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Capital Budgeting for Research and Development

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As the costs of R&D continue to rise in American industry, it becomes increasingly important to define the various categories involved in the activity in order to determine how to control each —

CAPITAL BUDGETING FOR RESEARCH AND DEVELOPMENT

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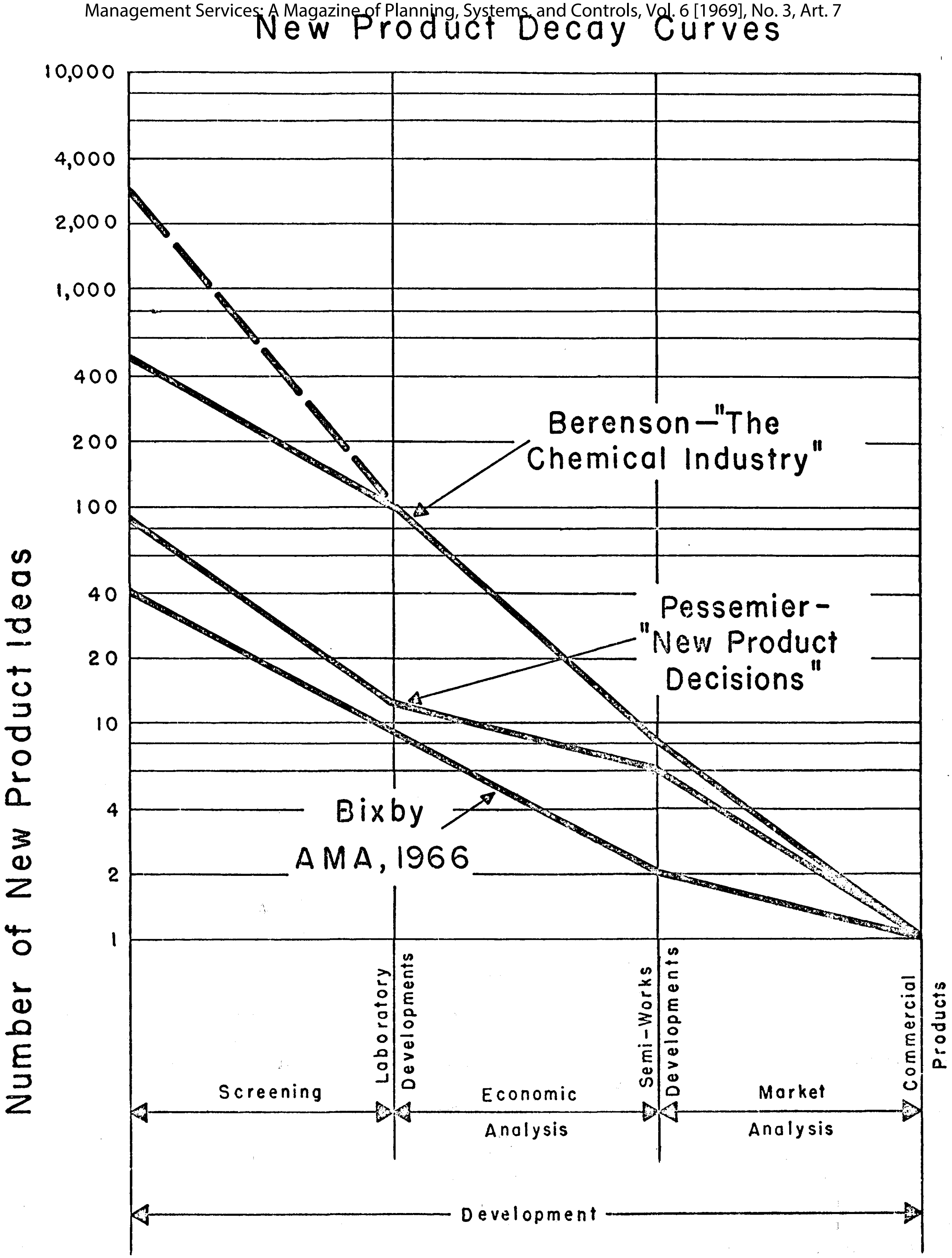
Research and development expenditures have become increasingly important in many companies. From 1953 to 1965 there was an average annual (compound) increase of about 12 per cent in funds for performance of industrial R&D[1]. This growth has slowed recently because of a decreased rate of growth of federally supplied funds; however, many companies appear to be supplying internal funds for R&D at an increasing rate.

This growing attention to research and development has been accompanied by a continuing conflict as to how funds should be allocated to the R&D effort as a whole and how they should be allocated among various projects within the R&D effort. Scientists and engineers argue that R&D expenditures cannot be handled as part of the conventional capital budgeting process because economic evaluation of R&D project proposals is impractical. Most financial managers, on the other hand, resist allocating funds without substantial justification.

As in many debates of this type, at least part of the problem stems from lack of communication and from failure to define the problem fully. This article defines the concept of the R&D spectrum and uses it to show where the arguments of the technicians are stronger and where the desires of the financial managers should dominate.

Where is the line?

Most people have a general idea of the difference between research and development. However, there


New Product Decay Curves
Number of New Product Ideas
10,000
4,000
2,000
1,000
400
200
100
40
20
10
4
2
1
Berenson—"The Chemical Industry"
Pessemier-"New Product Decisions"
Bixby AMA, 1966
Laboratory Developments
Semi-Works Developments
Commercial Products
Screening
Economic Analysis
Market Analysis
Development

is a rather large grey area between the two activities. What one company considers to be research another might define as rather conventional development. The common practice of discussing various subcategories of both research and development can compound the potential confusion. However, this practice can be of value because there are definable differences among various types of research (and development) activities. Thus, the consistent use of several subcategories can facilitate more precise communication.

Funds for Industrial Basic Research, Applied Research, and Development Performance, 1957-65

(All dollar figures are in millions)

Year	Totals	Basic Research $	Basic Research %	Applied Research $	Applied Research %	Development $	Development %
1965	$14,197	$607	4.1	2,673	18.8	10,918	77.1
1964	13,512	564	4.2	2,600	19.2	10,347	76.6
1963	12,630	535	4.2	2,457	19.5	9,638	76.3
1962	11,464	500	4.4	2,449	21.4	8,515	74.2
1961	10,908	407	3.7	1,977	18.1	8,525	78.2
1960	10,509	388	3.7	2,029	19.3	8,092	77.0
1959	9,618	332	3.5	1,991	20.7	7,295	75.8
1958	8,389	305	3.6	1,911	22.8	6,173	73.6
1957	7,731	271	3.5	1,670	21.6	5,790	74.9

Source: Based on data from *Basic Research, Applied Research and Development in Industry, 1965,* Report NSF 67-12, National Science Foundation, Washington, D.C., June, 1967, p. 77.

Five basic categories

This article defines and uses five basic categories of R&D activity. Other terms are often used for them, but the underlying concepts are usually quite similar.[2] The five basic categories are as follows:

Basic research — Basic research consists of investigations attempting to advance fundamental scientific knowledge but with an ultimate commercial objective. This is in contrast to "pure" research, such as that undertaken in many universities, in which there is no direct commercial objective.

Applied research — Applied research differs from basic research in that the specific goals of an applied research project are normally defined before work is initiated. Typical applied research projects include extensions of basic work directed toward a new product line and "fire-fighting" projects triggered by problems in production processes, quality control, or development projects themselves.

Advanced development — Advanced development activities focus on the exploration of engineering-oriented areas of technical uncertainty. The effort is usually concentrated on critical areas so that a more informed decision can be made on whether to accept the project for full-scale development.

New-product development—This is the conventional, coordinated engineering effort necessary to complete development of the new product so that it can be released to the production and marketing activities.

Product improvement — This category includes redesign and similar engineering activities directed toward improvement of products already on the market.

Research spectrum

These five categories can be thought of as a "spectrum" of the total R&D effort ranging from basic research at one extreme to product improvement at the other. Several characteristics of this spectrum are important.

At the research end of the spectrum uncertainty is considerably higher than at the development end; that is, it is very difficult to evaluate a project. For example, consider the "product decay" curves shown in the figure on page 46. In the figure the number of new-product ideas required to yield one successful new product is plotted against various stages in the development process. (Notice that the data cover only the new-product development phase; if the data were extended back into the basic research phase, the "idea mortality" rate would probably be even higher.)

The upper curve is from the Commercial Chemical Development Association and is representative of that industry's experience. Some 500 original new ideas are sifted down to 100 ideas that undergo laboratory evaluations; these drop to 8 or 10 that enter semiworks development; and finally one commercially successful product emerges. The experience of the pharmaceutical industry is even more severe, as shown by the dotted line; there 3,000 ideas are required to yield one commercial product.

The middle curve (from Pessemier[3] based on a Booz, Allen study[4]) shows that 90 ideas result in 12 laboratory developments and 5 semi-works developments to get one new product. The lower curve (from Bixby[5]) shows the experience of the appliance industry. There 40 ideas yield 8 pilot developments and one new product. Thus the degree of uncertainty varies significantly in different industries, but in all cases it is substantial for new-product developments, especially toward the research end of the spectrum.

A second important feature of the spectrum is that although there

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are a large number of new product ideas and projects at the research end of the spectrum, research expenditures are typically relatively low. The table on page 47, which is based on a recent National Science Foundation survey of industrial research spending, shows that, over the nine years covered, basic *and* applied research spending has been between 20 and 30 per cent of total R&D spending. Thus, with a large number of projects and relatively low expenditures, the cost per research project is usually lower than for development projects.

This is partially explained by the fact that the "hardware" costs for research projects are usually minor compared to those required for development projects. As the NSF report states, "In all major manufacturing industries except electrical equipment and communication and aircraft and missiles, the wages and salaries of R&D scientists and engineers and supporting personnel accounted for most of total R&D costs. The relatively high expense for materials and supplies and other related costs in these two industries underscores the high costs of projects largely oriented toward development work."[6]

Application

Knowledge of the R&D spectrum can now be applied to the capital budgeting problem. At the extreme development end of the spectrum the R&D decision environment is quite similar to the conventional capital budgeting environment. Uncertainty is fairly low; reasonable estimates of expected project costs and benefits can be made; project costs as noted, are high; and conventional capital budgeting decision techniques can be employed. Toward the middle of the spectrum uncertainty increases, and costs per project are still high. In this region the more sophisticated models based on risk-type estimates can often be used. Finally, at the research end of the spectrum uncertainty becomes dominant, and even the more sophisticated models are ineffective. However, each project represents a relatively small investment. Thus, there is less need for rigorous evaluation. The practice of granting the individual research scientist more autonomy in the choice of projects than is given to a development engineer has some economic foundation.

In fact, recent research findings have shown that the best basic research results tend to be achieved when projects are selected within the research organization itself. As a result of a study of several laboratories that are "generally conceded" to be outstanding, Isenson concluded that "basic research in the leading corporations observed is 80-100 per cent directed toward the achievement of goals established within the research laboratories."[7]

A warning

A caveat should be added here, however. If the research organization is given greater internal autonomy, care must be taken to ensure that the broad research interest areas of the scientists that staff the laboratory are generally congruent with the long-term technical interest areas of the corporation. To a large extent, this ensures that the projects selected for attention will be of value to the corporation. If this is not done, there is a danger that the laboratory will produce technically and socially valuable pure research results that are unfortunately of only limited economic value to the sponsoring company. The key is to pick good people who are interested in the things you are interested in and *then* turn them loose.

Thus, it can be seen that at one extreme—the development end of the R&D spectrum—projects should be evaluated by essentially conventional capital budgeting techniques while at the other end of the spectrum—the research end—conventional and even more sophisticated techniques are ineffective, and informed judgment must of necessity dominate the decision making process. The crucial question, then, is, "Where does a proposed project lie in the spectrum?"

Classifying projects

In order to clarify this conclusion, some of the principal elements of difference in the R&D decision environment should be considered in more detail. The major elements are uncertainty, long economic time horizons, intangibility of outputs, relation to strategic planning, behavioral factors, and flexibility. All of these factors are present in the typical R&D project decision environment, and they become more dominant in moving across the spectrum from development toward the more research-oriented projects.

Uncertainty—In most R&D project decisions uncertainty is more prevalent than in typical capital budgeting decisions. As mentioned, at the research end of the spectrum this uncertainty can become dominant. In order to aid understanding of this problem, several different types of uncertainty can be defined.[8]

Internal uncertainty refers to the technological, cost, and time uncertainties associated with developing a project to some initially established level of "internal" performance stated in technical and production cost terms. It encompasses all the uncertainties that would remain if the environment external to the project could be forecast with certainty.

External uncertainties are the uncertainties that would still remain if the project could be developed to meet its internal performance goals exactly as predicted. There are two subclasses of external uncertainties: static and dynamic.

Static — Even if a project could be developed instantaneously to meet its internal performance goals, there would still be uncertainty associated with estimates of its commercial success because of inability to predict market acceptance. This is static uncertainty.

Dynamic – The fact that the market is constantly changing adds an additional dimension to the problem. Development is based on forecasts of what market conditions are likely to be when the project is finally completed. This is usually a more difficult task than estimating current market characteristics (the static case).

Uncertainty is norm

The key point is that substantial uncertainty is present in *most* R&D decisions. Thus, conventional capital budgeting techniques that depend on certainty estimates are of only limited value, and even the risk-estimate-based models are almost valueless at the extreme research end of the spectrum.

Longer economic time horizons – For many R&D projects there is a substantial time lag between project initiation and receipt of the first cash inflows. For example, the development work on DuPont's Corfam, a synthetic material intended as a replacement for leather, was spread over a period of 35 years.[9] This in itself is not a unique characteristic. Investment in a bridge or a dam also covers a long time span; however, in the case of R&D projects the longer time span compounds the already difficult dynamic uncertainty problem.

Intangibility of outputs—A conventional capital budgeting system is based on estimates of cash flows associated with expenditure proposals. However, instead of some physically countable or at least "accountable" product, the only output of many R&D projects is knowledge.

Quinn tried to grapple with the problem of evaluating the output of various parts of an R&D organization. He proposed that output be measured as the net present value of the information produced by each organizational unit.[10] (He defines this as the dollar value of the output information to the organization receiving it minus the value of the input information received by the organizational unit being evaluated.) Since this evaluation must be made at each internal organizational interface, the approach presents formidable problems and has not to my knowledge been applied in practice.

Relation to strategic planning – Because the development time required for many R&D projects roughly coincides with the strategic planning horizon of many companies, it is necessary to coordinate strategic planning more closely with research budgeting than with most conventional capital budgeting. The ultimate products of presently funded research programs will in large measure define the future strategic position of the firm.

Behavioral factors – The relation between the R&D allocation process and the effectiveness of the R&D activity is frequently more direct and more significant than similar interactions between the conventional capital budgeting decision process and organizational performance on the programs selected. From one-half to three-fourths of the average company's R&D budget is used to pay the technical staff and its supporting people (technicians, secretaries, etc.).[11] One of the key outputs that the company hopes it is purchasing is the creativity of this staff. The R&D allocation system can significantly affect the quantity, the quality, and the economic value of this creative output.

Flexibility – Management is much more constrained in its ability to adjust the level of the R&D effort than in its ability to adjust the level of the capital investment program. When the company wishes to reduce traditional forms of capital expenditures, it is not difficult to limit the award of construction contracts or delay machine replacement. However, many R&D projects require continuing support over a period of years, and support levels cannot usually be varied from the programed level

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without significant loss of efficiency.

More important, because of the dominance of personnel costs in the R&D budget, the only way to cut back R&D expenditures substantially is to reduce the technical staff. Such a reduction (or even the threat of it) can have a serious effect on creativity. When people's basic security is threatened, they are not likely to engage in significant long-range creative efforts, directed toward company goals; instead, their mental efforts are directed toward the short-range personal goal of self-protection. The atmosphere created tends to reduce cooperation; more noncontributory effort is exerted in placing the blame for failures and making sure that the "proper credit" is received for success. Emphasis is on the short-range "showy" projects with a high probability of success rather than longer-range, potentially more valuable (but riskier) projects.

Dangers of cutbacks

If such cutbacks are frequent, the firm soon gains a poor reputation in the market for scientists and engineers, where, because of the continuing excess of demand for such people over the available supply, most technical employees have a considerable freedom of choice among employers. Everything else being equal, they will choose the more stable employment environment. It is, of course, possible to overcome this reluctance on the part of potential technical employees by paying some sort of a premium (salary, bonus, etc.) to offset the unfavorable instability factors. However, the effect of a fluctuating staff level on the efficiency of those already in the organization must be considered. In addition, it is possible that new technical employees attracted by such an unstable but lucrative environment are those who are more politically than technically oriented and, thus, think that they can excel in such an environment.

(The preceding discussion should not be taken to mean that cutbacks cannot be made in the R&D program and most especially that incompetent people should not be fired. Jones' study has shown that research people try to understand management's position and that they are as interested as management in removing nonproductive employees.[12])

Conclusion

Expansion of the R&D effort also presents more problems than a similar expansion of the capital investment program. A buildup in capital investment can usually be accomplished fairly easily except when bottlenecks in the capital goods and construction industries are usually severe, as in 1966. However, a significant increase in R&D output faces a host of bottlenecks: difficulty in finding and hiring certain specialists; a definite and sizable time lag required to integrate new people into the organization and make them effective as a team; and the substantial delay between the time they begin working effectively and the time when useful output is available (the time horizon problem again).

In summary, we can see that there are elements of truth to the arguments of both the scientists and the financial managers and that the answer lies somewhere between their positions. There are many development-type projects that can be handled by essentially conventional capital budgeting techniques. In many if not most companies such projects make up the major portion of the total R&D budget.

However, the financial manager must realize that there are major differences between the R&D and more conventional capital budgeting environments. For projects nearer the research end of the spectrum these differences become so significant that normal capital budgeting procedures usually are ineffective. Here most companies give the individual research scientist more autonomy in project selection decisions. Thus one of the first and most important tasks of the project selection process is in properly classifying a proposed project and guarding against the tendency for all projects to be proposed as "research."

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[1] *Basic Research, Applied Research and Development in Industry, 1965,* NSF 67-12, National Science Foundation, Washington, D.C., June, 1967.

[2] For example, the National Science Foundation typically utilizes three categories: basic research, applied research, and development. See National Science Foundation, *op. cit.*, p. 101, for its definitions. On the other hand, Pessemier focuses more on new product development efforts and defines six categories: search, preliminary economic analysis, formal economic analysis, development, product testing, and commercialization. See *New Product Decisions* by Edgar A. Pessemier, McGraw-Hill, Inc., New York, 1966, p. 10.

[3] Pessemier, *op. cit.*

[4] *Management of New Products,* Booz, Allen and Hamilton, Inc., New York, 1960.

[5] Unpublished presentation by Carl L. Bixby, Jr., at American Management Association R&D Orientation Seminar No. 7210-69, "Finding, Screening, and Appraising New Products," Jan. 19-21, 1966.

[6] National Science Foundation, *op. cit.*, p. 10.

[7] "Allowed Degrees and Type of Intellectual Freedom in Research and Development" by R. S. Isenson, *IEEE Transactions on Engineering Management,* Vol. EM-12, No. 3, September, 1965, p. 115.

[8] This discussion generally follows the concepts discussed in *Issues in the Choice of Development Policies* by T. K. Glennan, RAND Corporation, P-3153, October, 1965.

[9] "Harnessing the R&D Monster" by H. Kay, *Fortune,* January, 1956, p. 160.

[10] *Yardsticks for Industrial Research, the Evaluation of Research and Development Output by* J. B. Quinn, Ronald Press, New York, 1959.

[11] National Science Foundation, *op. cit.*, p. 43.

[12] *The Application of Management Controls to Technical Research Employees* by S. L. Jones, unpublished Ph.D. thesis, Graduate School of Business, Stanford University, Stanford, California, 1960.