents which may be applicable to the performance of their contract. They need not obtain permission from a patent owner, or even advise anyone that they are infringing. Should the patentee learn of this infringement the government holds out a so-called remedy to him in the form of administrative claim rights for compensation for use of the patient. This claim ultimately must be litigated in the Court of Claims in Washington, D.C., which is something a small business or private individual cannot begin to afford.

One can understand the government's policy to permit infringement of a patent when it involves critical materials or weapons systems effecting the national security. However, in my opinion the government is not justified in using this drastic measure in its procurement of advertising material, such as my patented watch calendar invention or other noncritical supplies of a low-priority nature.

I've heard a lot of people say "patents are worthless", but I never agreed. However, the government by its action infringing my patent has convinced me that it can make any patent nearly worthless if it wants to. It's ironic that on the one hand the government tries to foster and encourage innovation by offering its patent system, while on the other hand, through its infringement policy it is not only dampening would-be innovation, it is actually riding roughshod on prviately-owned property.

I am aware of your efforts to uphold and improve our patent system, and I would appreciate it very much if you could make my predicament known wherever possible in your efforts to correct policy.

Very truly yours,

ROBERT G. MERRICK, *President.*

On the Statistics of Individual Variations of Productivity in Research Laboratories*

WILLIAM SHOCKLEY†, FELLOW, IRE

In the following pages a co-winner of the 1956 Nobel Prize in Physics presents a novel study of one of today's most precious commodities—scientific productivity. The author not only measures the variations that exist between different research workers, he also explains these differences and draws some specific conclusions about the relationship of salary to productivity. PROCEEDINGS readers will find this an especially timely and significant discussion, particularly in view of the present widespread concern about manpower shortages and proper utilization of scientific personnel.—*The Editor*

Summary—It is well-known that some workers in scientific research laboratories are enormously more creative than others. If the number of scientific publications is used as a measure of productivity, it is found that some individuals create new science at a rate at least fifty times greater than others. Thus differences in rates of scientific production are much bigger than differences in the rates of performing simpler acts, such as the rate of running the mile, or the number of words a man can speak per minute.

On the basis of statistical studies of rates of publication, it is found that it is more appropriate to consider not simply the rate of publication but its logarithm. The logarithm appears to have a normal distribution over the population of typical research laboratories. The existence of a "log-normal distribution" suggests that the logarithm of the rate of production is a manifestation of some fairly fundamental mental attribute. The great variation in rate of production from one individual to another can be explained on the basis of simplified models of the mental processes concerned. The common feature in the models is that a large number of factors are involved so that small changes in each, all in the same direction, may result in a very large change in output. For example, the number of ideas a scientist can bring into awareness at one time may control his ability to make an invention and his rate of invention may increase very rapidly with this number.

A study of the relationship of salary to productivity shows that rewards do not keep pace with increasing production. To win a 10 per cent raise a research worker must increase his output between 30 and 50 per cent. This fact may account for the difficulty of obtaining efficient operation in many government laboratories in which top pay is low compared to industry with the result that very few highly creative individuals are retained.

I. INTRODUCTION

EVERYONE who has been associated with scientific research knows that between one research worker and another there are very large differences in the rate of production of new scientific material. Scientific productivity is difficult to study quantitatively, however, and relatively little has been established about its statistics. In this article, the measure of scientific production I have used is the number of publications that an individual has made.

The use of the number of publications as a measure of production requires some justification. Most scientists know individuals who publish large numbers of trivial findings as rapidly as possible. Conversely, a few outstanding contributors publish very little. The existence of such wide variations tends to raise a doubt about the appropriateness of quantity of publication as a measure of true scientific productivity. Actually, studies quoted below demonstrate a surprisingly close correlation between quantity of scientific production and the achievement of eminence as a contributor to the scientific field.

The relationship between quantity of production and scientific recognition has been studied recently by Dennis,[1] who considered a number of scientists who have been recognized as outstanding. As a criterion of eminence for American scientists, he has used election to the National Academy of Sciences; his study is based on 71 members of the National Academy of Sciences who lived to an age of 70 or greater and whose biographies are contained in the Biographical Memoirs of the Academy. He finds that all of these people have been substantial contributors to literature with the range of publications extending from 768 to 27, the median value being 145. (Based on a productive life of approximately 30 years, this corresponds to an average rate of publication of about 5 per year, a number to which I shall refer in later parts of this discussion.) Dennis concludes that relatively high numbers of publications are characteristic of members of the National Academy of Sciences. He conjectures that of those who have achieved the lesser eminence of being listed in American Men of Science, only about 10 per cent will have a

* Original manuscript received by the IRE, December 3, 1956. Presented first as the invited lecture, Operations Res. Soc. of Amer., Washington, D. C., November 19, 1954; also at the Washington Phil. Soc., late spring, 1955; and at the 1955 fall meeting of the Natl. Acad. of Science. It has been reported briefly in *Newsweek*, December 6, 1954; *Chem. Week*, November 26, 1955; abstracted in *Science*, December 10, 1955; and in *Science Digest;* February, 1955.

† Shockley Semiconductor Lab. of Beckman Instr., Inc., Mountain View, Calif. This material was prepared while the author was Deputy Director and Res. Director of the Weapons Systems Evaluation Group, Dept. of Defense, on leave from Bell Telephone Labs., Inc.

[1] Wayne Dennis, "Bibliography of eminent scientists," *Sci. Monthly*, vol. 79, pp. 180–183; September, 1954.

publication record exceeding the 27; which represents the minimum publisher of the 71 listed in Biographical Memoirs of the National Academy of Sciences. He has also studied eminent European scientists and comes to essentially the same conclusion. In fact his study goes further and shows that almost without exception heavy scientific publishers have also achieved eminence by being listed in the Encyclopedia Britannica or in histories of important developments of the sciences to which they contributed.

It should be remarked that in Dennis' work, he includes more routine types of contributions (such as popular articles) than are generally associated with scientific eminence. However, it may still be appropriate to quote a few of the statistics obtained by Dennis for people who certainly classify in the genius class of the scientific publishers. Among these Dennis refers to: Pasteur with 172 publications, Faraday with 161, Poisson with 158, Agassiz with 153, Gay-Lussac with 134, Gauss with 123, Kelvin with 114, Maxwell with 90, Joule with 89, Davy with 86, Helmholtz with 86, Lyell with 76, Hamilton with 71, Darwin with 61, and Riemann with 19. Riemann, who was the least productive, died at the age of 40. At his rate of publication, he would probably have contributed at least another 10 or 20 publications had he lived to the age of 70. Even with 19, he was in the top 25 per cent of the 19th century scientists referred to in Dennis' study.

The chief conclusion reached in this article is that in any large and reasonably homogeneous laboratory, such as, for example, the Los Alamos Scientific Laboratory and the research staff of the Brookhaven National Laboratory, which are included in this study, there are great variations in the output of publication between one individual and another. The most straightforward way to study these variations is to list the number of individuals with zero, one, two, etc., numbers of publications in the period studied. This compilation may then be plotted as a distribution graph [see Fig. 2(b) for an example]. In some cases, however, the data are too meager for a smooth trend to be seen easily and another form of presenting the data is more convenient.

The form used for most of the data presented in this paper is the *cumulative distribution graph.*

Such a graph can be illustrated in terms of the distribution of the height of a regiment of men. If the men are lined up in order of increasing height at a uniform spacing, then, as shown in Fig. 1(a), there will be a steady increase in height from the shortest man to the tallest man. There will usually be a few men who are exceptionally short, a few men who are exceptionally tall. For the majority of the men the height will vary relatively uniformly along the line of the men. In general, one should thus expect an S-shaped curve with an inflection point near the middle of the distribution.

Such a curve is closely related to the distribution in height shown in Fig. 1(b), which represents the number of men whose height lies in any particular interval of height. This can be obtained from Fig. 1(a), as is represented there, by drawing two lines bracketing a certain interval in height and counting the number of men lying in this range. Fig. 1(b) represents a smooth curve drawn through such a distribution. It can, in fact, be obtained from Fig. 1(a) by drawing a smooth curve through the distribution in height and differentiating the number of men as a function of the height.

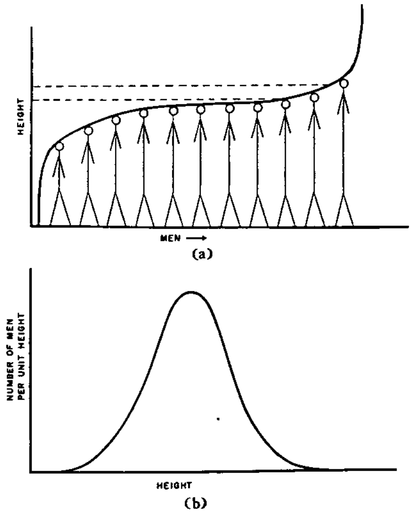

Fig. 1—The cumulative-distribution graph and the normal-distribution curve. (a) The cumulative-distribution graph represented by men arranged in order of height at uniform spacing. (b) A "smoothed" distribution curve, of normal form, such as might be obtained from (a) by finding the number of men in each small increment of height.

For many natural phenomena and in particular for those in which the measured quantity varies due to the additive effects of a large number of independently varying factors of comparable importance, a Gaussian or normal distribution, like that of Fig. 1(b), is obtained. Conversely, if distribution is normal, then the cumulative distribution graph will have the symmetrical S-shaped characteristic in Fig. 1(a), the middle flat portion corresponding to large numbers of cases in the central range, and the rapid convergence of the extremes to their asymptotes corresponding to the scarcity of cases which deviate much from the mean value.

One of the new results of this study, presented below in Section IV, is that the data on rates of publication can be well represented by a normal distribution when treated in a certain fashion. Some possible explanations for this observation are discussed in Section VI.

II. A Study of Publication Records

As a first example, I shall discuss the statistics of the publications of a group of people in the Los Alamos Scientific Laboratory. This sample of approximately 160 people was selected on the basis that the individuals were professionally mature and located in laboratories whose activities are of such a nature that there is some probability that workers in them might contribute to a physical or electrical engineering publication. Such publications are abstracted in *Science Abstracts A* and *B*, respectively. The publication record for each individual was ascertained by looking through the author index of *Science Abstracts* for the years 1950 to 1953, inclusive.

From these data, a cumulative-distribution graph constructed like that shown in Fig. 1(a) is obtained by listing the men in order of their publications. It is found that approximately half of the individuals have no publications at all. Then there are about 30 individuals with one publication, 20 individuals with two publications and so on. The cumulative-distribution curve shown in Fig. 2(a) has little resemblance to the simple S-shaped curve shown in Fig. 1(a). For one thing it is concave upwards throughout. For another it shows too many individuals with publication rates higher than seven in four years compared to the shape of the curve up to that rate. The distribution curve, shown in Fig. 2(b), is not normal, but instead is essentially hyperbolic in form.

Replotting this same data in Fig. 3 on a logarithmic scale for the number of publications results in a line which does look much more like a portion of the cumulative-distribution graph for a normal distribution. The line is not a smooth curve, of course, but rises in steps. However, a smooth curve drawn through the steps has an approximately linear portion, corresponding to the linear portion of Fig. 1(a), followed by an abrupt turn up at the high end corresponding to the relatively small number of people who on the logarithmic scale have exceptionally large rates of publication.

It is one of the chief conclusions of this study that the more or less normal distribution of the logarithm of rate of publication is characteristic of the statistics of the scientific creative process. Perhaps the most important feature of this conclusion is that the rate of publication increases approximately exponentially from individual to individual, taken in order of increasing rate, and that the differences in rate between low and high producers are very large. The conclusion that the exponential character of the distribution is fundamental to the creative process gains support from the fact that certain other hypotheses intended to explain it as some sort of artifact can be examined and rejected.

In subsequent sections we shall refer to the normal distribution of the logarithm as *log-normal distribution*.

III. Some Basic Data on Rates of Publication

One of the first hypotheses called the "organization hypotheses" put forward to explain how the log-normal

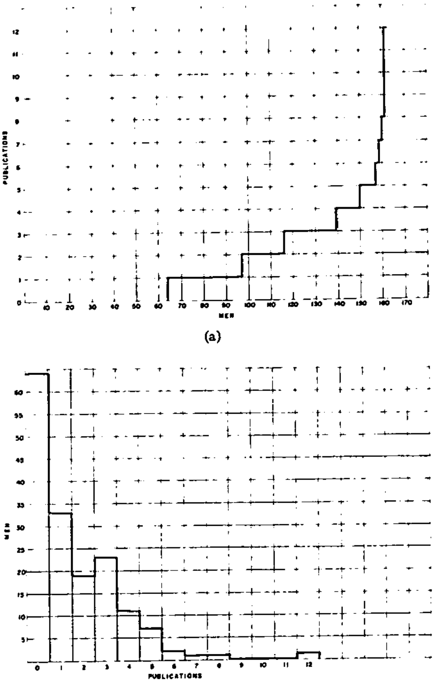

Fig. 2—Distribution of rate of publication (number of entries in *Science Abstracts A and B* in four years) at Los Alamos. (a) Cumulative distribution. (b) Distribution (number of men with each rate of publication).

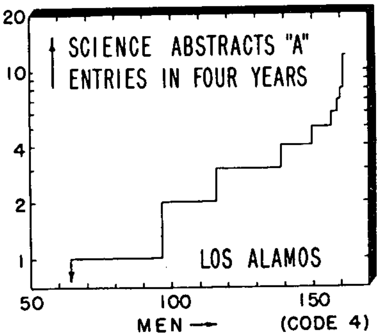

Fig. 3—Cumulative distribution on logarithmic scale for number of publications at Los Alamos.

distribution arises was that it is a consequence of the organization of research activities in large, modern laboratories. In such laboratories, physical scientists frequently make use of very complicated apparatus and large nuclear machines. As a result of this collaborative effort they frequently publish papers jointly, the number of authors varying from two to five or six in ordinary cases. The "organization hypothesis" endeavors to use joint authorship to explain the exponential character as follows: As a consequence of the size of the teams who work together, an individual who has some supervisory or organizational responsibility may contribute to the activities of many men and be listed as a co-author on many papers. As a result, a relatively few people will appear as co-authors of a very large number of papers and this group can be better included in a log-normal distribution than in a normal distribution.

This "organizational hypothesis" can be disposed of by several arguments, some of which are quite instructive. One of these arguments is based on the observation that the exponential aspects of the cumulative-distribution graph is independent of the particular organizational features of the laboratory considered and is a general characteristic of all laboratories studied in this article. For example, the organizational situation in some of the laboratories of the National Bureau of Standards would not lead to large numbers of publications by supervisors. For one Division of the National Bureau of Standards, records were available of the total number of publications and patents made by the individuals in this Division during a period of several years. These data are shown in Fig. 4. It is seen from this figure that the data lie on a relatively smooth exponentially increasing trend followed by a rapid turn-up corresponding again to a few individuals with exceptionally high publication records. Since the organization of activities is quite different in the Bureau of Standards from what it is at Los Alamos while the distribution curve is the same, the "organizational hypothesis" can be discarded.

The "organizational hypothesis" can also be rejected by studying the effect of joint authorship on the distribution of rate of publication. We shall illustrate this argument using data from the Brookhaven National Laboratory. There are approximately 180 members of the research staff of the Brookhaven National Laboratory. The "total" number of entries plotted as a cumulative distribution for these people is shown as the line marked "total" in Fig. 5. Since Brookhaven operates in a fashion rather similar to Los Alamos, it might be expected that the "organizational hypothesis" would apply equally well here. In order to test this, two other lines have been constructed on Fig. 5.

The bottom line, marked "solo," has been obtained by discarding all publications having more than one author. It is seen that a relatively small fraction of the people have made "solo" publications. However, it should be noted that the most prolific publishers of these have published at nearly half the maximum rate for the "total" line. On the other hand, a large number of people who appear as co-authors in the "total" distribution have no "solo" publications whatever. This fact shows that the rapidly rising part of the line is due

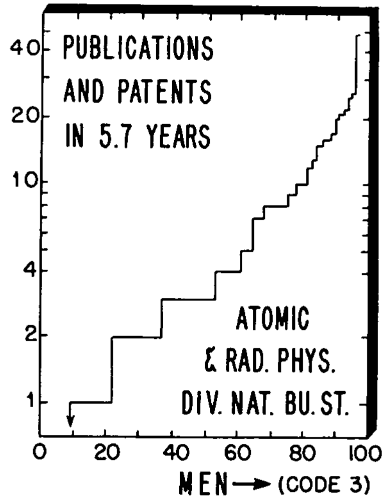

Fig. 4—Cumulative distribution on logarithmic scale for publications and patents for Atomic and Radiation Physics Div., National Bureau of Standards, for a period of 5.7 years.

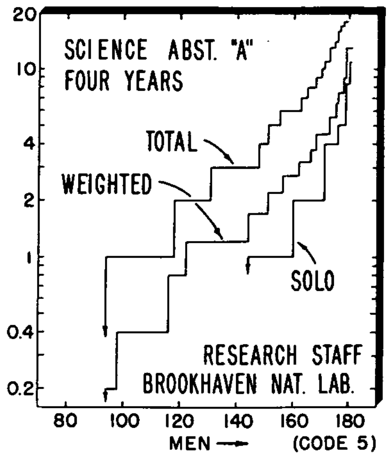

Fig. 5—Cumulative distributions on logarithmic scale for 3 cases at Brookhaven National Lab.

largely to people who are capable of producing "solo" publications, a conclusion contrary to the expectation based on the "organizational hypothesis." In fact, the evidence is that publication of about half of the people is supported by the more productive ones who would be capable of publishing at relatively high rates strictly on their own.

The middle line marked "weighted" is obtained by dividing the credit for multiple-author publications equally among the various authors. For example, each man on a four-author publication receives a contribution of 0.25 publication. The "weighted" line again shows the steadily increasing trend and does not permit an undue credit to be given to people who, through organizational position, may appear as a joint author on a large number of publications. This furnishes further support for the thesis that the exponential trend of the cumulative distribution is a fundamental characteristic of the distribution of productivity among the members of the laboratory rather than some organizational artifact.

Another possible explanation which can also be discarded is that the distribution of degree of publication from one person to another is a consequence of the distribution in age of the population considered. In principle, some such distribution might be obtained as a result of distribution in age since people on the average have a maximum in their publication rate at an age of about 35. The distribution of publication in age has been studied by Lehman.[9] Some of Lehman's results for rate of publication as a function of age are shown in Fig. 6. Very similar results are obtained for other geographical samples. Actually, what Lehman has studied is not simply publication record but "creative production." He judges creative production by references found in histories of science and other similar sources. Since the distribution of workers in the laboratories considered in this study shows a fairly uniform distribution from age 25 to age 50, it is difficult to see how the variation in productivity with age as shown in Fig. 6 could result in a very small fraction of people with exceptionally high publication rates: from Fig. 6, we would estimate that the maximum publication rate would be perhaps twice the publication rate of the median man. In contrast to this, the studies shown for Figs. 2, 3, and 4 correspond to maximum publication rates substantially more than ten times that of the median man.

IV. The Log-Normal Distribution of the Rate of Publication

The conclusion is thus reached that the exponential variation of productivity in the cumulative distribution graph is a characteristic feature of the statistics of productivity in a research laboratory. This conclusion receives further support from an additional analysis of the

[9] H. G. Lehman, "Men's creative production rate at different ages and in different countries," *Sci. Monthly*, vol. 78, pp. 321–326; May, 1954.

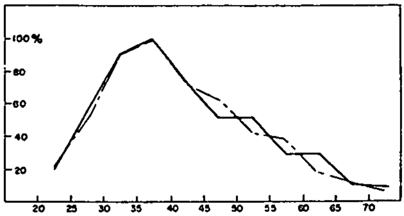

Fig. 6—Creative production rate in science and mathematics vs age for (solid line) nationals of 14 different countries other than Russia, England, France, Italy, Germany, and the U.S.A. and for (broken line) U.S.A.

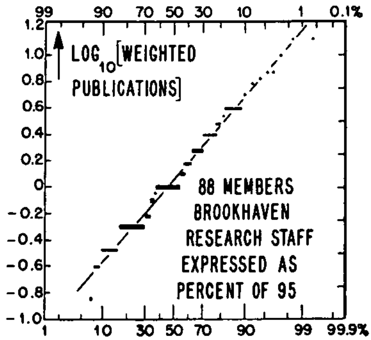

Fig. 7—Cumulative distribution of logarithm of "weighted" rate of publication at Brookhaven National Lab. plotted on probability paper.

data which show that the logarithm of the rate of publication can be well represented as a normal distribution in the cases studied.

The validity of the assumption of a normal distribution can be tested by making use of so-called "probability paper." On such paper, the cumulative number of men is expressed on a percentage scale. This percentage scale is so distorted as to increase the spread on the scale at percentages near the extreme distribution. This results in stretching out the ends of the cumulative-distribution graph of Fig. 1(a) so that it becomes a straight line, provided the distribution itself is normal.

Such a test has been applied to the weighted rate of publication for the Brookhaven Laboratory shown in Fig. 5. The result is shown in Fig. 7. It is seen that a straight line can be drawn in a very satisfactory way through the data with the exception of the two extreme men. It should be noted that in many cases so many men were assigned the same publication number that they have been represented as solid blocks on the diagram rather than as individual points. This grouping together is a genuine effect in the case of people who published one "solo" publication during the period studied and thus have a logarithm of zero and those who have appeared on two publications or as a co-author of a single two-author publication and appear at logarithms of 0.3 and −0.3. Some of the other groupings have resulted artificially from the means of handling the statistics: for simplicity in listing the people, the scale of possible publications was divided into intervals and those whose publication rates fell in these intervals were grouped together. If this had not been done, the data would fall more closely along a straight line, *i.e.*, the "fit" to the normal distribution would be better.

Fig. 7 illustrates strikingly the range of variation in rate of publication—a factor of 40-fold between lowest 10 per cent and highest 5 per cent.

The fit shown on Fig. 7 is based on the assumption that the research staff of Brookhaven may be divided into two parts, one part containing 95 members who have some likelihood of publishing physics papers referenced in *Science Abstracts A* and 85 others with negligible likelihood of making such publications. The number 95 was found by trial and error to give the best straight line in Fig. 7. This arbitrary procedure does have justification in terms of the distribution of activities in the Brookhaven Research Staff. In fact if the list of members of the Research Staff at Brookhaven is examined name by name, it is found that many are biologists, medical physicists, and the like whose fields are not covered by *Science Abstracts*. The final conclusion is that all but 101 names are considered extremely unlikely to make publications abstracted in *Science Abstracts A*. Since the difference between 101 and 95 is negligible in respect to other uncertainties in the study, we may conclude that for the publishing part of the population the rate of publication is well represented by a normal distribution on the logarithmic scale, or for brevity, a ***log-normal*** distribution.

Generally similar fits are obtained for the Los Alamos data and for the National Bureau of Standards data. Furthermore, the data on "total" and "solo" entries in *Science Abstracts A* can be fairly well fitted by log-normal distributions. The fit is very "jumpy," however, since the only possible values for publication rates are integers. On the basis of the rather limited investigation that I have carried out to date in regard to the distributions for "solo" and "total" rates of publication, it appears that these also have log-normal distributions except that the rates of publication differ from the "weighted" rates by factors of 0.6 and 1.6, respectively.

It would be interesting to compare the statistics of science departments in universities with those of the large laboratories studied above. This has not yet been done except for the limited data on the Physics Department of Columbia University shown in Fig. 8. In spite of the smallness of the sample, the general trend of the data is such as to give confidence that the log-normal distribution will also hold in such cases.

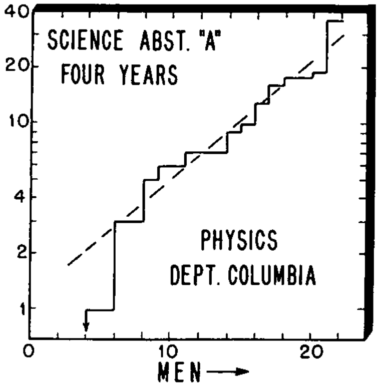

Fig. 8—Cumulative distribution on logarithmic scale for publications of the Physics Dept., Columbia Univ., for 4 years.

V. A Study of Patent Activity

Another measure of creative technical production, which is relatively readily available for study, is patent activity. Shown in Figs. 9 and 10 (opposite) are cumulative-distribution curves for patents for two large laboratories in the fields of electrical apparatus and communications. All of the data correspond essentially to "solo" publications since the number of joint patents is very small compared to individual patents.

It is instructive to compare patents with publications. Such a comparison is presented in Fig. 11 for a selected group of 60 men from one of the laboratories considered in Figs. 9 and 10. The most significant factor to note is that on the logarithmic scale, the patent distribution is markedly steeper.

VI. Speculations on the Origin of the Log-Normal Distribution

The very large spreads in productivity, for example the variation by nearly one hundred fold between extreme individuals in Fig. 7, are provocative of speculation. Most rates of human activity vary over much narrower limits, for example, pulse rates outside the two to one range from 50 to 100 per minute are extremely rare. Very few individuals walk at speeds outside the range of 2 to 5 miles per hour. In competitive activities involving trained and selected people, such as running the mile, the variation is much smaller, the ratio of speed for the mile between world's record and good high school performance being probably less than 1.5.

In the study presented here the individuals are presumably specially selected by natural ability and specially trained to accomplish scientific production. Yet the spread in rates is enormously greater than it is for the more physical activities discussed above. I believe

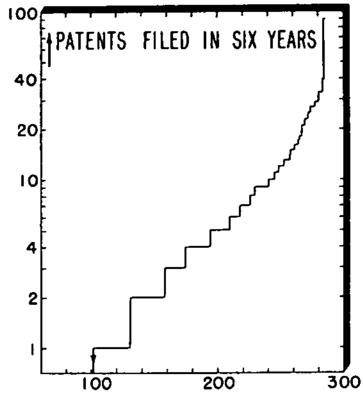

Fig. 9—Cumulative distribution on logarithmic scale for patents at a large industrial laboratory.

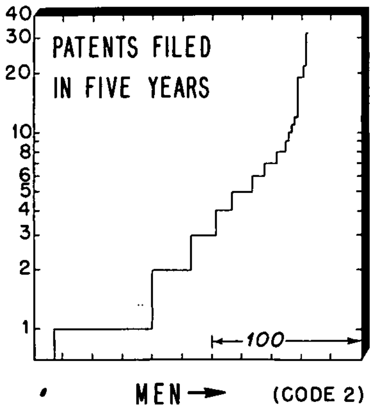

Fig. 10—Cumulative distribution on logarithmic scale for patents at another large industrial laboratory.

that it is possible to explain to some degree how such large variations in rate may occur in terms of certain characteristics of the creative scientific process. The basis of the explanation is that the large changes in rate of production may be explained in terms of much smaller changes in certain attributes. I shall illustrate this in terms of a simplified example of the inventing process.

In order to make an invention for which the United States Patent Office will issue a patent, it is, in general, necessary to conceive a new combination of features and

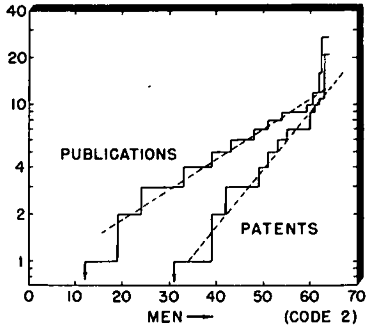

Fig. 11—Comparison of patent and publication activity for a group of research workers at a large industrial laboratory.

to appreciate how this combination may be useful. Let us suppose that the inventor perceives that he has made an invention when he appreciates the relationship between some number of ideas. For example, the automobile self-starter might have been conceived by recognizing the relationship of the following 4 ideas: the idea that a means of starting the engine without using human, muscular strength would be useful, the idea that the necessary energy could be held in reserve in a storage battery, the idea that a relatively small high speed electric motor could be used to turn the larger gasoline engine at starting speed, and the idea that the electric motor could be subsequently disengaged in order to avoid rotating it at excessive speeds.

Now let us suppose that there is some attribute of the human brain which allows an individual to be aware of "m" ideas and their relationships.[3] Then it follows that a man with $m=3$ will never invent the self-starter in the form discussed above whereas a man with $m=4$ can do so. A man with a higher value of m is much more likely to make the invention than a man with $m=4$. In fact, it may be established, by use of the formulas for permutations and combinations, that men with $m=5$, 6, and 7 can hold the 4 essential ideas in awareness (together with 1, 2, or 3 irrelevant ideas) in 5, 15, and 30 times as many ways as the man with $m=4$. This shows that a variation of 50 per cent in "brain capacity" ($m=4$ to $m=6$) can produce an increase in invention rate of 15-fold for inventions requiring the interaction of 4 ideas.

It may be instructive to illustrate the considerations presented above by an example which can be shown in detail. Suppose out of realm of idea associated with some field of endeavor an invention can be made by

[3] N. Rashevsky, "Mathematical Biophysics," University of Chicago Press, Chicago, Ill., ch. 29; 1938, presents very similar reasoning. His results are expressed in the form of equations rather than by numerical examples and lead to somewhat more general conclusions than those presented here.

holding ideas "1" and "2" in mind and seeing the relationship between them. Then a man with $m=2$ can make the invention in two ways as represented below:

$$(1, 2) \text{ and } (2, 1).$$

But a man with $m=3$ can think of these two ideas and some irrelevant idea x in six ways:

$$(1, 2, x), (2, 1, x), (1, x, 2), (2, x, 1), (x, 1, 2), (x, 2, 1).$$

Thus for every case in which the $m=2$ man can think of the idea, there are 3 ways in which the $m=3$ man can do it. Thus the $m=3$ man has 3 times as many chances to make the invention.

Evidently this advantage increases rapidly with the increasing complexity of the problem. For a 10-idea invention an 11-idea man has an 11-fold advantage over a 10-idea man; that is a 10 per cent increase in "mental capacity" produces a 1100 per cent increase in output. It is my impression that this sensitivity to the interaction of many factors in mental creativity is the key to the large variations in output found in this study. According to this explanation, the log-normal distribution in productivity then results from a normal distribution, over a relatively small range (say $m=8$ to $m=12$ in the model considered), of some attribute which controls productivity in a very sensitive way.

Still another way of rationalizing the log-normal distribution may be based upon the hypothesis that the interacting mental factors are of several different kinds rather than several of one kind, as in the case of several ideas as discussed above. For example, consider the factors that may be involved in publishing a scientific paper. A partial listing, not in order of importance, might be: 1) ability to think of a good problem, 2) ability to work on it, 3) ability to recognize a worthwhile result, 4) ability to make a decision as to when to stop and write up the results, 5) ability to write adequately, 6) ability to profit constructively from criticism, 7) determination to submit the paper to a journal, 8) persistence in making changes (if necessary as a result of journal action). To some approximation, the probability that a worker will produce a paper in a given period of time will be the product of a set of factors F_1, F_2, etc. related to the personal attributes discussed above. The productivity of the individual would then be given by a formula such as

$$P = F_1F_2F_3F_4F_5F_6F_7F_8. \qquad (1)$$

Now if one man exceeds another by 50 per cent in each one of the eight factors, his productivity will be larger by a factor of 25. On the basis of this reasoning we see that relatively small variation of specific attributes can again produce the large variation in productivity.

The factor explanation discussed above also has an appeal from the point of view of the log-normal distribution. According to the formula, the logarithm of the product is the sum of the logarithms of the several factors. If we suppose that these factors vary independently, then to a good approximation their sum will have a normal distribution, and so, consequently, will the logarithm of the productivity. It seems to me that this is at present the most attractive explanation for the log-normal distribution.

In closing this section mention should be made of an attempt to fit the data by assigning to each individual a single parameter describing his creative potential. This parameter was referred to as "mental temperature" when the original lecture was given. It was introduced in analogy with the quantity β or $1/kT$ which occurs in the equation for rates of chemical reaction or thermionic emission. According to this hypothesis an individual i is characterized by a value β_i. In a situation s his rate of production is determined by a rate constant P_s and a barrier U_s, so that his rate of production is

$$P(i, s) = P_s \exp(-U_s\beta_i). \qquad (2)$$

The rate constant P_s probably depends on β_i but in a relatively insensitive way, so that to a first approximation this dependence can be neglected.

On the basis of this equation, the difference between the two curves of Fig. 11 is to be attributed to a U value 1.7 higher for patents than for publications.

There appears to be a tantalizing possibility of establishing scales for U and β by comparing publications and patents and one laboratory with another. One might, for example, assume that the distribution of β values is the same in two laboratories having the same pay scales and similar working conditions. Then if U is chosen as unity for one activity in one of these, the scale of U can be chosen for the other cases in terms of the ratio of slopes like those of Fig. 11. Approximate values of P_s can be chosen by assuming that $\beta=0$ represents a situation in which the worker never lacks an idea to publish or an invention to patent so that his rate of production is limited by the mechanics of the situation. Such cases might correspond approximately to the most outstanding publishers in Dennis' study. On this basis P_s values of the order of 10 per year for either publications or patents might be chosen. I have made some attempts to establish scales of this sort but they are not well enough developed to warrant inclusion here.

VII. The Relationship Between Salary and Productivity

From the point of view of the economics of running a research laboratory, it is important to know the relationship between salary and productivity. For example, if the better paid men are more productive than their fellows in greater proportion than the increase in pay, then they are a sound investment. On the other hand, if they are less productive per salary dollar, then it may be wiser to hire relatively fewer of these outstanding people.

The question just posed is to some degree academic—anyone who has had to do with managing research knows that progress depends largely on a relatively small number of exceptionally able individuals. He also knows that these people are usually substantially better paid than their fellows. How much better one can afford to pay outstanding people and still find them profitable is a quantitative question faced by many organizations during periods of rapid build up. The findings in this section throw some light on this question, the conclusion being that, in general, scientific productivity is so much greater for the outstanding people that in the current scientific labor market, it is unlikely that they will be overpaid.

It is clear, of course, that increasing salary of an individual will usually not increase his productivity much, if at all. In some cases it may even have the opposite effect by reducing incentive. What is studied here is the statistical relationship between salary and productivity as established by existing pay roll procedures. If any causal relationship is important in this connection, it is that high productivity of an individual causes the management to give him high rewards.

Before considering the method of investigating the statistical relationship between salary and productivity, it may be worth-while to say something about salary in general. In determining the salary of an individual in a research laboratory, the management takes into account many factors. Only one of these factors is considered in the previous parts of this study, namely, the rate of scientific production as measured by total numbers of publications or patents. This factor is probably rarely considered in a quantitative way. Instead, the usual procedure is for a group of people charged with supervising research workers to gather together and discuss the relative merit of the individuals. In such considerations, quantitative measures of the individual's contributions are seldom referred to. There probably does not exist at the present time any valid analysis of the various factors that are considered and their relative importance. Among them may be mentioned, however, the originality and importance of publications which are made. Thus quality as well as quantity is brought into account. Other factors which are certainly considered are the ability of an individual to carry out the techniques of his work, whether these be of a theoretical nature involving pencil and paper or the manipulation of apparatus; the ability to contribute to the solutions of problems of other workers in the organization; the ability to produce cooperation among other workers; the ability to attract productive candidates to the organization; the ability to influence the activities of other workers along lines which are more wisely chosen than they would choose themselves with respect to the goals of the organization as a whole; the ability to carry out activities which enhance the prestige of the organization. These and many other factors are generally considered in determining a man's "merit" and thus deciding what salary he should receive.

The assumption of this article is that merit and salary are somehow determined by the combination of such factors as those which we have discussed above. These factors are not closely correlated with each other, although it is probable that there is a tendency for outstanding ability in any one to be coupled with a probability of higher abilities in the others as well. The only attribute which has been studied here is simple quantitative productivity in the sense of publications and patents. If it is found that this attribute, which was studied purely for purposes of convenience, is strongly correlated with increasing salary, then it seems likely that the other attributes are also strongly correlated with salary.

It is not appropriate to consider simply the relationship of salary to productivity. The reason for this is that there is a general tendency of salary to increase with age, this being a recognition of increasing general judgment and experience with age as well as a socially acceptable procedure. Thus, in order to get a truly representative comparison of merit with productivity, it is necessary to correct for age. This procedure can be done in various ways; the one selected for this article being that associated with the concept of "merit quartiles."

The division of the population of a laboratory into "merit quartiles" may be illustrated with the aid of Fig. 12. This figure represents the salaries of a group of

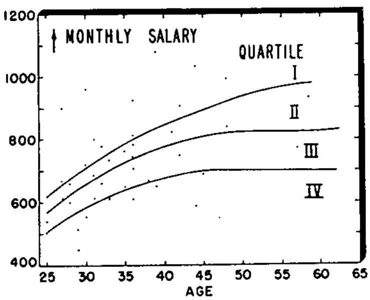

Fig. 12—Salary vs age (for a representative sample only of individuals) in a laboratory considered in this study with lines dividing the distribution into "merit quartiles"). Effective about October, 1954.)

individuals in a laboratory covered in this study. Each individual is represented by a point on the figure which shows his salary and his age. Three lines have been drawn on the figure dividing it into four groups of individuals, called quartiles. The procedure for drawing these lines is as follows: in each relatively small age interval the population of the laboratory is divided into halves such that half of the group gets more than the median salary and half less. Then the upper and lower halves are similarly divided into 2 equal parts so that each age interval is divided into 4 quartiles. This procedure is carried out for the various age intervals and then a smooth curve is drawn. These smooth curves are drawn in such a way that at each age interval, approximately ¼ of the population of the laboratory lies in quartile I and approximately ¼ in each of II, III, and IV. Thus the people in the first or top quartile have approximately the same age distribution as those in the second, etc. Furthermore, all of the people in the top quartile obtain higher salaries than those in the second quartile at the same age.

These merit quartiles furnish a basis for dividing the laboratory into parts in accordance with salary but chosen in such a way that the age distribution in each part is similar. Thus any effect of varying productivity with age affects all the quartiles about the same way.

Fig. 13 shows a similar plot for the individuals in a U. S. government laboratory operated under Civil Service. It is to be noted that the highest salaries at any age range are substantially lower than those in the other non-Civil Service laboratory. The difference would be even more striking if the higher paid executive types of an industrial laboratory could also be shown.

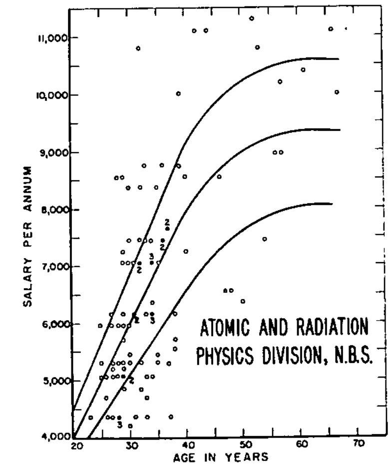

Fig. 13—Salary vs age for Atomic and Radiation Physics Div., National Bureau of Standards, together with "merit quartiles" divisions. (Effective about October, 1954.)

The use of merit quartiles, deciles, or similar divisions is playing a progressively more important role in salary administration.[4] One of the great advantages of the merit scale is that it provides an intuitively satisfactory way of ranking the individuals in an organization. The same would not be true if the men were ranked simply according to salary; thus a very able young man at a relatively low salary would be obviously out of place in company with an older group of average ability (but with more experience) at the same salary and it would be difficult to get any sense of order from such listing. On the other hand, a group of supervisors can come to agreement and reach decisions surprisingly easily about merit rank between people whose ages and salaries may differ by large amounts. I do not believe that it is evident in any *a priori* sense that such agreement would be expected; it appears rather an interesting and useful experimental result. In a sense, it is a surprising result since, as discussed above in this section, such diverse factors are considered in making the judgment. The agreement as to merit ranking by a supervisory group does not, of course, imply that the worth of the individual is truly assessed in any absolute sense. However, the large degree of consistency does imply that a useful and impartial tool for salary administration exists.

In principle, an organization can establish a family of merit curves at each raise period (allowing for cost of living adjustments, changing competition, etc.). The new salary for a man whose merit rank is correctly appraised can then be simply read off his location on the new curves. It sometimes happens, due perhaps to accidents of recruiting or due to changing skills on the part of the worker, that a revision of merit rating occurs. It is generally felt that only a fraction, say 50 per cent, of the correction should be made in any one raise since this will tend to smooth out fluctuations in the salary system.

A set of quartiles like those shown in the two previous figures have been prepared for the research staff of the Brookhaven National Laboratory. For each one of these quartiles, which contain about 46 men each, the publication records have been compiled as cumulative-distributions. These are not presented as graphs with steps since there are so many cases of overlap that the lines for different quartiles are very hard to separate. Consequently, smoothed distribution curves have been drawn through the steps in the manner illustrated in Fig. 1(a). The resulting curves are shown as Fig. 14.

From Fig. 14 it is readily seen that approximately the same numbers of people in quartiles I and II published, but that the amount of publication of the high publishing members of quartile I was larger by almost a factor of 2 than for the corresponding people of quartile II. Quartile III contains some individuals having high rates of publication and a smaller fraction of people publishing. The total amount of publication in quartile IV was substantially less than quartile III.

Similar diagrams have been made for other laboratories but there is no great uniformity in their characteristics. However, there is a very general trend which holds for all cases considered. This trend is for the average rate of publication per individual to increase steadily from quartile to quartile, being highest for the first or best paid quartile.

From the type of spread which is observed in Fig. 14,

[4] Employee interest is also high. For example, merit curves have been deduced from polls of employees of Bell Tel. Labs. by the Conf. of Prof. Tech. Personnel Inc., P.O. Box 625, Summit, N. J.

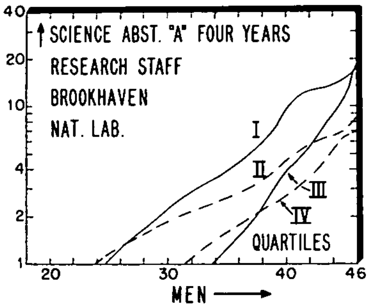

Fig. 14—Cumulative distributions (shown as smooth lines) for the four "merit quartiles" of the research staff of Brookhaven National Lab.

it is evident that publication per se is not given heavy weight in determining merit in terms of salary at Brookhaven. It is evident that something like 10 or 15 per cent of the individuals in quartiles III and IV exceed the publication records of about 50 per cent of the people in quartiles I and II. However, this is not sufficient to give them in terms of salary a recognition equal to those of quartiles I and II. Thus it follows that other factors certainly are being considered in determining salary.

From the general shape of the curves shown in Fig. 14, a very crude sort of an estimate can be made of the number of additional factors which must be taken into account in determining salary provided these factors are assumed to have importance approximately equal to amount of publication. For example, if we compare quartiles I and II we see that only 10 per cent, approximately, in quartile I exceed the maximum production of people in quartile II. This suggests that there might be something of the order of 10 other factors involved in weighting the people of quartile I, each one of these 10 other factors contributing to a group of about 10 per cent who exceed the performance of individuals in quartile II. Evidently this type of reasoning does not apply in the same way to quartiles I and III, but the fact that something between $\frac{1}{10}$ and $\frac{1}{4}$ of quartiles III and IV exceed most of the people in quartile I in terms of amount of publication suggests an analysis might lead to the conclusion that in determining subjectively the merit rating of an individual, salary reviewers act as if there were something like 4 to 10 factors of comparable importance to amount of publication.

I shall now return to the question taken up in the beginning of this section, namely, the quantitative relationship between salary and productivity. For the various laboratories considered in the study, sets of quartiles have been drawn and the average amount of production determined for each quartile. This information is gathered together in Fig. 15. The data have been expressed in terms of rate of activity in publication or patents per man-year. For the publications the total number of publications was used (not "solo" or "weighted"). It is observed that in all cases there is a monotonic increase in rate of activity with quartiles, increasing towards the highest paid quartile, quartile I. The actual spread in amount varies by a factor of about 9 for the most rapidly varying case and by a factor of a little over 3 for the most slowly varying case.

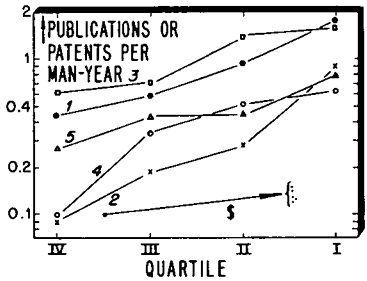

Fig. 15—Relationship between productivity and quartile number and salary and quartile number for several laboratories.

A comparison with salary is also indicated in the figure. The ratio of salary at the dividing line between quartiles III and IV to that between quartiles I and II at age 35 is also shown on the figure. Five cases have been considered and there are somewhat different spreads in salary for these. The line represents a sort of weighted average of the change in salary.

It is clear from inspection of the figure that in progression from quartile to quartile there is much less increase in salary than in productivity, in fact productivity lines rise 3 to 5 times as steeply as the salary lines. In other words, statistically an increase of 30 to 50 per cent in productivity is necessary for an individual to obtain an increase in salary of 10 per cent. However, as the reasoning given in connection with Fig. 14 shows, increase in scientific productivity alone is not sufficient to produce the increase in merit rating. In fact, coupled with the 30 to 50 per cent increase in productivity, there probably must be comparable increases in other kinds of contribution. In other words, the individual probably must become 30 to 50 per cent better in all respects in order to receive a recognition corresponding to a 10 per cent increase in salary.

VIII. Relevance to Civil Service Salary Scales

I should like next to discuss the relevance of these findings to the problem of Civil Service scientists in government laboratories. In addition to relatively low salaries positions in government laboratories are less attractive than those in industry or in universities. This is especially true in laboratories in the military establishments where periodically changing direction by officers who are not experienced in directing research frequently leads to morale problems. These problems have been thoroughly explored and reported in detail in the recent report[8] of the Riehlman committee of Congress. Clearly it is important to retain in these laboratories some highly-qualified, strong-minded, inspired leadership in order to prevent research effort from becoming thoroughly second grade.

This brings us to the most important conclusion in the study, and one which might possibly furnish a basis for action. The top salaries in government laboratories are substantially lower than both those in industry and in universities, at least for people in the latter whose line of work involves undertaking summer assignments and doing consulting. Even if there were no disadvantages aside from salary, the limits of salary set by Civil Service scales probably have a most severe effect on the leadership and originality available in government laboratories. Although these attributes have not been studied quantitatively, all of the findings in this article are consistent with the idea that leadership and originality increase very rapidly with salary just as do rate of publication and rate of invention. Cutting off the top of the salary scale at, say, $12,000 per year as compared to $18,000, will mean a reduction of productivity of 3 to 8 fold, according to the statistics deduced in connection with Fig. 15. Statistically, for the higher salaried man the return per dollar of salary is two to five times as great so far as individual productivity is concerned. If leadership qualities vary in a way similar to productivity, the return from increased salary will be enormously greater since an effective leader may substantially improve the output of many men.

[8] Organization and Administration of the Military Res. and Dev. Programs, Twenty-fourth Intermediate Rep. of the Committee on Government Operations; August 4, 1954.

In closing, I should emphasize that there are outstanding exceptions to most statistical results. Government laboratories do succeed in retaining a few outstanding individuals. These are unfortunately exceptions rather than the rule. Because of the present top limits on Civil Service salaries for scientists, the taxpayer's dollar is buying less research value than it should. A policy of having more highly paid positions might well double the return per dollar. It might also contribute significantly to offsetting the lead which the U.S.S.R. has currently gained in numbers of technical degrees granted in universities per year.

IX. Acknowledgment

The preparation of this paper has been made possible by the cooperation of a number of individuals and organizations. In particular, I would like to thank Dr. Lauriston S. Taylor and Mrs. Shea Kruegel of the National Bureau of Standards, Dr. L. J. Haworth of Brookhaven National Laboratory, Dr. John K. Herzog of Los Alamos Scientific Laboratory, and Dr. C. H. Townes of Columbia University. I would like also to acknowledge certain anonymous help. The appearance of this article in the Proceedings of the IRE results from a suggestion by E. W. Herold.

Mr. STEPHENS. I would like to make one final comment on a point which Norman Ramsey made.

The suggestion I made of presenting critical problems to creative minds was actually a generalization of several examples, one of the most prominent being a Navy bull session, or a rap session, as we call it today, on a problem having to do with keeping track of all ships I should say, on which I first met him. And he was the one that came up with the most creative solution to that problem. So he is speaking from firsthand experience when he made that suggestion of meetings and discussions and conferences of that type.

Chairman BENTSEN. Thank you very much, Mr. Stephens.

Again, I am very appreciative of the attendance and the contributions that you gentlemen have made, and I think you have developed quite a record.

Mr. RABINOW. We thank you for the opportunity to speak.

Chairman BENTSEN. The hearing is adjourned.

[Whereupon, at 12:05 p.m., the subcommittee adjourned, subject to the call of the Chair.]

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