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THE EVALUATION OF INDUSTRIAL RESEARCH AND DEVELOPMENT PROJECTS

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Introduction

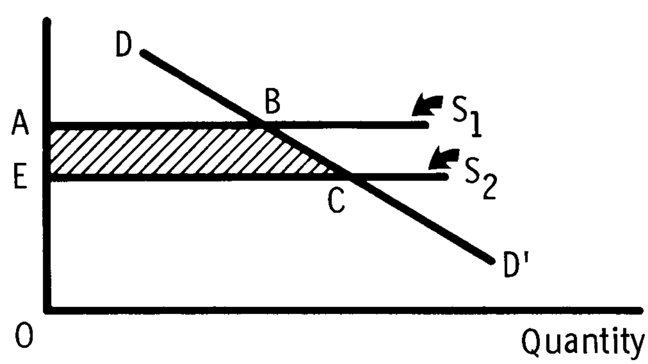
Considerable work has been carried out by economists, engineers, and others to formulate and study methods to evaluate industrial research and development R & D projects. In this paper, my purposes are (1) to describe the results of recent studies to measure (*ex post*) the social benefits from industrial innovations, (2) to indicate the sorts of *ex ante* evaluation techniques described in the literature and the extent to which they are used by American firms, (3) to provide measures of the biases and errors contained in *ex ante* estimates of development cost, development time, and the profitability of new processes and products, and (4) to indicate the effects on probabilities of success of how quickly *ex ante* economic evaluations are made.

Measurement of Social Benefits from Industrial Innovations

Any innovation, particularly a major one, has effects on many firms and industries, and it obviously is difficult to evaluate each one and sum them up properly. Nonetheless, economists have devised techniques that should provide at least rough estimates of the social rate of return from particular innovations, assuming that the innovations can be regarded as basically resource saving in nature.

To estimate the social benefits from an innovation, economists have used a model of the following sort. If the innovation results in a shift downward in the supply curve for a product (such as from S_1 and S_2 in Figure 1), they have used the area under the product's demand curve (DD') between the two supply curves--that is, ABCE in Figure 1--as a measure of the social benefit during the relevant time period from the innovation. If all other prices remain constant, this area equals the social value of the additional quantity of the product plus the social value of the resources saved as a consequence of the innovation. Thus, if one compares the stream of R & D (and other) inputs relating to the innovation with the stream of social benefits measured in this way, it is possible to estimate the social rate of return from the investment in the innovation.

Figure 1. Measurement of Social Benefits from Techological Innovation



Recently, a study was made by Mansfield, Rapoport, Romeo, Wagner, and Beardsley (in Mansfield, *et al*. 1977) of the returns from 17 specific industrial innovations. These innovations occurred in a variety of industries, including primary metals, machine tools, industrial controls, construction, drilling, paper, thread, heating equipment, electronics, chemicals, and household cleaners. They occurred in firms of quite different sizes. Most of them are of average or routine importance, not major

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breakthroughs. Although the sample cannot be regarded as randomly chosen, there is no obvious indication that it is biased toward very profitable innovations (socially or privately) or relatively unprofitable ones.

To obtain social rates of return from the investments in each of these innovations, my colleagues and I used a model somewhat like that described in Figure 1, except that we extended the analysis to include the pricing behavior of the innovator, the effects on displaced products, and the costs of uncommercialized R & D and of R & D done outside the innovating organization. The results indicate that the median social rate of return from the investment in these innovations was 56%, a very high figure. On the other hand, the median private rate of return was 25%.

Table 1. Typical Expenditure on an R & D Project before Studies of Market and Profit Potential, 16 Firms.

Expenditures ($000)	Number of Firms
Less than 10	7
10 - 24	3
25 - 49	1
50 - 99	2
100 - 149	1
150 - 199	1
200 and over	1
Total	16

In addition, my colleagues and I obtained very rich and detailed data concerning the returns from the innovative activities (from 1960 to 1972) of one of the nation's largest firms. For each year, this firm has made a careful inventory of the technological innovations arising from its R & D and related activities, and it has made detailed estimates of the effect of each of these innovations on its profit stream. We commputed the average rate or return from this firm's total investment in innovative activities during 1960-72, the result being 19%, which is not too different from the median private rate of return given in the previous paragraph. Also, we computed lower bounds for the social rate of return from the firm's investment, and found that they were about double its private rate of return, which also agrees with the results in the previous paragraph.

The foregoing results pertain to the average rate of return. In earlier investigations based on econometric estimation of production functions Mansfield (1968) and Minasian (1969) estimated the marginal rate of return from R & D in the chemical and petroleum industries. Mansfield's results indicated that the marginal rate of return was about 40% or more in the petroleum industry, and about 30% in the chemical industry if technical change was capital embodied (but much less if it was disembodied). Minasian's results indicated about a 50% marginal rate of return on investment in R & D in the chemical industry.

In a more recent study, Terleckyj (1974) has used econometric techniques to analyze the effects of R & D expenditures on productivity change in 33 manufacturing and nonmanufacturing industries during 1948-66. In manufacturing, the results seem to indicate about a 30% rate of return from an industry's R & D based only on the effects of an industry's R & D on its own productivity. In addition, his findings show a very substantial effect of an industry's R & D on productivity growth in other industries, resulting in a social rate of return greatly exceeding that of 30%. No evidence was found, however, demonstrating that government contract R & D has any effect on the productivity increase of the industries performing it.

Griliches (1975) has carried out an econometric study, based on data for almost 900 firms, to estimate the rate of return from R & D in manufacturing. His results pertain only to the private, not the social, rate of return. He finds that the private rate of return is about 17%. It is much higher than this in chemicals and petroleum, and much lower than this in aircraft and electrical equipment. He finds that the returns from R & D seem to be lower in industries where much R & D is federally financed.

Based on computations for the economy as a whole, Denison concluded that the rate of return from R & D was about the same as the rate of return from investment in capital goods. His estimate of the returns from R & D was lower than the estimates of other investigators, perhaps due to his assumptions regarding lags. In his presidential address to the American Economic Association Fellner estimated the average social rate of return from technological-progress activities, his conclusion being that it is "substantially in excess" of 13 or 18%, depending on the cost base, and that this is much higher than the marginal rate of return from physical investment at a more or less given level of knowledge.

To sum up, practically all of the studies carried out to date indicate that the average social rate of return from industrial R & D tends to be very high. Moreover, the marginal social rate of return also seems high, generally in the neighborhood of 30 - 50%. Of course, there is a variety of very important problems and limitations inherent in each of these studies. Certainly, they are very frail reeds on which to base policy conclusions. But recognizing this fact, it nonetheless is remarkable that so many independent studies based on so many types of data result in so consistent a set of conclusions.

Models for R and D Project Selection

Economists and operations researchers have devoted considerable attention to R & D project selection. A variety of models have been developed to help solve this problem. These models vary enormously in sophistication, some relying on the crudest sorts of ranking procedures, some employing fairly straightforward adaptations of capital budgeting techniques, some using linear programing, some using dynamic programing, and some using Bayesian decision theory. Among the best known of these techniques are PROFILE (Programmed Functional Indices for Laboratory Evaluation) and QUEST (Quantitative Utility Estimates for Science and Technology), both of which were developed for the U.S. Navy, and PATTERN (Planning Assistance Through Technical Evaluation of Relevance Numbers), developed by Honeywell. (See Cetron et al. 1969.)

For present purposes, it is sufficient to present a relatively simple programing model to illustrate the nature of such techniques. Suppose that a firm has a list of n possible R & D projects that it might carry out and that i^{th} project would cost C_i dollars to carry out. Moreover, the i^{th} project is estimated to have a probability of success of P_i, and if successful, it will result in a profit (gross of R and D costs) of π_i. Then, if the firm can spend no more than C dollars on R & D, its problems can be represented as follows:

Maximize $$\sum_{i=1}^{n} x_i(P_i\pi_i - C_i).$$

where $$\sum_{i=1}^{n} x_iC_i \leq C$$

and $$x_i = 0, 1.$$

In other words, the firm's problem is to choose the X_i--where $X_i = 1$ if the i^{th} project is accepted and 0 if it is rejected--in such a way that the expected value of profit is maximized, subject to the constraint that the total amount spent on R & D be no more than C. This, of course, is an integer programing problem. (See Freeman, 1960.)

Of course, this is a relatively simple model. It is possible to make this model more realistic by recognizing that the firm may be interested in parameters of the probability distribution of profit other than the expected value. It is possible to recognize that, in most cases, there is a variety of expenditure levels at which a project can be carried out. It is possible to recognize that the impact of one project may depend on the outcome of another project. If one is willing to cope with the complexities and data requirements that result, it is possible to extend this model in many directions. But for present purposes, this simple model is a suitable illustration.

The Application of Project Selection Models

It is difficult to measure with accuracy the extent to which project selection models of this sort are being used in the United States. Our own surveys indicate that a large proportion of the laboratories--particularly the larger laboratories--in the chemical, drug, and electronics industries are using some form of quantitative project selection technique. But it is difficult to tell how significant such techniques are in the decisionmaking process. In some laboratories, they are taken much more seriously than in others. Indeed, one suspects strongly that in some laboratories these techniques are little more than window dressing, the real determinants of project selection professional hunch, intrafirm politics, as well as a host of other factors being at work behind the facade.

However, one thing appears to be clear: the more sophisticated types of models are not being used very extensively. For example, Cetron and Ralph report that only 20% of the firms responding to their survey had tested or used linear programing models and that only about 10% had tested or used more complicated techniques like PROFILE, QUEST, or PATTERN. And for a variety of reasons, I suspect that these figures are overestimates for American industry as a whole. In the American government, there has been considerable attention devoted to such models, particularly in the Department of Defense. But it is difficult to tell with any certainty the extent to which these models have actually been applied.

There are a number of reasons why the more sophisticated models have not found extensive use. First, even the more sophisticated models are often oversimplified in important respects. For example, many models fail to recognize that R & D is a process of buying information, that unsuccessful projects can provide much valuable information, and that the problem is one of sequential decisionmaking under uncertainty. Thus, they fall into the sorts of traps that the RAND studies of military R & D describe so well. Second, application of the more sophisticated models is not cheap. For example, Jantsch has estimated that the cost of setting up a PATTERN model is about $250,000 and that the cost of maintaining the model is about $50,000 per year. Needless to say, many techniques do not cost nearly this much, but they are far from costless. Third, and perhaps most important, these models are based on estimates that are not very reliable, as we shall see in the following section.

Accuracy of Estimates of Development Cost and Time

Practically any project selection model requires estimates of the cost of carrying out a prospective R & D project, and the time that it will take. Unfortunately, these estimates tend to be quite inaccurate. In the military field, it is well known that there tend to be large overruns in R & D costs and lesser overruns in R & D time. For example, Peck and Scherer found that for a sample of 12 airplane and missle development projects, the average ratio of actual to estimated cost was 3.2, and the average ratio of actual to estimated time was 1.4. In civilian fields, there seems to be more optimism concerning the accuracy of these estimates, with a surprising number of R & D managers regarding such estimates as good or excellent. However, the available evidence indicates that these estimates are almost as bad for civilian as for military work when reasonably large technical advances are attempted.

Even when firms doing commercial work attempt relatively minor advances, these estimates tend to be considerably wide of the mark. For example, in a proprietary drug firm we studied, the average ratio of actual to estimated development cost was 2.1 and the average ratio of actual to estimated development time was 2.9. Moreover, the standard deviation of the cost ratio was 3.2, and the standard deviation of the time ratio was 1.6. Clearly, these estimates of development cost and time were quite inaccurate. Studies of the accuracy of estimates of the probability of technical success indicate that they too are not very trustworthy. For example, in the proprietary drug firm cited above, although the estimated probabilities of technical completion are of some use in predicting which projects will be completed and which will not, they are not of much use. (See Mansfield et al. (1971).)

Given the large biases and errors in the estimates that are used in project selection models, it is no wonder that managers have not been quick to adopt them. Indeed, as noted above, there is some evidence that managers may be more optimistic than they have a right to be about the accuracy of some of these estimates. If they had a better idea of how bad these estimates tended to be, they might be even more reluctant to place heavy dependence on them. With regard to these errors and biases, it should be noted that, to a considerable extent, they are not merely a product of uncertainty. It would be naive to close one's eyes to the fact that these estimates are used to allocate the firm's resources. Consciously or unconsciously, cost and time estimates may be biased downward--and estimates of the value of research results may be biased upward--to "sell" projects to management. This factor, as well as the uncertainties inherent in research and development, is responsible for the large errors in these estimates.

Accuracy of Industrial Forecasts of the Profitability of New Products and Processes

Very little information is available concerning the accuracy of estimates of the profitability of investments in new products and processes. In a recent study, Beardsley and I (1978) presented detailed empirical results on this score concerning all of the major innovations developed by one of the nation's largest firms in 1960-64. Because these data have been systematically and carefully updated by the firm, they provide a relatively unique opportunity to study how quickly forecasts of this sort converge on their true value.

These data indicate that the initial estimates of the profitability of a new product or a new process are no more reliable than forecasts of development cost and time. This is not because of inadequate forecasting or analytical work on the part of the firm studied here. Based on all available indications, this firm is among the more competent in this regard in the country. Instead, these results reflect the inherent uncertainty involved in estimating the profitability of an innovation.

Second, our results indicate that it takes four or five years after the development of a new product or process before this firm can estimate reasonably well the discounted profits from the innovation. Undoubtedly, this length of time varies from firm to firm, and we cannot be sure that this firm is typical in this regard. But to the extent that it is typical, potential innovators must reckon on relatively long periods of time when they will be unable to tell with much accuracy whether it was wise or foolish to have developed a particular new process or product. Obviously, this makes life difficult for a potential innovator, who would like to buy information concerning success or failure quickly and cheaply.

Third, in this firm at least, there seem to be large forecasting errors both for new processes and new products, and how long it takes after the new technology is developed to estimate the discounted profits reasonably well does not seem to vary much between new products and new processes. This may seem surprising, since one might think that the firm could estimate its own savings from a new process far better than it could its sales of a new product to other firms and the public. But it must be recognized that the firm finds it difficult to forecast future input prices, royalty receipts, and a variety of other factors influencing the profitability of a new process.

Fourth, and perhaps most interesting from the point of view of public policy, there seems to be a tendency for this firm (and others as well) to underestimate the profitability of very profitable innovations and to overestimate the profitability of relatively unprofitable innovations. In part, this seems to stem from the belief by the forecasters that the penalties for being conservative in their estimates are less than those for being too far out on a limb (particularly in an upward direction). In general, but perhaps not in the case of this firm, this reduction in the forecasted increment between the discounted profits from the expected "big winners" and the more run-of-the-mill innovations may result in a distorted allocation of resources. Because the extra profits to be obtained from the expected big winners are underestimated, many of them may not be carried out on as big a scale or as quickly as would seem justified if the forecasts were unbiased in this regard.

Economic Evaluation: Effects on Probabilities of Success

Although the more sophisticated types of project selection models have not found extensive use, and although the estimates of development cost, development time, and profitability are not very accurate, this does not mean that firms do not find it worthwhile to make relatively straightforward (and often rough-and-ready) evaluations of various project proposals and of continuing projects. On the contrary, the available evidence suggests that most firms make such evaluations--and that a firm's chances of success are related to how quickly such evaluations are carried out.

The probability of technical completion is the probability that an R & D project will achieve its techical objectives. The probability of commercialization (given technical completion) is the probability that a technically complete R & D project will be commercialized--that is, that there will be a full-scale marketing or application of the new or improved product or process beyond a test-market or pilot-plant trial. The probability of economic success (given commercialization) is the probability that a commercialized R & D project will yield a rate of return (on the R & D costs plus any additional investment made to introduce the innovation) in excess of what was available from other (non R & D) investment alternatives. Note that the product of these probabilities equals the probability that an R & D project begun by the firm will be an economic success.

We would expect that all three of these probabilities would be affected by how quickly R & D projects are evaluated from the point of view of potential market and profit. Some firms allow R & D projects to proceed much farther than do other firms before the potential profitability of the project is studied. Table 1 shows that, on the average, the firms that Wanger and I (1975) studied permitted about $40,000 to be spent on a R & D project before such a study was made. But there was a great deal of interfirm variability in this respect. Some firms allowed $200,000 to be spent before such a study, whereas other firms spent little or nothing before it.

In general, there are many arguments for integrating technological considerations with economic considerations relatively early in the game. Unfortunately, one suspects that many firms do not integrate these factors early enough, the result being that many projects with very little potential economic payoff are started and continued too long. And because this is the case, the probability of technical completion is lowered, since more projects are started which are stopped short of technical completion because of poor profit prospects. Also, the probability of commercialization (given technical completion) is lowered because more projects are completed technically before it is recognized that their profit outlook is poor. And the probability of economic success (given commercialization) is lowered, since the firm's portfolio of R & D projects tends to be more poorly geared to economic realities and conditions than would otherwise to the case.

Based on detailed data for 16 firms, Wagner and I (1975) found that, holding other factors constant, each of these probabilities of success was directly related to how quickly economic studies of this sort were carried out. More specifically, each of these probabilities was inversely related to the amount (in thousands of dollars) that could be spent on an R & D project before studies were made of market and potential profit. This relationship was highly significant.

Conclusions

Considerable advances have been made in recent years in the measurement of social returns from industrial innovation. Studies of such social returns have played an important role in recent policy discussions concerning civilian technology (for example, President Carter's Domestic Policy Review on Industrial Innovation). Much has also been learned in the past decade concerning the ways in which firms evaluate R & D projects, and the accuracy of the estimates used for this purpose. The results indicate that estimates of development cost, development time, and profitability are quite inaccurate. For this and other reasons, firms seldom use the more sophisticated models proposed in the literature to select projects. Instead, they generally use simple (and often rough-and-ready) adaptations of capital budgeting techniques. Despite the inaccuracy of the estimates, the available evidence suggests that firms that make a systematic attempt to evaluate a project's economic potential relatively early in the game tend to have a higher probability of success than do other firms.

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