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THE ECONOMICS OF INVENTION: A SURVEY OF THE LITERATURE

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I. INTRODUCTION

THIS paper reviews the literature on the economics of invention, with special stress on two aspects. The first is that invention is strongly motivated by perceived profit opportunities. Demand and cost factors play major roles with the state of scientific knowledge significantly affecting the cost and hence the profitability of invention. Much of the data on relative rates of technological progress can be explained in this fashion.

The second aspect of invention stressed in this paper is that it is an activity often carried on under conditions of great uncertainty. The tremendous difficulty of predicting or planning technological advance closely explains why the large industrial research laboratory has not completely eliminated the private inventor and why many of our most significant inventions still come from small workshops. Inability of established firms to perceive the profit potentials of radically new inventions has often necessitated the creation of new firms, as in the case of radio and the jet engine. Case studies to illustrate these two aspects of invention are examined; these include development of nylon, the atomic bomb, military aircraft engines, hybrid corn, and long-range radio communication.

Finally, the management practices of industrial research laboratories are reviewed. It is found that, despite talk of close controls, budgetary and otherwise, much industrial research is conducted under very loose control. In general, the greater the technical advance represented by the object of research, the looser are the controls. There appears to be recognition in practice of the great uncertainty attached to major inventions and research findings.

This paper is a selective summary of the literature on the economics of invention. It is not, however, an annotated bibliography, nor is the literature coverage exhaustive. Rather the paper is an attempt to focus on the subject the light shed by many different authors writing in many different, though related, fields. The views presented in this paper are by no means held by all the authors, but the basic ones would probably be accepted by a majority of them.

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The striking increase in scientific research, invention, and technological change in Western culture during the last three and a half centuries has been remarked by innumerable historians, philosophers, and scientists. Economists have recently attempted, with some success, to measure the relative importance of technological change in economic growth—only one dimension of the effects of scientific research and invention but certainly an important one. Traditionally, economists have attempted to explain increases in output per worker by increases in the quantity of capital equipment used and by technological improvement. A recent study by Solow yields the rather surprising conclusion that only 13 per cent of the increase in output per worker in the United States between 1910 and 1950 can be statistically explained by increases in capital equipment per worker.[1] The work of Abramovitz yields similar results.[2] Both authors are well aware of the great conceptual difficulties that underlie their analysis. And Solow stresses that, even if the quantitative conclusions are accepted, improvement in the quality of labor, better allocation of resources, and many other factors must share with invention the credit for the remaining 87 per cent. But even a casual comparison of the goods on the market and the production techniques used to make them today with the goods and production techniques of fifty years ago dramatically indicates the tremendous role that invention has played in improving our standard of living.

[1] Robert M. Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics*, August, 1957.

[2] Moses Abramovitz, "Resource and Output Trends in the United States since 1870," *American Economic Review, Papers and Proceedings*, May, 1956.

Although Solow's figures imply that, if the quantity of capital per laborer had remained constant, the rate of increase in productivity would have been about 87 per cent of what it actually was, such an implication would certainly not be warranted, even if we could rely on the accuracy of the statistical observations. For the tie between technological change and capital formation is close—so close perhaps as to make the economists' distinction between technological improvement and capital increase a misleading one for the purposes of explaining growth. Although capital per worker can often be profitably increased without significantly changing the existing production process, and without invention "outside" the industry, by using "off-the-shelf" machines to do jobs previously done by labor, significant increases in the capital-labor ratio are usually generated by development of new production processes and machinery, inventions, and innovations. And, of equal importance, invention must usually be embodied in new plant and equipment before it yields increased productivity.

Invention of a new and useful device is one thing; its production and use are something else again. Invention requires innovation on the part of both producers and consumers before economic change results, and the struggles of countless inventors to get someone to finance or undertake production of their inventions and the often fruitless efforts of producers to market new products are common lore. But this paper is not concerned with innovation, it is concerned only with how inventions occur.

Section II of this paper looks at the rate and direction of inventive activity and their relation to expected profitability. Section III examines the effect of uncertainty upon research and development. Section IV is a brief study of the way that industrial research laboratories conduct research and development.

II. INVENTION AND EXPECTED PROFIT

In 1953, the latest date for which relatively sophisticated figures are available, total expenditure on research and development was about $5.4 billion.[3] All evidence indicates that since 1953 expenditure on research and development has increased markedly; $10 billion seems a reasonable estimate for 1958. A large share, but not all, of our resources engaged in research and development is directed toward invention.

The term "invention" is one of the most illusive in the English language, but the fundamental idea underlying it is that of something new, something which has not existed before. An invention may be defined as a new combination of new or old elements—things or ideas. The proportion of new elements in can invention is often small; a modern textile machine, for example, can be disassembled into a heap of perfectly familiar machine elements. Granted that the term can be used to include all human creative activity, from composing a poem to developing a new chemical process, for our purposes it is well to define the term narrowly so as to include only patentable inventions.

Rossman divides invention into two broad classes: basic inventions and improvements on existing basic equipment or processes.[4] The former usually (but not always) involve applications of new knowledge; hence they commonly involve new elements as well as new combinations of old elements. The dynamo, telephone, radio, and incandescent lamp are basic inventions. Improvements are primarily marginal inventions, usually (but not always) based on experience with existing basic equipment and seldom involving strikingly new elements. Obviously, the line between basic invention and improvements is blurred.

A. INVENTION AND DEMAND

One of the most interesting studies of invention is Gilfillan's *Sociology of Invention*.[5] His argument can be paraphrased as follows: Inventions generally occur through the accretion of details rather than as major creations (thus Gilfillan implies that Rossman's "basic" inventions are relatively rare and that most so-called basic inventions are actually just evolutions of existing objects). They constitute combinations of ideas previously known and are not necessarily based on prior formal scientific ideas. Instead, most inventions are based on common practical experience and knowledge of persons well acquainted with a particular industry or a particular machine. Social need, usually manifesting itself through perceived opportunities for private profit, not chance, is the cause of inventions. When the time is ripe, they are inevitable. Revolutionary inventions tend to be made by persons outside of, but close to, the field to which the invention applies. These men are conversant with the needs in a field and with the existing techniques but have not been indoctrinated with common ways of thinking about the field. Following the breakthrough of a basic invention, there is at first a rising, then a falling, rate of increase in practical adoptions of the new

[3] National Science Foundation, *Science and Engineering in American Industry* (Washington, D.C.: Government Printing Office, 1956).

[4] M. J. Rossman, *The Psychology of the Inventor* (Washington, D.C.: Inventors Publishing Co., 1931).

[5] S. C. Gilfillan, *The Sociology of Invention* (Chicago: Follet Publishing Co., 1935).

invention. In time the new invention is made obsolete by still newer inventions, and its use declines or falls off entirely. A basic invention is usually followed by a number of improvement inventions. The rate at which improvements are made rises as practical experience with the equipment increases, then falls off as improvements reach physically determined limits.

Gilfillan's theory of invention thus stresses two major factors—demand and learning through experience—and leads naturally to the deduction that, after an initial breakthrough and the subsequent burst of secondary inventions, the rate of invention should slow down as demand reaches its equilibrium rate of growth and as the basic invention is perfected. This belief is supported by a number of writers. Kuznets has found that the rate of technological progress in an industry shows definite retardation after a time[6] and that concurrently the percentage of all patents issued which relate to the industry declines.[7] Merton has published data on patents taken out in a number of industries.[8] Very shortly after the breakthrough marking the birth of an industry, the number of patents granted starts to rise markedly. After a few years the number of patents reaches a peak and then falls. Jerome's study of the life-histories of a number of machine tools also tends to confirm the Gilfillan hypothesis.[9] Hirsch,[10] Asher,[11] and others, in studies of manufacturing costs, find that the unit costs of producing a new product decline as experience accumulates. The decline is at first rapid, then slacks off. Unfortunately, little work has been done relating "learning curves" to improvement-type inventions, but Hirsch believes that a large share of the cost reduction should be attributed to changes in process and product resulting from experience.

Most writers today would agree with the basic outlines of Gilfillan's view but might object at several points. First, he regards the process of invention as basically mechanical in nature and automatic in direction and results. This may be true as a broad statistical mean, but invention in the small is anything but mechanical, automatic, and predictable. Chance plays a tremendous role. Second, many writers today would argue that Gilfillan, in attempting to refute the then-popular "hero" theory of invention, went too far in the other direction in implying that the activities of specific individuals count for naught—that, if Goodyear in 1839 had not discovered a way to improve rubber by vulcanizing it, someone else would have soon after. Modern critics might ask, "How soon?" Third, in arguing that social need is the major factor explaining invention, Gilfillan overemphasizes the "demand" side. Many inventions occurred when they did because of shortly preceding scientific breakthroughs that had lowered the "cost" of invention. And major scientific breakthroughs seem often to occur independently of "social need."

[6] Simon Kuznets, *Secular Movements in Production and Prices* (Boston: Houghton Mifflin Co., 1930).

[7] For a discussion of the value of patent statistics as a measure of the rate of inventive activity see Jacob Schmookler, "The Level of Inventive Activity," *Review of Economics and Statistics*, May, 1954.

[8] Robert K. Merton, "The Rate of Industrial Invention," *Quarterly Journal of Economics*, 1935.

[9] Henry Jerome, *Mechanization in Industry* (New York: National Bureau of Economic Research, 1934).

[10] W. Z. Hirsch, "Manufacturing Progress Functions," *Review of Economics and Statistics*, May, 1952.

[11] Harold Asher, *Cost Quantity Relationships in the Airframe Industry* (Doc. No. R-291 [Santa Monica, Calif.: The RAND Corporation, 1956]).

B. INVENTION AND SCIENTIFIC KNOWLEDGE

Invention and scientific research, though tending increasingly to conjoin in practice, are best kept separate conceptually. Scientific research may be defined as systematic investigation for the purposes of discovering new knowledge, where, in this context, knowledge may be loosely translated as ability to predict. No strict line can be drawn between scientific research and all other human activities. Men have always experimented and observed and have always generalized and theorized, thus men have always been, at least in a limited way, scientists. And knowledge has often (usually?) been acquired in activities not consciously directed toward the pursuit of it. But even fuzzy definitions often have value.

The line between science and invention is not sharp. In Conant's terms there is a continuous spectrum of scientific activity.[12] At one end of the spectrum is basic scientific research; at the other end, engineering development. Moving from the pure-science end of the spectrum to the engineering end, the goals become more closely defined and more closely tied to the solution of a specific practical problem or the creation of a practical product. And usually the degree of uncertainty as to the results of a specific project, if successful, decreases as we move across the spectrum. Though surprises sometimes occur at the development end, they seem to be the rule, not the exception, at the basic-research end.

Much inventive effort has proceeded more or less independently of science—that is, little scientific knowledge was required or the relevant scientific knowledge was long available. Such useful inventions as the zipper and the safety razor presuppose little scientific training on the part of the inventor. And the gas refrigerator, though scientists had long known that expanding gas absorbs heat, was not invented until the twentieth century.

But new scientific knowledge greatly facilitates invention. In the activity of invention, as in most goal-directed activities, the actor often has a number of alternative strategies among which he must choose. If he is attempting to invent in a field where there is little underlying knowledge, he must grope by trial and error, seeking the needle in the haystack by examining the hay, handful by handful. Many advances occur this way. Goodyear's attempt to improve the characteristics of rubber and Edison's attempt to develop an incandescent lamp are cases in point.

Conant defines the degree of empiricism in a field as the extent to which the results of previously untried experiments cannot be predicted. The stronger the underlying scientific theory, the less the degree of empiricism. The development of the contact method of making sulfuric acid—an inventive effort in a field where there was considerable scientific knowledge—required little trial-and-error experimentation, for paper-and-pencil calculations enabled the results of trying one alternative or another to be closely predicted. Hence the number of alternatives which actually had to be tried was greatly reduced. As scientific knowledge increases, the expected amount of trial-and-error groping needed to find a solution to a problem decreases. Thus an increase in scientific knowledge reduces the cost of invention.

Many analysts writing during the 1920's and 1930's argued that the connection between science and invention was becoming radically different from

[12] James B. Conant, *Science and Common Sense* (New Haven, Conn.: Yale University Press, 1951).

what it had been in the pre–twentieth-century world. In the modern industrial research laboratory new scientific knowledge was pictured as being quickly translated into invention by teams of men skilled in the several sciences, and expected profitability of a new, not-yet-invented, product was pictured as quickly resulting in a scientific research project to provide the knowledge necessary for that invention. Thus science stimulated invention and invention stimulated science, and the industrial research laboratory institutionalized this arrangement. In contrast, these analysts pictured the typical pre–twentieth-century inventor as a clever craftsman, innocent of formal science. It was argued that in the eighteenth and nineteenth centuries in Western society scientific knowledge was primarily seated in the upper classes and technological knowledge in the lower classes; thus there was little reciprocal stimulus.[13] But recent evidence[14] indicates that, even during the eighteenth and early nineteenth centuries, the inventor was often well read in formal science, and invention often resulted directly from new scientific discovery. James Watt had a good knowledge of formal science (informally acquired), and his invention of the improved steam engine with a separate condensing chamber stemmed directly from the work of his friend Black on latent heat. Oliver Evans and Richard Travethick, who independently invented a high-pressure steam engine, both were self-educated in science. Several of the important inventors of textile machinery were good amateur scientists. And the relationship between science and invention was a two-way relationship long before the twentieth century. The work on thermodynamics of Carnot, Joule, and Rankine came after, not before, the development of an economic steam engine and was stimulated, in large part, by a desire to explore the theory of the steam engine and so permit the engine's improvement. This is not to say that the link between science and invention is not now a closer one than it was. But the evidence indicates that the difference is one of degree, not of kind.

Ogburn, in a book equally as well known as Gilfillan's, argues that, though social need may stimulate greater inventive effort, the social heritage of knowledge and technique is the real mother of invention.[15] Ogburn, like Gilfillan, is impressed by the great extent to which multiple inventions have occurred, but he argues that the cause of this is not that current needs stimulate inventive effort in certain directions but that current breakthroughs in knowledge make particular inventions, previously impossible or extremely difficult, easy and natural. He argues that often inventions occur when there is little social need for them —they are adopted by society only at some much later time—and that, though many social "needs" have existed for a long time, we still do not have satisfactory solutions to them. But Ogburn, like Gilfillan, overstates his case. Cost and demand determine profitability or net value together, like the two blades of a scissors. Though by the late eighteenth century the social stock of knowledge was ripe to produce the steamboat, so was social need, particularly in the United States, increasingly pressing. Both of

[13] Yale Brozen, "Research, Technology, and Productivity," in L. R. Tripp (ed.), *Industrial Productivity* (Madison, Wis.: Industrial Relations Research Association, 1951).

[14] J. Jewkes, D. Sawers, and R. Stillerman, *The Sources of Invention* (London: Macmillan & Co., 1958).

[15] William F. Ogburn, *Social Change* (New York: Viking Press, 1933).

these factors certainly underlie the fact that Fulton, Stevens, Symington, and several others all developed working steamboats within a few years of one another.

The analysis and many case histories of Usher's *History of Mechanical Invention* illustrate the interplay of moving frontiers of knowledge and growing need upon the direction and likelihood of success of individual "acts of novelty."[16] Likewise does Kaempffert's history.[17] The growing specialization and urbanization of the American economy in the late nineteenth century increased the value of something like an automobile. And at the same time advances in metallurgy, machine-tool technology, and engines made the invention of a working automobile much less difficult by reducing the number of new components needed. Similarly for the telephone and telegraph.

Thus it appears that conditions of cost and demand may be as important in explaining invention as in explaining the rate of output of a firm or an industry. Increases in scientific knowledge, by reducing expected cost, and increases in demand, by increasing expected gross revenue, act to increase the expected profitability of an invention and hence the inventive effort applied. This analysis by no means implies that inventors, private or hired, are spurred exclusively by economic motives. But, if economic motives are important ones, then an "economic" theory of invention should help to explain and predict the rate and direction of inventive effort.

Clearly, the inventive efforts undertaken in the research and development laboratories of private companies are strongly profit motivated. And the efforts of the private inventor, which still play a major role, likewise seem to be strongly affected by perceived profit opportunities. Rossman asked 710 inventors: "What motives or incentives cause you to invent?"[18] For most inventors, inventing is creative self-expression, the two most frequently mentioned motives being "love of inventing" and "desire to improve." "Financial gain" was ranked third. "Prestige" is of course important. Miscellaneous motives, most of which are rephrasings of the motives listed above, ranked lower. But the inventors indicate rather clearly that, in the absence of the possibility of substantial financial reward, many, despite their love of inventing, would spend far less of their effort on inventing and more on other, more lucrative, occupations. And many of them said that the direction of their inventive efforts was influenced in no small part by their estimates of profit possibilities. Taussig's findings are similar to Rossman's.[19] Most inventors invent because they have a strong "instinct of contrivance." Curiosity and hope of fame play large roles. But Taussig found that inventors did choose from alternative inventive projects in part on the basis of relative expected profit.

C. PROFITABILITY AND PREDICTION OF INVENTION

Can an expected profitability theory help to explain varying rates of invention in different industries? Maclaurin has attempted to generalize regarding the characteristics of several American industries.[20] Those he considers technologically

[16] A. P. Usher, *A History of Mechanical Invention* (Cambridge, Mass.: Harvard University Press, 1954).

[17] Waldemar Kaempffert (ed.), *A Popular History of American Invention* (New York: Charles Scribner's Sons, 1924).

[18] *Op. cit.*, chap. x.

[19] Frank Taussig, *Inventors and Money Makers* (New York: Macmillan Co., 1915).

[20] W. R. Maclaurin, "Technological Progress in Some American Industries," *American Economic*

progressive—chemical manufacturing, photographic manufacturing, airplane manufacturing, oil-refining, radio- and television-set manufacturing, and electric-light manufacturing—were marked by the existence of large firms, and all, save radio and television manufacturing, were relatively difficult to enter. Those he considers technologically backward—food-processing, cotton-textile manufacturing, coal-mining, and house-building—were marked by the existence of many small firms and, save for food-processing, included no giants. The technologically dynamic industries, except for airplane manufacture, tended to rest on a thriving scientific basis and had what Maclaurin calls a "research conception" (i.e., they actively engaged in research), and all hired a considerable number of scientists with doctorates. None of the technologically backward industries had a "research conception," and none hired many scientists with doctorates. The entrepreneurial leadership of the firms in the dynamic industries (but not including chemicals) tended to be new men with engineering or scientific training. The leadership of the firms in the backward industries (save for house-building) tended to be inherited and had little training in engineering and the sciences.

Obviously, technical progress in an industry involves innovation as well as invention. And much of Maclaurin's analysis deals more with innovation than with invention. But several of Maclaurin's generalizations are compatible with the cost-demand theory of invention previously outlined. The cost of invention, in Maclaurin's progressive industries, is low because the sciences underlying the industry's technology are flourishing. And in Maclaurin's progressive industries, demand, though not necessarily great initially, has been growing rapidly. This suggests that the anticipated profit from an invention may be as strongly affected by anticipated future demand as by present demand. But it is hard to say whether technological progress resulted from demand growth or vice versa.

Maclaurin's conclusions about the importance of large firms and barriers to industry entry would be contested by many writers. The theoretical arguments pro and con are well known, so we shall be concerned here with the limited empirical evidence. Stigler has found that the industries in which output per worker rose most strikingly during the 1899–1955 period tended to be industries in which the degree of monopoly power (measured by the per cent of output produced by the top four firms) was falling.[21] And Brozen has found that one of the most cited reasons for engaging in research and development is a research and development program by a competitor.[22]

National Science Foundation data throw a little additional light on the subject. The 1953 survey shows that large firms do play the major role, at least in terms of expenditure, in our research and development effort.[23] Three hundred and seventy-five companies each employing over five thousand persons performed 70 per cent of the research and development work undertaken by industry. (This figure may be compared with the 40 per cent of total manufacturing employment provided by firms each employing over

Review, May, 1954. See also Maclaurin's "The Process of Technological Innovation," *ibid.*, March, 1950.

[21] George Stigler, "Industrial Organization and Economic Progress," in Leonard D. White (ed.), *The State of the Social Sciences* (Chicago: University of Chicago Press, 1956).

[22] Yale Brozen, "The Economic Future of Research and Development," *Industrial Laboratories*, December, 1953.

[23] National Science Foundation, *op. cit.*

five thousand employees.) But a very large number of small and middle-sized companied were engaged in research and development; nearly 85 per cent of the companies doing research and development employed less than five hundred persons. And progressive small companies have, in the past, proved to be the source of much of our technological progress.

The empirical research of Maclaurin,[24] Bright,[25] and Schlaifer[26] has led them to conclude that established firms, even progressive established firms, are usually backward about radically new inventions and that the birth of a new firm is often necessary to introduce an invention to the market. The established firms of the communications industry—the Bell Telephone Company, General Electric—were not interested in the new invention—radio. Established aircraft-engine companies would not work on the new jet engine. Brown, in his study of new firms established in Connecticut in the post–World War II period, has found that a large proportion were established to produce a new product invented by one of the founders.[27] Often the inventor quit his previous job to found a new firm because his superiors were not interested in his invention. Since an industry with a growing demand is more likely to draw in new firms than is an industry with a relatively constant demand, this could be an additional factor explaining why technical progress is more closely associated with growth in demand than with level demand.

Although established firms are often sluggish about venturing into new areas, particularly before new ideas have been put to a test, many established firms have purchased inventions after practicality has been demonstrated. Du Pont purchased the rights to Dacron. Kodak purchased the rights to what is now called Kodachrome. And in many instances only an established firm with large financial resources could have developed and marketed the new invention.

Thus the evidence as to what type of market organization is most conducive to technological change is conflicting. It may be that market organization of an industry is not so important as other factors, in particular a thriving scientific base and rapidly growing demand. For, though the market organization of the most technologically dynamic industries appears to vary widely, in all cases these industries do rest on a well-developed and developing science and have experienced a rapid growth of demand, though, as has been mentioned before, this rapid growth of demand is certainly in part a result, as well as a cause, of technological progress.

In addition to providing at least a partial explanation of relative rates of invention in different industries, the cost-demand framework also throws some light on the factors influencing the overall rate of inventive activity in an economy. Schmookler has found that the annual number of domestic patent applications is highly correlated with the number of technically trained personnel, though more so in recent than in former years.[28] Blank and Stigler have found

[24] W. R. Maclaurin, *Invention and Innovation in the Radio Industry* (New York: Macmillan Co., 1949).

[25] A. A. Bright, *The Electric Lamp Industry: Technological Change and Economic Development, 1800–1947* (New York: Macmillan Co., 1949).

[26] R. Schlaifer and S. D. Heron, *The Development of Aircraft Engines and Fuels* (Cambridge, Mass.: Harvard Business School, 1950).

[27] Gilbert Brown, "Characteristics of New Enterprises," *New England Business Review*, June, 1957, and July, 1957.

[28] *Op. cit.*

that, despite the many non-pecuniary factors affecting a young man's decision to acquire a scientific or technical education, relative income prospects do play an important role.[29] Thus it appears that the profitability of inventive effort, which is reflected in high salaries offered by companies to technologists and in opportunities for the private inventor, does affect the number of persons who choose to acquire training in technology and, consequently, the rate of inventive activity.

Schmookler has also found that deviations from the secular trend of patent applications are closely correlated with deviations from the secular trend of variable production inputs. His explanation for this is that, the greater the quantity of variable factor inputs, the greater are the returns from a factor-saving invention. Hence the level of inputs used at any time is a crude index of the aggregate demand for inventive effort. Schmookler believes that the causative relation between number of patent applications and quantity of variable inputs goes far toward explaining the retardation in the rate of growth of patent applications which has been noticed by many writers,[30] for the growth of variable factor inputs has also exhibited significant retardation.

Graue's study of patent fluctuations yields similar results.[31] He finds that patents issued tend to fluctuate with the general level of production.

Thus there is good evidence that fluctuations in expected revenue affect the over-all rate of inventive effort. Little quantitative work has been done relating advances in science and reduction in the expected cost of invention to the over-all rate of inventive effort. However, Brozen believes that the increasing fraction of American industry which rests on a scientific base will be a significant factor increasing our national research and development effort.[32]

III. UNCERTAINTY AND INVENTION

While traditional economic analysis focused almost exclusively on how conditions of demand and cost affect profitability and thus the resources applied to alternative activities, it is only in recent years that economists have given much attention to cost and demand uncertainties. Traditional demand and cost analysis goes a long way toward explaining the rate and direction of inventive effort. It is less successful in explaining invention itself, as opposed to inventive effort, because of the major role played by uncertainty.

A. UNCERTAINTY AND THE SOURCES OF INVENTION

Both Ogburn and Gilfillan picture the gradual movement of invention to the industrial research laboratory, gravitating there by the pull of applied scientific research. T. J. Kreps, speaking before the Temporary National Economic Committee, argues that this movement has radically changed the nature of invention:

> After 1850 the progress of science, particularly of the physical sciences, became so systematized that the invention of a product was first blueprinted before realized. . . . Thus the contact process for sulfuric acid started with elaborate mathematical computations because that was the only practical way in which to find the one best set of conditions under which the synthesis of sulfuric acid by the contact process

[29] D. M. Blank and G. J. Stigler, *The Demand and Supply of Scientific Personnel* (New York: National Bureau of Economic Research, 1957).

[30] See, e.g., A. B. Stafford, "Is the Rate of Invention Declining?" *American Journal of Sociology*, May, 1952.

[31] E. Graue, "Inventions and Production," *Review of Economics and Statistics*, 1943.

[32] "The Economic Future of Research and Development," *op. cit.*

could be achieved. The most theoretical in modern times is in many cases the most practical. It is this technique of scientific blueprinting by means of involved chemical and mathematical formulas which has made the industrial research laboratory the creator of new processes and new products. . . . Industrial research laboratories today aren't places in which so-called contriving geniuses work. They are, rather, clusters of workers completely familiar with the most advanced scientific techniques of analysis who cooperatively explore the terrain which their theoretical compilations have shown to be most likely to produce results. . . . Invention, in short, is a cooperative product.[33]

And Galbraith writes:

> There is no more pleasant fiction than that technical change is a product of the matchless ingenuity of the small man forced by competition to employ his wits to better his neighbor. Unhappily it is a fiction. Technical development has long since become the preserve of the scientist and the engineer. Most of the cheap and simple inventions have, to put it bluntly, been made.[34]

To what extent is this picture valid? What actually is the relative importance of the scientist-engineer teams of the large industrial research laboratories to technological change? Schmookler has found that, though the academically trained professional inventor is playing an increasingly important and increasingly advertised role, "the prevailing view has magnified an important characteristic of modern invention into a universal one, and in so doing a serious distortion of reality has occurred."[35] The Yankee inventor is scarcely extinct.

Schmookler examined a representative sample of recent patents. He found (1) that, of all patents issued, about 60 per cent were assigned to firms, about 1 per cent to the government, and about 39 per cent to private individuals; (2) that about 60 per cent of the patentees in the sample were full-time or part-time hired inventors, about 40 per cent being employed full time in research laboratories (but note that 60 per cent of the patentees were *not* full-time hired inventors); (3) that about 60 per cent of the inventors were technologists—defined as engineers (save for civil engineers), chemists, assayers, and metallurgists—and that about 40 per cent were not; and (4) that about 50 per cent of the inventors were college graduates and that about 50 per cent were not.

To determine the trend of the distribution of inventive activity, Schmookler ran a correlation between the number of patents issued to individuals in a state and the number of technologists in the state. For 1909 the correlation coefficient was .08. For subsequent decades the figure rises monotonically but at a decreasing rate. In 1950 it was .83. Schmookler summarizes his results as follows:

> Invention changed [during the first half of this century] from an activity overwhelmingly dominated by independent individuals to one less overwhelmingly dominated by business enterprise. The dominance of the latter, qualitative considerations aside, is not as great as is commonly assumed and amounts at the outside to no more than three-fourths of the total, measured in terms of either inventions or inventors. The shift from the independent to the management directed captive inventor has been accompanied by a corresponding shift in the occupational characteristics of the inventor group. Whereas earlier invention stemmed from individuals in many walks of life today a little more than half comes from individuals in the technological professions. . . . While invention has become more concentrated in a few occupations it remains primarily a part-time activity. Somewhat less than half of all inventions are

[33] *Hearings before the Temporary National Economic Committee*, Part 30, "Technology and Concentration of Economic Power" (Washington, D.C.: Government Printing Office, 1940), p. 16212.

[34] J. K. Galbraith, *American Capitalism: The Concept of Countervailing Power* (New York: Riverside Press, 1952), p. 91.

[35] Jacob Schmookler, "Inventors Past and Present," *Review of Economics and Statistics*, August, 1957.

made by full-time inventors, the rest being contributed by executives, line technologists, and individuals in a wide variety of other occupations working in their spare time. Mainly because part-time and independent inventors continue to operate in large numbers about half of inventions patented are still being made by individuals who lack college educations. Moreover, the process of transfer of the inventive function from the independent to the hired inventor seems to be slowing down.[36]

Jewkes, Sawers, and Stillerman, in studying fifty significant twentieth-century inventions, find that better than half were made by private individuals, in the sense that the inventors were not doing company-directed research.[37] A large share of the inventions were made by persons without great experience in the industries which their inventions most affected. And many of the inventions coming from company laboratories did not result from a systematic attack on the problem solved by the invention but came rather from work pointed in a quite different direction. The jet engine was invented and carried through to the early stages of development in both England and Germany by individuals not connected with established engine firms. Many of the early breakthroughs in radio were made by individuals not connected with firms in the communications industry. The early work on both catalytic cracking of petroleum and magnetic recording was done by independent inventors. Kodachrome was invented by two musicians. The originator of tungsten-carbide tools seems to have been a research worker in the electric-lamp industry. Dacron was first produced in the laboratories of a firm with no interest in fiber production.

[36] *Ibid.*, p. 329.

[37] *Op. cit.*

B. UNCERTAINTY AND PLANNING AND PREDICTION

Thus it is not true that the research and development laboratory now completely dominates the field of invention. The reason why it does not is that invention still is a most uncertain business, chance still plays a crucial role, and often the clever and lucky individual inventor rather than the better-equipped research team wins fortune's smile.

Scientists and research administrators with long experience in research and development are in relatively general agreement on the following points:

1. There is considerable uncertainty as to the outcome of a research and development program, the uncertainty being, of course, closely related to the degree of knowledge in the relevant fields and to the advance sought in the program. Attempts to develop an object that represents a marked advance in the state of the art are subject to great uncertainty; attempts to improve an existing object are subject to relatively little uncertainty.

> The picture presented of the orderly process of research is badly warped [D. H. Killeffer, chemist-research administrator].[38]

> Inventions do not spring up perfect and ready for use. Their conception is never virginal and must be many times repeated. One seldom knows who the real father is. The period of gestation is long with many false pains and strange forebirths. The number of abnormal creatures which see the light of day is ghastly. Few of the children of the mind ever survive and those only after many operations and much plastic surgery [F. R. Bichowsky, widely experienced engineer and research director].[39]

> When you come to research and development you can't answer any of the questions on

[38] D. H. Killeffer, *The Genius of Industrial Research* (New York: Reinhold Publishing Co., 1948), p. 1.

[39] F. R. Bichowsky, *Industrial Research* (Brooklyn, N.Y.: Chemical Publishing Co., 1942), p. 162.

the forecast. You don't know when you are going to get the thing, whether it is going to work or not, and whether it is going to have any value whatever [Charles Kettering, former director of research for General Motors].[40]

2. What looks good on paper may not look so good in practice. Therefore it is desirable to confront hypotheses with experiments and suggested designs with test models as quickly as possible. Only in this way will the actual problems of a development program be rapidly discovered and suggested solutions tried.

It is because of the great complexity of things in general and the imperfection of knowledge in every field that it is not possible, without experimentation, to predict with precision the validity of any theory or the outcome of any experiment, however simple [T. A. Boyd, scientist and research director at General Motors].[41]

To Whitney [first director of General Electric Laboratories] the experiment was the thing. . . . Long experience has convinced him that in research it is the unforeseeable, or at least the unforeseen, that is forever cropping up. Theorize all you wish, but experiment.[42]

The way to find out about a thing, to solve a problem, or to improve upon something is to begin experimenting with it [Charles Kettering].[43]

Try it out and see is one of the first rules of successful research [T. A. Boyd].[44]

3. For these reasons the attempt to plan a development program in detail will lead to frustration and failure if the program represents a significant leap forward. Usually, not just sometimes, unexpected obstacles are discovered, and many expected ones prove relatively easy to solve. Since the solution deemed most promising at the start of the program often turns out to be a poor one, parallel efforts are often desirable. Solutions to practical problems often come from unexpected sources.

It is logical, and often extremely profitable, to organize research laboratories to solve specific problems. Efficiency requires that the director shall assign each worker a carefully planned program. . . . This method, however, has serious limitations. Directors are rare who can foresee the solutions sufficiently well to plan out a good campaign of attack in advance. [The researcher given a relatively light reign] may solve the specific problem in still better ways than could have been achieved by direct attack [Irving Langmuir, General Electric scientist].

One kind of duplication is highly advantageous. That is when the objective is important and the solution difficult, involving a high degree of inventiveness or scientific imagination. In such cases parallel endeavor by different groups may be the only reasonable hope for an early and superior solution. [Karl T. Compton, former president of Massachusetts Institute of Technology].

In the early stages of research and development on a problem, parallel programs are essential not only to be sure that all avenues of attack are covered but because the parallel approach affords the necessary stimulus of competition. Parallel effort is often highly economical in the long run sense [Vannevar Bush, scientist, director of OSRD during World War II].

A case in point is the electronics industry's effort to find a way to make a cheap and reliable cathode ray tube for receiving color television. Present tubes cost ten times their black and white equivalent and it will probably require an entirely new concept or design of manufacture to reduce this ratio substantially. Chances are that when the way is found it will be so simple and obvious that every television repairman in the country will wonder why he didn't think of it first. But then, that has been true of every great invention—after it has been invented [E. L. Van Deusen, magazine writer on technology].[45]

Crayons that mark children's coloring books, but not your walls, facial tissues that replace a

[40] Charles Kettering in *Hearings before the TNEC*, Part 30, p. 16299.

[41] T. A. Boyd, *Research, the Pathfinder of Science and Industry* (New York: D. Appleton-Century Co., 1935), p. 171.

[42] L. A. Hawkins, *Adventure into the Unknown* (New York: William Morrow & Co., 1950), p. 15.

[43] Boyd, *op. cit.*, p. 85.

[44] *Ibid.*, p. 167.

[45] *Fortune*, November 13, 1957, p. 202.

bottle of perfume in a purse, paper with a built-in ink supply. These items may sound far-fetched. But they exist right now in the laboratories of the National Cash Register Company of Dayton, Ohio.... Company researchers stumbled on these products while trying to find a means of speeding up the printing process and hence the output of NCR's latest processing equipment. But as Robert Chollar, vice president in charge of research, says, "The by-products often are more valuable than the goal you originally set out for."[46]

The findings of a group at the RAND Corporation studying military research and development projects confirm the above conclusions.[47] Cost estimates of airframes, engines, and electronics equipment, based on paper design studies, have tended to be wrong, often by a factor of five or more. Performance estimates have also been extremely unreliable. Information derived from prototype tests, though by no means foolproof evidence, has enabled a significant improvement in the reliability of cost and performance estimates. The more successful development efforts have been marked by the early exploration and test of several alternatives.

From the preceding it does not appear that significant inventions occur in an orderly way, even in the modern industrial research laboratory. Booz, Allen and Hamilton, a management consulting firm, recently inquired of one hundred and twenty large companies doing substantial research and development work the percentage of projects which never produce a commercially used product or process. Sixty per cent was the median failure rate; 50 per cent was the minimum failure rate.[48] And uncertainty is great even when a project has progressed to a point where investment in plant and equipment is called for. Carter and Williams, studying a number of British companies, found that, though there usually was an attempt to estimate project return, the correlation between actual return and estimated return was extremely low—.13.[49] Of course the ability to predict results is much greater for product or process improvement projects than for projects seeking the development of a significantly new product or process, and the failure rate is correspondingly smaller. But so is the expected profitability, if the project results in a success.

Invention, at least major invention, is not just a rationally planned, orderly process. In Conant's terms, the Kreps quotation above definitely underestimates the number of fields in which the degree of empiricism is still high. A historian of science, I. B. Cohen, says:

> We cannot think, for example, that the expenditure of 50 million a year for so-called cancer research is a guarantee that the cure for the disease will be found in, say, five years.... Science consists to a large degree in an attack upon the unknown; how can a man foretell whether his work will eventuate in something that can be applied or not before he knows what he is going to discover? Who knows what branch of apparently useless scientific research will provide the key to unlock the cancer problem.[50]

Where primary inventions resulted from scientific breakthroughs, desire for the invention was seldom the motive for the scientific research that led to the invention. Applied research—research directed toward acquiring knowledge to help solve a particular practical problem —is relatively unlikely, save by accident, to result in significantly new break-

[46] *Wall Street Journal*, November 13, 1957.

[47] B. H. Klein, "A Radical Proposal for R. and D.," *Fortune*, May, 1958; B. H. Klein and W. H. Meckling, "Application of Operations Research to Development Decisions," *Operations Research*, May-June, 1958.

[48] *Chemical and Engineering News*, July 10, 1957, p. 38.

[49] C. F. Carter and B. R. Williams, *Investment in Innovation* (London: Oxford University Press, 1958).

[50] I. Bernard Cohen, *Science, Servant of Man* (Boston: Little, Brown & Co., 1948), p. 7.

throughs in scientific knowledge. If significant new breakthroughs are needed before a practical problem can be solved, applied research on the problem is not likely to be undertaken or invention attempted, since the expected cost of achieving such breakthroughs is likely to be extremely high. It is basic research, not applied research, that is likely to result in significant advances in knowledge.[51]

It is greatly to be doubted that X-ray analysis would have been discovered by any group of scientists who, at the turn of the century, decided to examine the inner organs of the body or the inner structure of metal castings. Radio communication was impossible prior to the work of Maxwell and Hertz. But Maxwell's work was motivated by his desire to explain and elaborate the work of Faraday, and Hertz built his equipment to test empirically some implications of Maxwell's equations (he had little faith that his study would result in something practical). But Marconi's practical equipment was a simple adaptation of Hertz's equipment, and it seems most unlikely that inventors attempting to develop a better means of long-range communication could have developed radio without a model like the equipment of Hertz.

Thus the evidence is that, though the expected profitability of an invention in a particular field affects the rate of inventive activity in that field, the tremendous uncertainties involved in making any major technological breakthrough preclude either the routinization of invention or the precise prediction of invention. Conditions of demand and of scientific knowledge provide us with guides for prediction and analysis, but only with rough guides.

[51] Richard R. Nelson, "The Simple Economics of Basic Scientific Research," *Journal of Political Economy*, April, 1959.

C. SELECTED CASE STUDIES

There are applied research projects that result quickly in answers to the pertinent questions. Some development programs, even programs attempting a significant advance, proceed smoothly and on schedule with few surprises and no basic changes in design. But the course of many projects resembles a labyrinth rather than a direct path from start to finish. The following case studies of research and development projects illustrate this. The first few of the studies lie toward the science end of the spectrum; the last few, toward the engineering end. For many more case studies the reader is referred to Jewkes and to Kaempffert.

1. *Hybrid corn.*—The practical value of research directed toward acquiring knowledge rather than achieving "practical" objectives is too well known to be belabored. However, the history of the development of hybrid corn is a relatively unknown but quite interesting case in point.

During the latter half of the nineteenth century several attempts were made to improve the yield of corn but with little success. Considerable progress was made during the first decades of the twentieth century, but "the curious fact is that this important development did not come directly from the work of any of the people whose primary concern was to increase the annual corn crop. Had the investigators who successfully solved the problem of corn improvement been interested simply in getting a better corn crop and confined themselves to this limited objective the practice of corn breeding would probably be no farther advanced than it was fifty or even one hundred years ago."[52]

[52] Cohen, *op. cit.*, p. 181.

Many researchers had directed their attention, at one time or another, to the inbreeding of corn to obtain a predictable and profitable strain. But, as corn plants were inbred, though they tended to breed true, they also tended to deteriorate in yield and quality. For this reason the applied researchers attempting to improve corn dropped this seemingly unpromising approach. But George Harrison Shull, a geneticist working with corn plants and interested in pure breeds not for their economic value but for experiments in genetics, produced corn breeds that bred true and then crossed these strains. The hybrid offspring of the sickly purebreds were vigorous and predictable. In short, an ideal economic corn strain resulted from a project motivated purely to advance science.

2. *Short-wave radio.*—The attempt to plan the development of a new product or the future course of an industry on the basis of existing scientific knowledge is dangerous business. Existing theory may point in exactly the wrong direction.

In the early days of radio practical radiomen worried little about the theory of radio transmission. But scientists raised the question of how, if radio beams follow straight paths, it was possible to beam a signal more than a few miles, much less across the Atlantic, considering the fact that, between two points separated by a hundred and fifty miles, the earth's curvature raises a hump of about three-quarters of a mile. The answer suggested by the English physicist Lord Rayleigh was that radio waves, like other waves, diffract and hence bend around barriers. A logical deduction from this quite plausible theory was that long wave lengths were most suitable for long distances and that short waves were useful only for very short distances. Thus, in the years following World War I, the longer waves, communication over more than very short distances, were reserved by law for commercial use, the short waves being left to amateurs.

But, as the quality of the radio equipment improved, the distance performance of amateur stations working with short waves soon outstripped the distance performance of commercial stations working with the theoretically superior long waves. These empirical facts led scientists to seek an explanation, other than diffraction for the ability of radio waves to be sent long distances around the hump of the earth. The currently accepted explanation is the presence of a region of the earth's upper atmosphere that reflects short radio waves back to earth but does not satisfactorily reflect long waves.

3. *Nylon.*—Another example of the way that new products come from new scientific knowledge is the research project at Du Pont that led to the development of nylon. Carothers' work in linear superpolymers began as an unrestricted foray into the unknown, with no particular practical objective in mind. But the research was in a new field of chemistry, and Du Pont believed that any new chemical breakthrough would likely be of value to the company. The very lack of a specific objective, the flexibility of the research project, was an important factor contributing to its success. In the course of research Carothers obtained some superpolymers that became viscous solids at high temperatures, and the observation was made that filaments could be obtained from these materials if a rod were dipped in the molten polymer and then withdrawn. At this discovery the focus of the project shifted to these filaments. Nylon was the result, but at the start of the project Carothers had no idea, nor could he possibly have known

in advance, that his research would lead him to the development of a new fiber.

4. *The development of the atomic bomb.* —The development of the atomic bomb during World War II absorbed a greater quantity of resources than any prior organized development effort and is probably the most publicized development effort in history. By 1941 physicists had verified the fact that the process of fission released enormous quantities of energy. Theoretical calculation indicated that certain materials were capable of a self-sustaining chain reaction, although in 1941 no chain reaction had actually been produced by man. The attack on Pearl Harbor quickly resulted in the decision to direct a large share of our scientific resources to an attempt to develop an atomic bomb.

Of the many problems that had to be solved before a working bomb could be produced, one of the most formidable was the development of a method of producing large quantities of material capable of sustaining a chain-fission reaction. It was believed that either U^{235}, which would have to be separated in some way from the much more abundant isotope U^{238}, or Pu^{239}, which would have to be generated by an untested pile chain reaction and then separated (probably chemically) from other reaction elements, would be a satisfactory material for a bomb. The effort directed toward developing a method for producing bomb material proceeded hand in hand with the effort to design and develop the bomb itself, to develop satisfactory methods for obtaining pure uranium from ore, etc., but we shall be concerned only with the U^{235}-Pu^{239} program.

Several alternative methods looked good on paper both for the separation of U^{235} from U^{238} and for chemically separating Pu^{239} from other pile elements. Since there existed no consensus among scientists as to which of several alternative methods was the most promising, it was decided to go ahead with research on several methods. Conant reported to Bush on May 14, 1942:

> All five methods will be entering very expensive pilot plant development during the next six months. . . . [But] while all five methods now appear to be about equally promising, clearly the time to production . . . by the five routes will certainly not be the same but might vary by six months or a year because of unforeseen delays. Therefore, if one discards one or two or three of these methods now, one may be betting on the slower horse unconsciously.

Slightly later Bush and Conant reported: "It would be unsafe at this time, in view of the pioneering nature of the entire effort, to concentrate on only one means of obtaining the result."

The wisdom of this policy decision is borne out by the fact that the first method to produce appreciable quantities of material, the electric magnetic method of separating U^{235} from U^{238}—the method which produced the material which went into the first bombs—was a method which had been considered relatively unpromising early in the program. And the method considered early in the program to be most promising—the gaseous diffusion method—did not produce appreciable quantities of U^{235} until after the war was over, although it did turn out later to be the best method.

5. *Development of aircraft engines and fuels.*—The development of military aircraft engines and fuels prior to World War II vividly illustrates the great uncertainties involved in technological advance, the importance of testing, and the advantages of flexibility in research and development.

> Development of engines and fuels has not been what the layman would call a scientific process; it has rather been a process of cut and

try. In the case of the engine the process has been one of trying things, with the good ones accepted and the poor ones discarded. In the case of fuels, despite the brilliant work of organic chemists in synthesizing hydrocarbons which were theoretically possible but which hitherto had been unknown, the process has been one of finding out what happens with very little increase in knowledge of why things happen. . . . The importance of imagination in a development such as that of fuels and engines is often underestimated. . . . A planned program would have taken almost supernatural intuition in regard to some phases of the development.[53]

Since it was impossible to foresee before the program started what the actual problems would be, hence which alternative design would prove the best basic design, Schlaifer found that the best way to get a good engine quickly was to bring to a test as soon as possible a number of alternative designs, pick the one that combined good characteristics with few development problems, and then work intensively to develop it. "Although the complete development of any engine, whether of a completely new or a well-known type is necessarily an extremely expensive process, only a relatively small amount of preliminary development, or better experimentation, is needed to show whether an innovation . . . has promise or not."[54]

Many development problems that looked easy on paper proved very difficult or impossible to solve in practice, and many problems that looked difficult on paper proved relatively easy to solve. Therefore, the attempt to specify engine characteristics with rigidity on the basis of expected difficulty of solving various problems often led to trouble.

The government had the power to control . . . the work done by private firms in all its details, but the development was fully successful only when the services gave the firms all possible freedom in deciding on details of design and development. Government intervention in technical details always led to very considerable delay, and often to a poorer product in the end.

. . . The Army's insistence on maintaining its original specifications for coolant temperature . . . would alone have sufficed to put the entire development behind its competitors . . . [for] any aircraft engine contains a host of compromises, since a gain in one direction may very well be outweighed by a loss in another.[55]

Although the cost of finding out whether a basic design is promising or not was usually relatively low, the job of developing a basic engine to operating requirements was usually a very costly and time-consuming business. The reason why a number of development programs failed was that the firms "failed to realize the extent of the development which would have to be done on an engine before it could be gotten ready for production."[56]

In the aircraft-engine industry the costliness of developing a new engine acted to slow down the rate of advance, for, "although progressive refinement of proved types of engines comes from established producers, radical innovations in design are extremely unlikely to come from that source."[57] Though new firms were the source of major innovations, they seldom had the funds or the experience to develop their designs successfully. The history of the turbojet engine is a case in point.

The only radical innovation during the period in aircraft engines, the turbojet, was backed in the early stages neither by the government nor by an established engine producer. This was so in England and Germany as well as in the United States. The first successful turbojet design was that of RAF Flying Officer

[53] Schlaifer and Heron, *op. cit*., p. 559.

[54] *Ibid.*, p. 90.

[55] *Ibid.*, pp. 7 and 22.

[56] *Ibid.*, pp. 8 and 78.

[57] *Ibid.*, p. 98.

Frank Whittle. He was unable to interest the government in a program to test his engine design and eventually obtained financial backing through a rather irregular arrangement with a small English investment banking firm. The total cost of the work at Power Jets (the company set up to conduct work on Whittle's design) from its beginning in 1936 to the middle of 1939, when it was definitely shown that the turbojet was not a dream but a practical engine, was about $100,000.

However, the cost of bringing the engine from this stage to a stage suitable for practical work was many times this amount, and the job was not completed until after the war. Although Whittle's design was basically sound and Power Jets' decision to bring the design quickly to a test stage helped to point the way for development effort, Power Jets' inexperience in the engine field and lack of funds resulted in a very slow development. Accordingly, in 1941, the British government took over control of the development of Whittle's engine.

It is interesting to note, incidentally, that Whittle's only suggestion as to an immediate practical use for his engine was for long-range mail planes. It is extremely difficult to forecast the uses for an invention before it is actually invented.

IV. ADMINISTRATION AND CONTROL OF INDUSTRIAL RESEARCH AND DEVELOPMENT LABORATORIES

Although the industrial research laboratory has by no means pre-empted invention—the individual inventor is still the main source of ingenious gadgets, and many of the most significant inventions can be so described—the industrial research and development laboratory is playing an increasingly important role, particularly in areas where theoretical scientific knowledge is important and where costly equipment and materials are needed. Therefore, in order better to understand the factors influencing the rate and direction of inventive activity, it is useful to examine the industrial research laboratory, its organization, and the management techniques used to control work. The discussion will be descriptive rather than normative.[58]

A. THE ORGANIZATION OF RESEARCH AND DEVELOPMENT LABORATORIES

Research laboratories may be created and maintained by firms for many purposes, including development and application of quality control and other testing techniques, elimination of manufacturing troubles and improvement of manufacturing methods, improvement of existing products and development of new uses for them, development of new products and processes, and scientific research to acquire knowledge enabling more effective work to be done to achieve the above purposes. Any given laboratory may serve any or all of these purposes, but the vast majority of industrial laboratories emphasize the more practical and immediate, and only a few do much in the way of general scientific research. It is important to realize that the line between improving existing products and developing new ones is very hard to draw; most development efforts represent only small advances in the state of the arts.

The position of a laboratory within a company usually depends on the origins of the laboratory, who established it, and for what purpose. Often, as a laboratory grows, the "menu" of work changes. A laboratory established for quality-con-

[58] For an excellent bibliographical introduction to the normative literature see A. Rubenstein, "Looking Around, Guides to R and D," *Harvard Business Review*, May–June, 1957.

trol purposes, and hence positioned under the production division, may also undertake development work; laboratories established for research and development may spend a considerable share of their resources on short-term trouble-shooting. Many large companies have a number of laboratories, some attached to the operating divisions and undertaking quality control, trouble-shooting, and product improvement, and one or more established as separate divisions. The independent laboratories usually work on problems affecting several divisions and on those problems where the results of research cannot be preassigned to any particular operating division, particularly those that may result in new products.

For example, at Du Pont each operating department has its own research division. In addition, Du Pont has a central chemical department, with laboratories for general, long-range research. The director of this department has the same status as the general managers of the operating divisions and reports directly to the president and the executive committee.[59] And at Eastman Kodak the Kodak Research Laboratories are independent of any manufacturing division. But the research and development work of the company has never been confined to the Kodak Research Laboratories, about three-fourths of the work being carried on by the laboratories of the manufacturing divisions. The central laboratories emphasize longer-range programs and more basic research than do the laboratories of the operating divisions.[60]

The director of the research laboratory is usually linked to top management, and top management to over-all research and development decision-making, by a chain of committees. The organization of one medium-size electronics company is perhaps an extreme example of this concatenation of committees. Over-all company policy is set by a management committee, which consists of the company president and treasurer, the heads of manufacturing and sales, and the chief engineer (who acts as research director). The chief eingineer is also a member of the new-products committee, which has formal decision control over new products to be developed, and is chairman of the development committee, which is responsible for directing the research staff.

Under the research director the medium- and large-size laboratories are usually divided into a number of departments. Most of the early established industrial laboratories were structured on the model of university departments: physics, organic chemistry, electrical engineering, etc. But, as experience made it increasingly clear that research cannot be closely planned and scheduled, that the types of skills needed cannot always be forecast, and that often men from several disciplines are needed many companies have displayed the "academic department" system by an organization based on areas of interest. Each department may include men trained in different scientific areas but applying their knowledge so that their work converges on one broad subject area of interest. Thus the department for research on the chemistry of high polymers at Kodak Research Laboratories includes organic and physical chemists, physicists, and engineers. In laboratories so organized, in theory the fields of work, hence the departments, are defined so as to cover most effectively the fields of interest to the company. In practice the organization, like Topsy, usually just grew.

[59] C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, 1946), p. 124.

[60] *Ibid.*, p. 146.

Practically all laboratories operate with a hybrid organization which includes features of various systems of work allocation. Thus Kodak Research Laboratories has departments of both black-and-white photography and physics, both color photography and organic chemistry. And it is becoming increasingly common to deviate from formal organization so as to have a structure that changes as the menu of laboratory projects changes. Many laboratories are reorganizing formally along these patterns because experience has shown that such a pattern actually develops informally. Mees depicts the actual structure of most laboratories in a diagram that looks like a spider web or sociograph, with influential men at the important nodes, lesser men at lesser nodes, and much cross and circular linkage, with many men working on several projects at once.

The nature of the work done by the laboratory, of course, influences the type and complexity of the formal and informal structure. A laboratory directed mainly toward scientific research is usually operated by a system permitting considerable freedom of the research workers. Projects are usually handled by single individuals or small groups. Since goals cannot be closely specified in advance and since the success of one man's work is relatively independent of the work of others, organization can be loose. Development projects, on the other hand, usually involve a number of relatively closely defined problems, all of which must be solved before the project as a whole can succeed. The solutions to the problems are often interdependent. Hence the project must be organized to assure coverage of all important problems and co-ordination of the operating groups.

B. THE DECISION-MAKING PROCESS

Given the formal laboratory organization, how is research planned?

> At the outset it is well to distinguish between planning for research and planning of research. It is well accepted that consistently productive research must be planned for in the sense that competent men must be assembled, facilities provided, and equipment installed for their use. . . . [But] planning may describe the over-all outline on large-scale programs at high administrative levels. Again, it may refer to exceedingly detailed procedures and operations at the opposite end of administration.[61]

In recent years a thriving literature has developed on how to plan and control research and development.[62] The basic problems, as most of the writers see them, are to allocate research money to those projects that yield the greatest payoff and to run each project as efficiently as possible. Certainly, none would quibble with these aims. Many writers believe that there should be a strong set of formal control practices involving periodic evaluation of operating projects. Many other scientists and research administrators disagree. James Conant argues:

> There is only one proved method of assisting the advancement of pure science—that of picking men of genius, backing them heavily, and leaving them to direct themselves. There is only one proved method of getting results in applied science—picking men of genius, backing them heavily, and keeping their aim on the target.[63]

As we shall see, in most laboratories, though there may exist a rather formidable formal control structure, in practice most of the operating decisions are made quite informally, and there are few strictly enforced control rules.

A major objective of research and de-

[61] J. R. Steelman, *Science and Public Policy* (Washington, D.C.: Government Printing Office, 1947), III, 28.

[62] See Rubenstein, *op. cit.*

[63] Cohen, *op. cit.*, p. 307.

velopment policy is profit. But the tremendous uncertainties involved in predicting cost and return seem to have precluded the systematic use of formal decision rules. This problem is typified by the budget decision. What is the optimum budget size? No one seems to know. Thus formal rules for making the budget decision are seldom used. Typically, an over-all laboratory budget is presented annually by the laboratory director for approval by the top-management committee, but quite often, before the formal budget is prepared and submitted, the committee, of which the research director is almost certainly a member, has informally agreed to a broadly defined laboratory research program and an acceptable budget magnitude.

What determines the size of the total budget approved by management? About 25 per cent of companies responding to a 1948 NAM inquiry said that they attempt to keep their research budget a relatively constant percentage of the value of total sales. A figure of 1.5 per cent was often given. Twelve per cent of the companies replying stated that the size of the research budget was regulated by the companies' over-all profit rate. But the vast majority of companies stated that a great many factors influenced the size of the research budget: sales and profits, general business conditions, research policies of competing firms, etc., and, of course, the menu of suggested laboratory projects.[64] The National Science Foundation undertook a series of interviews with top research officials of two hundred companies engaged in research and development. Despite experimentation by a number of these companies with various ratios (such as the research and development budget to total sales), few companies reported that they relied heavily on such formulas. In many cases the major factor determining the research and development budget was a government contract. Virtually all companies reported that competition in large part determines the magnitude and direction of the research and development effort. An increase in expenditure for product improvement by one company certainly will tend to stimulate an increase in expenditure by a company producing a competing product.[65]

"An inquiry into the control over expenditure on research and development revealed that the general practice is for management to approve an annual budget and then leave to the research management detailed allocation of the sums approved."[66] Over 50 per cent of the respondents to a Harvard Business School questionnaire stated that all funds were approved as a single lump sum. Seventy-five per cent stated that over 50 per cent of their funds were received in this form, the rest of their funds being allocated to specific projects. Sometimes operating divisions make contributions which may or may not be specified as to use.[67] Although top management theoretically exerts control through the over-all size of the budget, management in fact often shows great reluctance to tamper with the laboratory budget. Indeed, in many organizations the laboratory is permitted to exceed the size of the programed budget by a certain percentage, sometimes 10 per cent, without going to top management.

Typically, then, because no good way

[64] Mees, *op. cit.*, p. 165.

[65] National Science Foundation, *op. cit.*, pp. 41–42.

[66] Mees, *op. cit.*, p. 163.

[67] R. N. Anthony, *Management Controls in Industrial Research Organizations* (Cambridge, Mass.: Harvard University Press, 1952).

has been discovered for establishing good criteria for the budget decisions, top management exerts formal control over the research laboratory neither by tightly controlling expenditure on specific projects nor by maintaining tight rein on the size of the over-all budget. Though formal control procedures exist, actual control is usually informal.

> The directors of a number of well-known laboratories stated unanimously that their company management did not dictate the choice of R and D programs or projects. . . . All of the laboratories to which the above remarks apply had established their position within their companies by satisfactory performance over a period of years, and it may be argued that their semi-autonomy was due only to successful operation. Some of the directors expressed the definite opinion that should the laboratory fail in the eyes of the company management, attempts would probably be made by management to exercise more definite control over its work.[68]

Within the constraint that the program of projects yield a return that management deems satisfactory at a cost deemed reasonable, what determines the selection of a laboratory program? Where and how do project proposals arise? How are decisions made about which project proposals to implement?

In May, 1952, a conference of research administrators was held at the University of California at Los Angeles to discuss a number of questions, among them, "Where and how are project proposals typically made?"[69] The respondents stated that proposals originate from numerous sources, often from consumers of the products of the company either by informal suggestion or by formal request (sometimes a contract for research and development work), and sometimes from the sales division or the manufacturing division, commonly as a by-product of work that is already going on in the laboratory. Often new developments in science (outside the laboratory) or new products created by other firms stimulate laboratory workers to suggest a project.

> Objectives usually come from the top but when you say, here's a good idea, let's research the hell out of it, it usually comes from the bottom. . . .
>
> . . . Apart from those ideas which originate outside the [laboratory] organization proposals are outgrowths of actual work in the laboratory. Laboratory teams of research chemists, physicists and technicians may begin with a suggestion based on a need recognized outside of the [laboratory] organization and attempt to do the analytical work necessary to yield specific ideas that might be tried for solution. In the course of carrying out this work a great many by-products, ideas, occur which in turn lead to suggestions for new projects; actually therefore these ideas have their origin strictly in the laboratory.
>
> A distinction must be drawn between research and development problems. In almost all cases research problems originate with individual scientists. . . . Usually the early development work on new products or processes arises from and is a continuation of the fundamental or applied research work of an individual or group of individuals, and the originator of the development work is the individual or group who has done the research work.[70]

From the many alternatives, how does the laboratory select the projects it will undertake? The general direction of fundamental work is usually marked out by the research director and his staff in consultation with the senior operating research workers. Laboratory policy usually involves the attempt to achieve a balance between fundamental long-range payoff research, new-product development and allied research, and work on

[68] Mees, *op. cit.*, p. 218.

[69] I. R. Weschler and P. Brown (eds.), *Evaluating Research and Development* (Los Angeles: Human Relations Study Group, Institute of Industrial Relations, UCLA, 1953), p. 20.

[70] Mees, *op. cit.*, pp. 233 and 234.

problems presented by the operating divisions. Mees has found that in a number of large research laboratories "about ten to twenty per cent of the scientific work is directed to fundamental research, between forty and sixty per cent to the development of new products and processes, and between thirty and forty per cent to work on existing products and processes."[71] Of course, the ratios aimed at differ markedly from laboratory to laboratory.

The answers to the National Science Foundation interviews showed clearly that the time horizon of the company strongly affects the direction of the research and development effort. Several companies, which were concerned, as they put it, with "staying in business" or "holding their market," described their research programs as being primarily dictated by short-run problem-solving, including meeting stepped-up competition. But other companies with a lower time-discount factor viewed much of their research and development effort as long-term investment in new products. In determining their research and development program, these companies tended to give less emphasis to their current product menu than did companies with shorter time horizons. Two-thirds of the companies said that they were engaged in some "basic research."[72]

The production of new knowledge that may lead to the development of new products is by no means the only motive given by companies for including basic research in the menu of laboratory projects. The scientists working on basic research are expected to relay information from the world of new scientific knowledge, created outside as well as inside the confines of company facilities, to laboratory management. Many companies believe that top scientists can be hired and held only if they are permitted to spend a good share of their time on basic-research projects. Further, many companies believe that a company's prestige is increased and market position improved by a well-publicized basic-research program.

A large number of companies state that they have no specific formal plan for selecting projects. In these companies, according to Mees, the personal influence of the director is of great importance.[73] However, many laboratories have some formal project-selection plan involving the comparison of probability of success, expected costs of the project, expected capital requirements, production costs, analysis of the market demand for the product, consonance of the product with company production know-how and sales contacts, etc. It is clear that these calculations are subject to great uncertainty and have only limited value, even when the goals of the project are very well defined—developing a specific type of product for a specific use, solving a specific production problem. There seems to be wide agreement that the calculations are almost completely meaningless in areas of research where there is no specific practical goal in mind (e.g., Carothers' research on high polymers).

As shall be shown, though many companies use a formal project evaluation scheme, the results of this formal evaluation do not play a dominant role in actual research and development project decision-making.

At the Wyandotte Chemical Company all project proposals are submitted to the research director. In consultation with his staff, he makes a rough cut in the list of project proposals by eliminating those

[71] *Ibid.*, p. 230.

[72] National Science Foundation, *op. cit.*, p. 44.

[73] *Op. cit.*, p. 230.

projects that seem too difficult for the research staff, those that involve the development of products far removed from the company line, those that appear to involve excessive capital expenditures for production of the new product, etc. The remaining project proposals are evaluated on the basis of anticipated profit rate on the capital required in relation to the estimated cost of carrying the project through to completion. Projects are often reviewed.[74]

Approximately 90 per cent of the companies responding to a Harvard Business School questionnaire said that they used written project proposals for some classes of projects, 55 per cent said that they were used for all projects, and 10 per cent said that they never used written project proposals.[75] Here the definition of a "new project" is important. When is a project *new?* When is informal work formally a *project?* Written project proposals are usually preceded by a certain amount of informal, preliminary work. Project proposals require approval of persons or groups within laboratory organization before the project is formally put into the laboratory schedule, and sometimes they require approval by extra-laboratory company committees.

Many scientists and research and development administrators are much concerned with the growing tendency toward formalization of laboratory decision-making. They believe that such formalization will tend to stifle many desirable projects and that the intuitions of the research worker, which cannot be neatly written down in a formal project proposal, are the best guides to the potential worth of a project. Mees's comments are typical:

> The best person to decide what research work shall be done is the man who is doing the research. The next best is the head of the department. After that you leave the field of best persons and meet increasingly worse groups. The first of these is the research director who is probably wrong more than half of the time. The comes a committee, which is wrong most of the time. Finally there is a committee of company vice presidents which is wrong all of the time.[76]

But to what extent are choices for laboratory projects made formally by comparing competing written project proposals? Twenty-five per cent of laboratories reporting said that they accept all proposals, 90 per cent said that they accept over 75 per cent of written proposals, and only 3 per cent said that they accept less than half of the proposals submitted. Thus the written document is usually just a formal statement of ideas that have already been informally accepted.[77]

Once a project is undertaken, laboratory management usually keeps a formal running check. The scheduling of manhours, money expenditure, and progress by project is apparently becoming increasingly popular. However, these schedules seem to be taken seriously only for projects involving relatively little uncertainty; they are seldom if ever taken seriously for broadly defined research projects. The use of periodic reports is even more common than the use of schedules. About seventy-five of the laboratories responding to a Harvard Business School questionnaire said that regular checks were made on project costs, manpower, and estimated completion date.[78]

[74] T. H. Vaughn, in *Proceedings of the Fourth Annual Conference on the Administration of Research* (Ann Arbor: University of Michigan, 1951).

[75] Anthony, *op. cit.*, p. 112.

[76] *Op. cit.*, p. 135.

[77] Anthony, *op. cit.*, p. 122.

[78] *Ibid.*, p. 176.

Though management, through project review, theoretically must continuously make decisions as to whether projects should be continued or dropped, in actual practice "fundamental research programs are discontinued largely upon the decision of the individual scientists and seldom at the request of the director. . . . Actually, laboratory research directors do not very often decide to discontinue research and development problems. Most either die of their own accord or are successfully solved."[79]

Thus the decision-making processes of the research and development laboratory reflect the dual nature of the process of invention sketched in the earlier sections of this paper. Decisions are profit oriented. But, because of the great uncertainties involved, it is difficult to predict or to control the course of any project.

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