ADDRESSES AND CONTRIBUTED PAPERS

SCIENTIFIC RESEARCH AND ECONOMIC INDICATORS

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When C. P. Snow wrote his treatise on *Two Worlds*, he emphasized the different world views of scientists and non-scientists (11). His viewpoint was emphasized by the question, "How often is the second law of thermodynamics discussed at cocktail parties?" I have tried for the past ten years to do just that, but with limited success. Now Jeremy Rifkin has written a book, *Entropy* (9), based on the penetrating analysis by Georgescu-Roegen of economics and the second law (5).

As we enter the eighties, it is necessary that all the tools at our command be brought to bear on social problems that we have come to call *economic*. I believe that C. P. Snow's *Two Worlds* in this matter relate to disinterest and self-interest on the part of scientists, who I believe have the necessary knowledge for purposes of solving these problems. On the other hand, social scientists and economists are making proposals such as enhancing productivity through basic research and other scientific activities, misleading us because of their lack of experience and understanding of current scientific techniques, culture, and organization.

My comments are based on involvement in the American science/research arena for the past 30 years. This involves 20 years as a scientist, engineer, and administrator with an aerospace division of a large American automobile manufacturer. Those twenty years were sandwiched between exposure to academic research in the early 50s as a graduate student; and, to academic and non-profit research in the late 70s as an administrator. My approach to administration is oriented toward motivating individuals to develop and make their scientific talents available by providing the appropriate instrumentation and equipment, clear-cut goals, and a demanding, but intellectual, environment. This has led me to believe that the two worlds gap is widening—not closing—and that we, as scientists, are largely responsible. I am also aware that my comments may be deemed unsophisticated by economists, but risk that criticism in order to aid in developing the forum for communication.

Let us review, briefly, the economic indicators related to our social problems. First, of course, is the question of the depressed value of currencies, in particular, the American dollar. Figure 1 is a plot showing that the cost of industrial materials has grown more rapidly than consumer prices since 1967. The latter have increased by a factor of 2½ in that time period. At the same time, the balance of trade for the U.S. has been negative. Figure 2 demonstrates that oil shocks have modulated a basic trend showing a lack of competitiveness in the marketplace on the part of American products (8). For instance, in the days following the first oil boycott, OPEC countries used their surplus to import American products, thus, producing a surplus. This market was quickly saturated and the downward trend continued. We now are experiencing a second wave of that same effect which, unfortunately, will apparently not reach zero this time.

An economic term called *productivity* has been identified as a significant

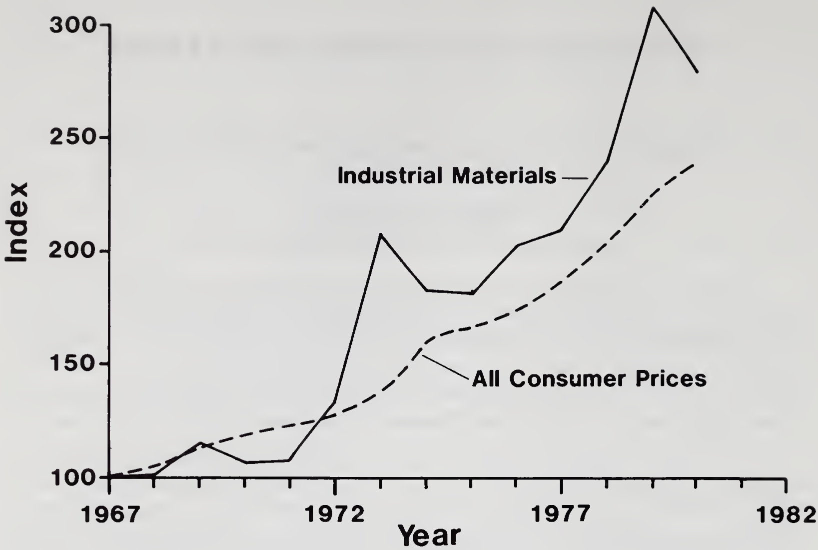

FIGURE 1. *Inflation*

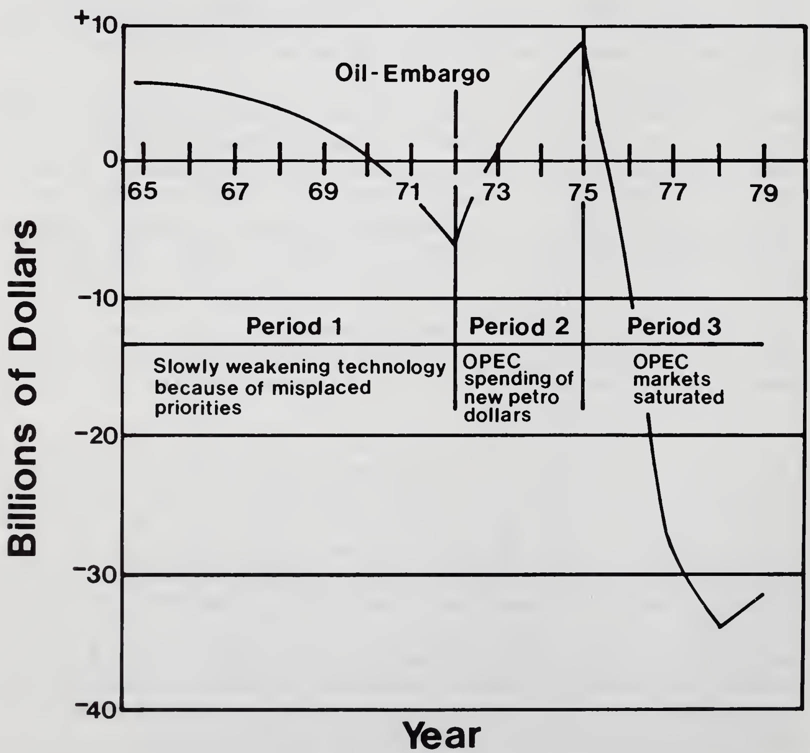

FIGURE 2. *U. S. Trade Balance—All Commodities*

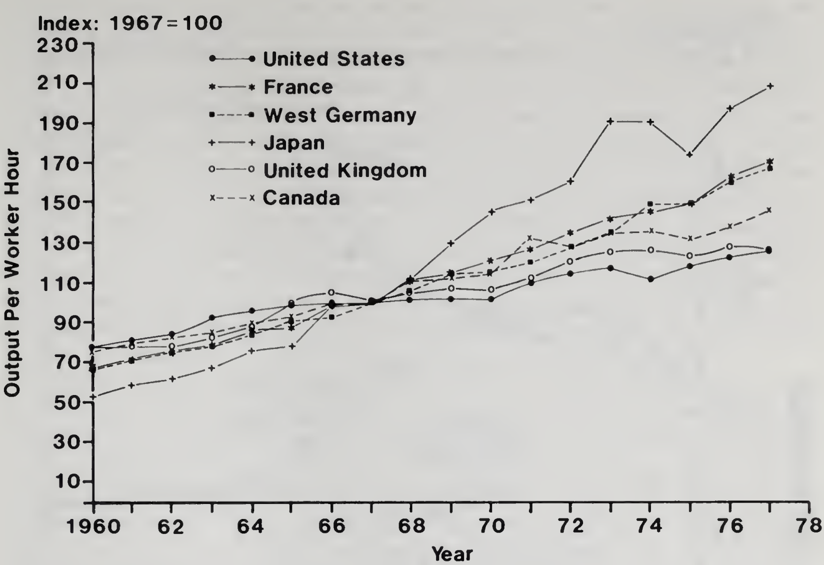

FIGURE 3. *Productivity Change in Manufacturing Industries By Selected Countries, 1969-1977*

culprit. Figure 3 and Table 1, however, show that although U. S. productivity is increasing more slowly than that of other countries, it is the highest in the world and is still increasing (3,2).

Further, it has been argued that lack of research and development is the basis for lowered productivity; and, therefore, a root cause of economic problems.

TABLE 1. *Real Gross Domestic Product Per Employed Civilian, for Selected Countries Compared With the United States: 1960-77*
[Index, United States = 100]

Year	United States	France	West Germany	Japan	United Kingdom	Canada
1960	100	55.4	52.4	24.7	51.1	86.6
1961	100	57.0	53.1	27.2	49.8	85.6
1962	100	58.0	53.1	27.6	47.9	85.6
1963	100	59.0	53.2	29.6	48.5	86.2
1964	100	60.0	55.2	32.1	49.1	86.0
1965	100	60.8	56.2	32.2	48.2	85.6
1966	100	61.2	58.1	33.4	47.4	83.5
1967	100	63.4	57.3	36.8	49.0	83.4
1968	100	64.2	59.5	40.0	49.7	84.6
1969	100	67.6	63.2	43.9	50.4	86.2
1970	100	71.4	67.0	48.7	52.6	88.6
1971	100	72.9	67.0	48.7	52.6	88.6
1972	100	74.8	68.5	53.9	53.6	90.7
1973	100	76.4	70.2	56.5	54.8	90.8
1974	100	80.0	74.3	58.0	56.0	93.0
1975	100	81.2	74.7	59.5	55.4	91.9
1976	100	83.1	77.7	60.8	55.6	92.2
1977 (prel.)	100	84.7	79.1	62.2	55.1	91.6

SOURCE: Department of Labor, Bureau of Labor Statistics

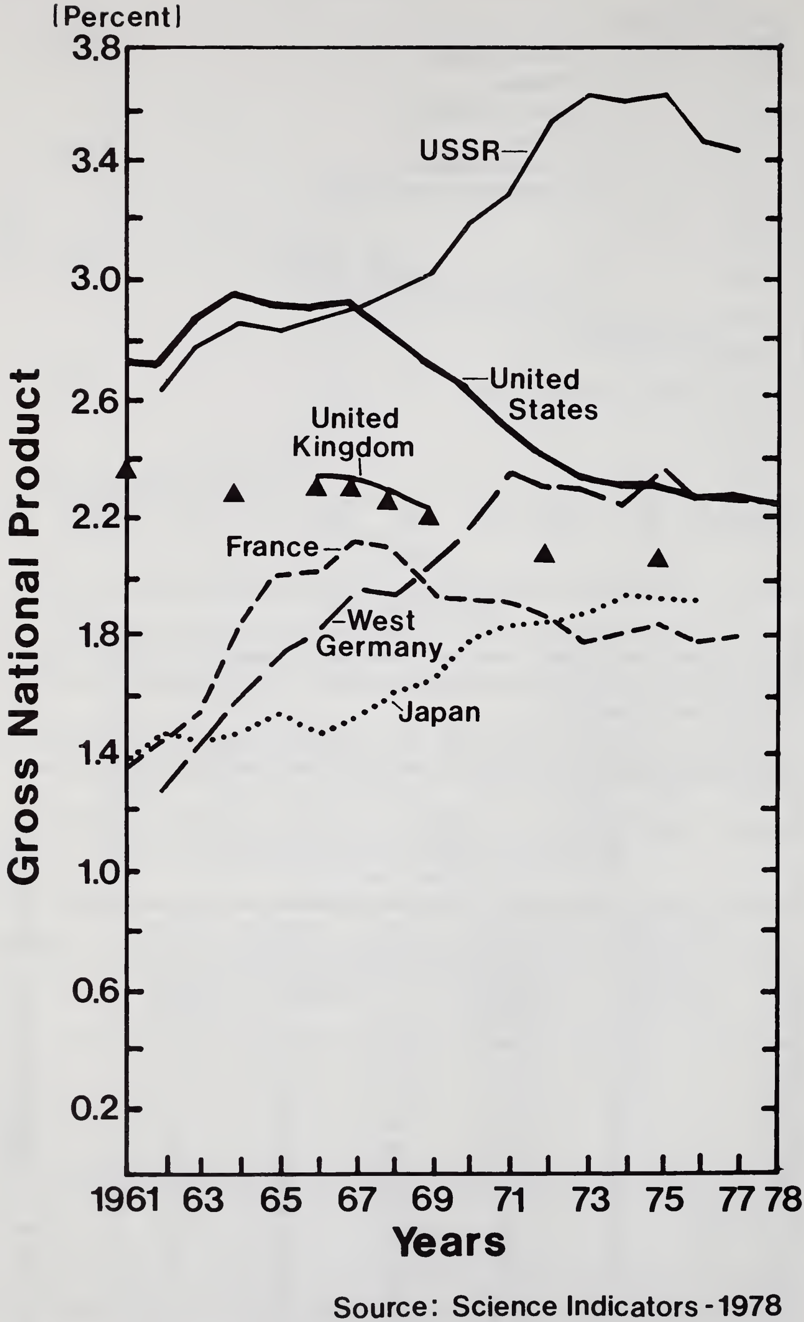

FIGURE 4. *National Expenditures for Performance of R&D as a Percent of GNP by Country, 1961-78*

Figure 4 demonstrates that, on the contrary, U. S. R&D efforts are supported not only at a higher dollar level than any other country in the West, but also at a higher level of percent Gross National Product (10). I would add that the recent Nobel awards emphasize the fact that the U. S. is *the* world leader in scientific research. In the magazine, *Science*, a speech by Lewis Branscomb of IBM Corporation was recently condensed as a relevant editorial. He pointed out that we are still world leaders in science, our engineering talents are still tops, and the quality of our technology is still the highest in the world. He summarized by saying, "The picture of American science and technology today is one of great strengths yet deep doubts; of strong foundations and timid commitment; of critical importance to the economy and uncertain political priority. If indeed our domestic and our foreign trade performance are poor, is lagging technology the symptom or the cause? And if technology lags, is this because the steam has gone out of our science? Or because of a failure of economic policy and industrial will?"[1]

Although there are many versions of cause and effect for these ills, they generally take the form of decrying lack of support for a given ingredient. For instance, certain manufacturers call for trade restrictions, the construction industry for lower interest rates, and economists discuss taxes and tax incentives at great length. In scientific circles, the connection is made between R&D support, innovation, and productivity. Thus, spokesmen for science are calling for ever greater support in more or less traditional form as a major component in improving American productivity and, thereby, combating inflation.

Another major source of inflation is clearly the increase in the price of oil; and, to a lesser extent, other strategic materials. These increased prices feed through the economy and increase the cost; and, therefore, the price of all goods, and especially food stuffs, since American farming techniques have become highly energy intensive. Once again the proposals made to solve this aspect of inflation are generally related to the needs of the petitioner to a greater extent than the solving of the problem. For instance, consumer groups call for more price controls, oil companies for lifting of environmental restrictions and the opening of more regions to exploration and drilling, and economists once again discuss tax and subsidy approaches. Engineers and scientists recognized at an early stage the need for their expertise in meeting this challenge. However, the outcome has been a wealth of self-serving proposals generally emphasizing that support of R&D will reap energy benefits in the future. A good example is the politicization of fusion research. This interesting, but costly, scheme is not relevant to the liquid fuel crunch of the eighties, but its public relations announcements are timed to concur with congressional budget hearings.

In summary, the consensus scenario is that productivity decline and foreign energy monopolies, together with normal inflationary pressures (wage demands), have caused a serious social problem called *inflation* (3). Our federal government is responding to pleas from various constituencies to provide support to those special interest groups in order to solve the problem. In particular, the science establishment is calling for greater and greater support of R&D in order to enhance productivity and replace foreign oil.

I believe there is a serious deficiency in this scenario of increasing R&D budgets (already high) as a response to inflation, unless we make major changes in the way we do our research. It seems to me that we are not managing the R&D

[1] Lewis Branscomb, "Needed: Conviction to Match Our Science," Science, August 1980, Vol. 309, No. 4457, p. 641 (excerpted from a commencement address at Polytechnic Institute of New York, May 29, 1980).

establishment in a way that is beneficial to the nation and that this ineffectiveness has three causes:

1. *Insufficient knowledge on the part of decision makers.* The combination of economists, industry leaders, governmental bureaucrats and elder scientific statesmen lack knowledge of modern hardware technologies. The extremely rapid growth and complexity of such fields as microelectronics makes anything but direct experience with laboratory work or design cause rapid obsolescence in scientific knowledge. For example, some governmental officials do not have a realistic grasp of what research is, how it is carried out, what it can do, and what its limitations are. The more complex the methodology, the more mysterious it seems to those who are not privy. This leads to an understandable frustration in that these officials have an unrealistic expectation of solid answers being made available to major problems fairly quickly.

2. *Conflicts of interest.* All parties related to these problems perceive of them in a way that a greater need for their own expertise is required. For scientists, this has all too often amounted to requests for continuation of conventional research. Certainly, for example, there are different roles that researchers and policy makers must play. Many, if not most, policy issues have important political components that policy makers must weigh—one of our jobs as researchers, then, is to insure that we take into account the non-technical aspects of our research projects as well as the technical ones. More about this later.

3. *Time lag phenomena.* The symptoms of economic distress relate to causes which predate them by many years, in most cases. Analysts are not clear on either the nature of causes or the appropriate time lags. A good example of long-term innovation is the Cable TV industry. Work began on the research and development of this method of bringing the TV signal into the TV set as far back as the early 1930s. Forty-four years later we are finally seeing the rapid rise of marketing and installing this system for community use. Many patents and much research funding failed to bear fruit for many, many years; but persistent effort to bring this innovation to commercialization appears finally to be a reality.

I do not have a well-prepared solution to these many problems. There is little time in this talk to develop song and verse to rebut the conventional economics, not to mention support a different scenario. I would, however, like to express a possible viewpoint based on my own experience—one which is markedly different from those of present decision makers. Possibly this viewpoint could be analyzed by others for its value and added to the mix of propositions for problem solution.

The scenario, as I see it, is as follows:

1. The Productivity Index is an economist invention which is nearly meaningless. Its slowing growth rate probably relates to the steady increase in the service industry sector.

2. In the United States, innovation normally comes through smaller industries. The venture capital drought in the mid-1970s made this impossible. A five-year time lag makes this problem now evident.

3. Our energy problem is a result of consistent government *cheap energy* policies. This is a case of legislating science. The time lag involved here is of the order of ten years.

4. Our trade imbalance is due to the innovation slump mentioned previously with a major downward revision due to energy policy.

5. Inflation comes about because of increased dependence on service industries for growth in U. S. and the trade balance deficit.

6. The administration of R&D nationally has been poor and has been coordinated with the cheap energy policy. A major problem has been the type of research conducted, not the amount of research.

Before proceeding with further comments regarding my views of the root causes of our economic trauma, a short history of U. S. research and development taxonomy is in order.

Since World War II, research and development have been defined as follows (1):

> Basic Research. Basic research is that research which is directed toward increase of knowledge in science. The primary aim of basic research is a fuller knowledge or understanding of the subject under study, rather than any practical application thereof.
>
> Applied Research. Applied research is that effort which (a) normally follows basic research, but may not be severable from the related basic research, (b) attempts to determine and exploit the potential of scientific discoveries or improvements in technology, materials, processes, methods, devices, or techniques; and, (c) attempts to advance the state of the art. Applied research does not include efforts whose principal aim is design, development, or test of specific items or services to be considered for sale; these efforts are within the definition of the term *development*, defined below.
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> Development. Development is the systematic use, under whatever name, of scientific and technical knowledge in the design, development, test, or evaluation of a potential new product or service (or of an improvement in an existing product or service) for the purpose of meeting specific performance requirements or objectives. Development includes the functions of design engineering, prototyping, and engineering testing. Development excludes subcontracted technical effort, which is for the sole purpose of developing an additional source for an existing product.
>
> Systems Analysis. Systems and other concept formulation studies are analyses and study efforts either related to specific IR&D efforts or directed toward the identification of desirable new systems, equipments or components, or desirable modifications and improvements to existing systems, equipments, or components.

Of course, slightly different versions of these definitions abound.

Many new definitions and experiments in research and development methodology have been undertaken in recent years. In general, the purpose of these studies has centered around attempts to increase productivity as measured by such parameters as number of publications, patent activity, product innovation, and societal impact.

One of the major problems confronting research administrators, in this age of high costs, is that of increasing productivity. Measuring productivity is difficult at best; but, one way of doing it in the research field is to use the relationship between societal benefit from research to its cost.

Numerous problems are inherent in this methodology. The most important is that of the time constant between time at which research is conducted and time at which benefit is derived. Research managers have historically approached this by either selecting short-range projects or by breaking longer-range projects into definable shorter-range milestones. There is some question whether the first approach is research at all, while most research scientists view the latter as an unnecessary activity at best and more often substitute the intermediate goals for the social benefit. This, in turn, tends to further the already growing division between science and society. Good examples of the failure of this approach are the figures of merit developed by thermoelectricity groups in the 50s and by fusion groups in the 60s. The figures of merit were broken into material properties which set off research projects intended to produce those specific properties. Research efforts soon diffused and large expenditures led to no social benefits.

The development of a refined approach to improving R&D performance, by linking research more directly to a process, we have come to call *innovation.* Innovation is defined as that process by which a new idea is successfully translated into economic impact within our society. It is the introduction, sale or use of a new technology—whether product, process, or system, in the consumer or industrial marketplace. Thus, innovation encompasses the entire spectrum of research, development, invention, finance, marketing, production, management and sales.

Many studies have been conducted which link innovation in technology to small companies in the private sector. A recent study of this type made by John Gilman of Allied Chemical resulted in the graph in Figure 5, which plots inventi-

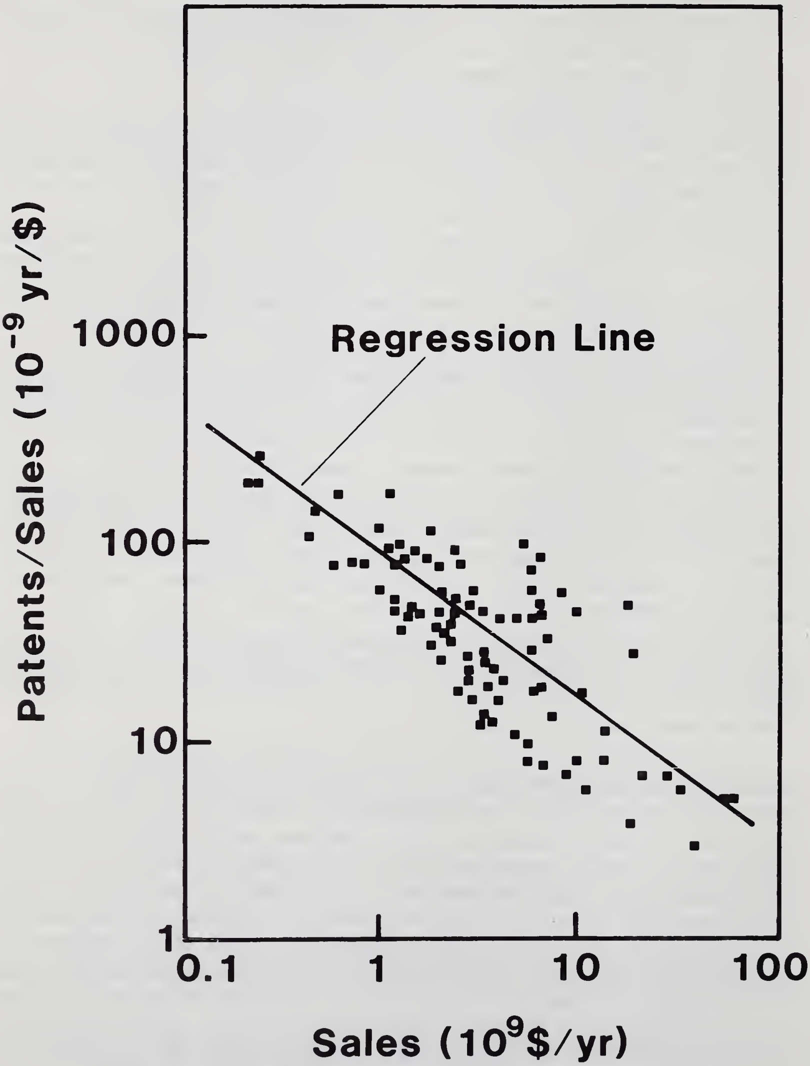

FIGURE 5. *Inventivity in Patents/Sales*

vity in patents/sales versus sales for American corporations (7). Note the inverse relationship. Statistically, according to Gilman, the inverse dependence is relatively high with a coefficient of 0.79. In Table 2, venture capital developed in the U. S. for high technology firms as a function of the years through the 70s, demonstrates clearly that we have starved just those companies which can best speak to the question of innovation (4). Although recent increases in this type of investment are occurring, the time lag between company formation and commercial significance is such that no effects have been felt on the economy.

TABLE 2. *Venture Technology Public Offerings 1969-77*

	Small Technical Firms Total Dollar Amount	Number of Offerings
($ millions)	($ millions)	
1969	1367	698
1970	375	198
1971	551	248
1972	896	409
1973	158	69
1974	16	9
1975	16	4
1976	145	29
1977	118	30

Source: MIT Development Foundation

The explanation for this cause and effect has been described by George Gilder as nothing more than the battle between the old and the new, or the past and future (6). Government and industry have established factories, offices, and bureaucracies which will seem worthless in the face of new technologies. He quotes the oft quoted Joseph Schumpeter as saying, "creative destruction is the essential fact of capitalism." As a result, large industry and government tend to want to plan while new companies go ahead and implement. This is noticeable in the energy program. While the government spends millions on developing an energy plan, small companies spend thousands on building and selling solar collectors.

The Indianapolis Center for Advanced Research, for the past seven years, has been conducting a living experiment in combining the various elements of R&D together with innovation and discovery to the end that the time lag between discovery and commercialization can be reduced. The results would thus be an improvement in U. S. competitive capability through R&D. The results have been encouraging. The characteristics of this type of research are fourfold:

1. Novelty and discovery.
2. Mission orientation. For ICFAR this means efforts that foster either
   a. the improvement of the quality of life (cultural);
   b. the development of new products, enterprises, processes, and services which strengthen the free enterprise system (commercial);

or,

   c. the development of nationally-recognized scholarly research in the life and physical sciences (academic).

3. The time lag between initiation of research and implementation of the results due to social and other non-technical barriers is taken into account in research planning.

4. A research style is employed which embodies special approaches to surmount social, economic, political, and other non-technical barriers to the application of results.

This concept of advanced research, using novelty, creativity, mission orientation, and time dependent planning, emphasizes the development of style. It is, therefore, appropriate to quote an acknowledged leader of research from another era, J. Robert Oppenheimer. He said: "It is style which complements affirmation with limitation and with humility; it is style which makes it possible to act effectively, but not absolutely; it is style which enables us to find harmony between the pursuit of ends essential to us, and the regard for the views, the sensibilities, the aspirations of others; it is style which is the deference that action pays to uncertainty; it is above all style through which power defers to reason."[2]

It is certain that we have had enormous development and diffusion of knowledge in the last three decades. This knowledge now transcends that to which we had previously become accustomed. It includes fantastic capabilities on the part of technicians, engineers, and scientists. The potential for accomplishment of objectives has been increased by orders of magnitude. We have come so far that the potential for accomplishment, for practical purposes, can be considered limitless. This basic argument means that the important decisions to be made in research and development at the present time must deal with deciding what we wish to accomplish. What are the questions that need to be answered? What kinds of problems are those that we must address? These are not simple questions, because we have many, many different problems reported by different groups. Our politics at the present time, to a great extent, are single issue politics. Nevertheless, the question is more one of selection of what it is we want to solve and to accomplish, than one of stocking our shelves with a great deal more information. A second type of question takes the following form: Define the economic, social, and cultural barriers to the application of the new knowledge generated in a given field and how can the research be best structured to surmount them. Thus, my view of the administration of research and development programs today is to determine what new accomplishments we must achieve and how shall we overcome the economic and cultural barriers to the application of the results of the necessary research.

Reviewing once again. . .With the enormous amount of knowledge and technique that we have available at the present time, it is my contention that it is a question of selecting objectives and how to treat the environment surrounding the science in order to bring about the application of the results of research.

Possibly a general example will be of help. We decry the depletion of our resources—everything from fossil fuels to critical metals such as cobalt are rumored to be, or in fact are, in short supply for various reasons. There are many approaches to the solution to these problems. First, recycling is an obvious approach to regaining major amounts of natural resources, both in fossil fuels and for critical metals. A second is underwater mining. The sea has available enormous resources, including fossil fuels. The nation is undertaking some programs now in the Baltimore Canyon on the east coast. Major resources of natural gas lie under the Gulf of Mexico. The decision to go ahead with respect to programs of this sort is not a question of technical know-how or more knowledge. It is a question of resolve to overcome the associated problems and a willingness to recognize

[2]From Dr. Laurence J. Peter. Peter's Quotations-Ideas of Our Time. William Morrow and Company, Inc., 1977.

them as problems. I might point out that there is a third resource in space, which is essentially infinite in scope. This is a much more expensive route, but one worthy of longer-range considerations.

Solutions such as these face economic and social barriers. These must be recognized and overcome by putting scientists and engineers to work in an advanced research modality. We have not yet, of course, done that. The problems must be well defined and the barriers to the application of the results of the research recognized and handled during the course of such a program.

For specific examples, let us turn to the Indianapolis Center for Advanced Research (ICFAR), where this concept of advanced research is practiced. One of the most interesting projects underway at the present time is early detection and treatment of breast cancer. Not only is breast cancer a major killer of women nationally, but it is a kind of death that is a very unhappy one; full of pain, cost, and emotional trauma. This problem should be directly approached, in my view. The research war on cancer that started during the Nixon administration; and, is presently funded through the Department of Health and Human Services and the National Cancer Institute, pays little heed to surgical techniques. Surgery is the present treatment for many forms of breast cancer. What we have suggested at the Center is an approach in which we employ ultrasound for early detection and then use focused ultrasound as a non-invasive surgical technique. We have made significant accomplishments in early detection of breast cancer using ultrasound through the support of the Showalter Residuary Trust. A small group of lay individuals in Indianapolis were willing to listen to our request for funds to directly attack this major problem. The National Cancer Institute, with its concepts of basic research and generation of knowledge that would be put on the shelf, is not responsive to this kind of approach. The Showalter Trust decided to support a major program to attack this problem some years ago. We defined the results desired as (1) an increase in the probability of early detection, (2) a use of ultrasound techniques to alleviate the normal radiation damage of mammography; and, (3) to provide a relatively low cost, rapid scanning device for wide-spread use. The barriers to the application of this kind of research, again, are numerous. First, there is the economic barrier to accomplish the instrumentation, the clinical testing, and all of the other technical work in association with such a project. For instance, let me suggest to you that after a major instrumentation company has laid out tens or hundreds of millions of dollars on the development of x-ray computerized tomography equipment, they do not relish the idea of a product coming on the market which will cost a tenth as much as their equipment and which is far more capable of early detection. Thus, it is difficult to find an avenue by which to commercialize the results of this type of work because of that kind of competitive situation. Another major barrier to the application of these techniques is the need for training of physicians. Of course, most radiologists and doctors have been in practice for a long time. You cannot depend upon medical schools to train physicians in ultrasound or you would be waiting for a decade to bring about the application of these techniques. It is not a simple matter to bring new instrumentation and techniques to bear because of the need for extensive familiarization on the part of the physicians who will actually bring about the application of the effort, not to mention technicians, nurses, and other medical personnel. The advanced research approach to this situation has been to develop the necessary instrumentation in leased space in a hospital environment working closely with radiologists and other medical technologists throughout the project. At the same time a small company, Medrix, Inc., was formed in order to market the resulting system. The diagnostic phase of this effort is now nearing production having been demonstrated to be effective (see Figure 6).

FIGURE 6. *Prototype of Ultrasound Breast Scanning System*

Another example of advanced research is being conducted by the ICFAR Urban Systems Engineering Department. These scientists recognized, during an early study of energy usage in the City of Indianapolis, that a major problem existed in terms of energy consumption at the Belmont Street incinerator plant. As we looked further into this waste disposal plant, it was recognized that not only were there major costs involved in energy consumption; but, also, in replacement of critical parts. Therefore, a study was initiated in conjunction with the City to thoroughly instrument the incinerator plant to determine where losses were occurring and how improvements could be made. Prior to commencing the study, the results required were defined as (1) major fuel reduction, (2) stack emission reductions so as to better meet EPA air quality requirements; and, (3) operating net reductions. A running incinerator plant was used as a test bed to obtain new information with respect to the operation of sludge incineration plants (see Figure 7). This research was successful and a solution was recommended involving a relatively simple feedback control system in which the gas composition of the exhaust stack controlled air flow to the incinerator. The resultant relatively constant temperature will not only reduce fuel flow, but also increase incinerator part life. This type of operation was successfully demonstrated for three days continuously in March of 1978. This demonstration not only showed lower operating costs but also demonstrated emissions low enough to meet EPA standards. Permanent changes are being made in the operation of the plant, which will bring about a reduction in cost of as much as $300,000 annually. Further, a higher operational reliability can be attained, thus, reducing the requirements for future capital investments on the part of the City. In this program, scientists and engineers from ICFAR worked closely with supervision and workers at the incinerator plant itself, just as in the breast cancer detection project they worked with radiologists and hospital personnel. In this way, much of the not-invented-

Figure 7. *Combustion Scientists Review Urban Sludge Incinerator Instrumentation*

here kind of attitude on the part of the practitioner is overcome because they are part of the development program. Finally, a small company was formed which will offer this service to other cities throughout the nation.

As a final example of advanced research, let me present a logical approach to addressing the energy problem. First the major research results required in order to become more independent of foreign oil are: (1) conservation—methods to reduce energy usage, (2) exploration enhancement—techniques to reduce costs and time in finding new fossil fuels; and, (3) development of alternative fuels. In each of these cases, rapid strides can be made through application of the techniques of advanced research. ICFAR efforts take the following form:

(1) Research on infiltration losses in fenestration in conjunction with manufacturers of storm windows and conducted in the housing environment with the aim of reducing cost of high thermal flow resistant storm windows for retrofit applications (see Figure 8).

(2) Development of computer algorithms for tomographic reconstruction of underground regions by means of seismic, electromagnetic, or other signals through study of present fossil fuel systems in conjunction with producing companies.

(3) Develop solar/gas hot water heating systems in conjunction with a gas utility for purposes of financing and maintenance arrangements.

Each of these efforts has involved a licensing agreement with a small company to bring about commercialization of the results.

There are many other projects and programs at ICFAR, but all of them follow this general approach of advanced research. This is a type of research involving discovery, new techniques, and innovation, but the sense of discovery occurs in the course of the accomplishment of programs which relate to the future of

FIGURE 8. *Thermal Resistance Measurement System*

the community and the nation, and are conducted in a style such that barriers to the applications of the research will be reduced as much as possible as the research results become available. The two cultures which C. P. Snow discussed so well are being brought together in an efficient manner at the working level.

In summary, steps necessary for alleviation of economic problems according to this scenario are:

1) Encourage venture technology capital.

2) Apply principles of advanced research in developing public/private sector research projects.

3) Take time constants into account in assessing results.

4) Support the scientists and the environment in which new ideas will continue to flow. For with the scientist, lies the source of the intellectual property that will eventually benefit society.

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Photo by Lloyd Anderson, IAS

ROBERT E. HENDERSON

Robert E. Henderson checks the projector and transparencies for use with his Presidential Address, "Scientific Research and Economic Indicators."

Photo by Lloyd Anderson, IAS

President Robert E. Henderson and Richard L. Conklin, Chairman of the Fellows Committee, greeted these distinguished members of our Academy at the annual banquet. These eleven members represent persons who have been Fellows of the Academy for at least twenty years. Numbers in parentheses indicate the year in which each person was elected a Fellow.

Front Row, Left to Right: Richard L. Conklin ('63), Willis H. Johnson ('50). William A. Daily ('49), Winona H. Welch ('35), Fay K. Daily ('53), and Robert H. Cooper ('55).

Back Row, Left to Right: Alton A. Lindsey ('50), Harry G. Day ('53), Ernest E. Campaigne ('54), Robert E. Henderson ('79), Benjamin Moulton ('53), Ira Baldwin ('53), and William W. Bloom ('57).

At the time of this photograph, this group collectively represented 508 years of membership of which 336 years (66%) were served as Fellows.

Photo by Lloyd Anderson, IAS

WINONA H. WELCH

One of the highlights at the annual banquet at St. Joseph's College was the recognition of members of the Academy who have been Fellows for at least twenty years. Winona Welch, who received special recognition from President Robert Henderson, set a record for the group by being a Fellow for forty-five years.