Chapter 4

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This chapter reviews the use of quantitative analysis in industry research and development (R&D) decisionmaking at the level of the individual firm. OTA reviewed articles, surveys, and reports, and interviewed several research managers. Very little systematic information is available about industry's use of quantitative models in research resource allocation. A few surveys cover limited numbers of firms and are not necessarily representative. Much of the material is in the form of anecdotal accounts of an individual firm's uses of certain methods, providing no information on whether other firms have adopted similar approaches. Relatively more information is available about noneconomic quantitative methods for project selection than about economic models. The literature, as well as OTA interviews, demonstrates the limited practical utility of quantitative techniques for research decisionmaking in industry, and the reliance on subjective judgment and good communication between R&D, management and marketing staffs in the decisionmaking process.

In the private sector, R&D is an investment that must compete for corporate support with other investment opportunities such as plant expansion or new product marketing. Program and laboratory directors must defend the value of their research to top management and decide what mix of projects is best for the firm. Project managers must determine whether their projects are proceeding as planned and whether expected payoffs will justify costs. This chapter looks in turn at the use of quantitative methods in review and evaluation of ongoing research, new research project selection, and research resource allocation as part of strategic planning.

REVIEW AND EVALUATION OF ONGOING RESEARCH ACTIVITIES

As part of their normal operations and management, firms periodically review research programs, projects, and staff to assess progress and determine the contribution that individual researchers and research groups are making to the firm's goals. These reviews can justify research expenditures to management, assist in budget and. program planning, or evaluate personnel performance.

Few firms use quantitative methods to review ongoing research. In 1982, Schainblatt surveyed 34 R&D-intensive firms about methods of measuring research productivity.[1] The survey focused on the groups, programs, or other organizational units, rather than on individual scientists or engineers. Managers in only four firms reported using performance or output measures as part of their program reviews. Only 20 percent of the firms routinely collected any kind of productivity data, and 20 of the 34 firms reported using no productivity-related measures at all.

Several respondents said they had tried for years to measure R&D productivity but had not been successful. Noted one research manager, "We . . . came to the conclusion that there is no good way to do it on a week-to-week or month-to-month basis. " Managers doubted that R&D productivity measures were meaningful. According to one manager, "Attempts to quantify benefits of R&D have led to monstrosities that caused more harm than good. "[2]

A. Schainblatt, "How Companies Measure the Productivity of Engineers and Scientists, " *Research Management,* May 1982, pp. 10-18.

'Gerald A. Cole, ***The Evaluation of Basic Research in Industrial Laboratories*** (Cambridge, MA: Abt Associates, 1985), p. 59.

Macroeconomic Models

The retum-on-investment (ROI) model is one of a family of economic analysis tools, along with discounted cash flow analysis and present value analysis, which business managers use as aids to investment decisions. These methods are commonly applied to decisions where uncertainty is low. As we have seen in chapter 2, research investment decisions entail considerable uncertainty. Moreover, the ROI methods carry a bias against the long-term, high-risk, and high-uncertainty projects like basic research, and work in favor of high-vield, short-term investments.[3] The bias increases in periods of high inflation like the 1970s.

Despite the fact that economists have typically viewed R&D activity as an investment, a number of scholars recently have criticized the misuse by private sector managers of quantitative financial techniques for evaluating investments in R&D and technology more generally. Hayes and Abernathy argue that the application to investment decisionmaking of principles of portfolio management has led U.S. firms to underinvest in new technologies:

> Originally applied to help balance the overall risk and return of stock and bond portfolios, these principles have been applied increasingly to the creation and management of corporate portfolios —that is, a cluster of companies and product lines assembled through various modes of diversification under a single corporate umbrella. When applied by a remote group of experts primarily concerned with finance and control and lacking hands-on experience, the analytic formulas of portfolio theory push managers even further toward an extreme of caution in allocating resources.[4]

Similarly, Hayes and Garvin have argued that present-value analysis of R&D investment decisions has led to a systematic bias against such investments, due in part to the use of discount or "hurdle" rates for such decisions that are too highs Rather than relying solely on quantitative techniques for the evaluation of R&D investments, these authors argue that managers must develop an understanding of the underlying technologies, and apply informed judgment in making such decisions.

ROI methods, as applied to R&D projects, estimate the economic value of research and compare it with the cost to the organization. For example, a firm could estimate the sales or revenues generated or expected from a new product resulting from research efforts. Alternatively, the firm could estimate the share of total profits or savings attributable to research. The financial results are usually discounted to reflect the time value of money.

One major problem with such methods is that it is difficult to apportion profits generated by a product developed in the past among research, development, and marketing activities, all of which contribute to the process. Even if one could attach accurate figures to the present payoff from past research, such calculations offer little guidance for current decisions, which are occurring under different technological, management, economic, and organizational conditions.[b] In addition, companies often acquire R&D through purchase, corporate acquisition, or merger; or they may sell R&D themselves.

ROI techniques seem most applicable to justifying the value of ongoing research to top management by placing a value on past research. Such

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[3] J.E. Hodder and H.E. Riggs, "Pitfalls in Evaluating Risky Projects," *Harvard Business Review,* January-February 1985, pp. 128-135; and G.F. Mechlin and D. Berg, "Evaluating Research—ROI 1s Not Enough," *Harvard Business Review,* September-October 1980, pp. 93-99.

[4] Robert H. Hayes and William J. Abernathy, "Managing Our Way to Economic Decline," *Harvard Business Review, vol. .s8,* No. 4, *1980,* pp. 67-77, reprinted in *Survival Strategies for American industry,* Alan M. Kantrow (cd.) (New York: John Wiley & Sons, 1983), pp. 15-3S (quotation is from pp. 22-23, 1983 reprint).

[5] As these techniques have gained ever wider use in Investment decisionmaking, the growth of capital Investment and R&D spending m this country has declined We summit that the discounting approach has contributed to a decreased willingness to invest for the following reasons: (1) it is often based on misperceptions of the past and present economic environment; and (2) it is biased against investment because of critical errors in the way this theory is applied Bluntly stated, the willingness of *managers* to view the future through the reversed telescope of discounted cash flow is shortchanging the future of their companies.

Robert H. Hayes and David A. Garvin, "Managing As If Tomorrow Mattered," *Harvard Business Review,* VOI. 00, No. 3, pp. 70-79, reprinted in *Survival Strategies for American Industry,* Alan M. Kantrow (cd.) (New York: John Wiley & Sons, 1983), pp. 30-51, (quotation is from p. 37, 1983 reprint).

[6] B. Twiss, *Managing Technological Innovation* (New York Longman, 1980), pp. 121-122; and D W. Collier, "Measuring the Performance of R&D Departments," *Research Management,* VOI, 20, No. 2, 1977, pp. 30-34.

techniques do not appear very helpful for determining changes in a research program's budget, or deciding the fate of particular project. The ROI methods and other cost/benefit methods, as demonstrated below, are more applicable to decisions involving setting priorities among a set of applied research projects.

Business Opportunity Techniques

Collier describes a "business opportunity" technique that avoids some of the pitfalls of ROI appreaches. ' It is based on the notion that the primary objective of industrial research is to identify and define business opportunities that can be exploited commercially. Management evaluates the performance of research staff by comparing its technical accomplishments against a set of previously established objectives—for example, to develop a new control system with certain performance characteristics. Next, when the project is completed and is ready to be transferred to the production and marketing departments, total expected sales revenues are estimated and discounted to the present. The result, when divided by the project's cost, is a return on research figure which, if summed across all completed projects, yields an estimate of the research department's value to the company for that year.

The business opportunity method depends on the accuracy of future sales estimates and of product development time, which are subject to substantial error. Thus, uncertainties in the ROI method's allocation of credit for profits retrospectively among the various factors are replaced by the business opportunity method's uncertainties about future payoffs. *Collier evidently assumes that there is a higher level of certainty about future profits than about the contribution to profits of past activities.* Nevertheless, Collier states explicitly that neither the ROI nor the business opportunity method should be used to evaluate basic research, ' probably because particular basic research projects and programs are difficult to tie uniquely to economic impacts, which may well be separated from the research by many years and institutional boundaries.

Little is known about the use of other formal evaluation techniques such as bibliometrics, patent counts, and colleague surveys for peer review. DuPont, Bell Laboratories, and other large industrial research establishments carry out intensive, annual reviews of their scientists' work. These reviews stress the scientists' contribution to science and technology, particularly in areas of strategic interest to the company. The reviews are performed by peers in the fields of research concerned.[9] No evidence suggests that this practice is widespread. One complicating factor is that much industrial R&D is shared work, so attributing some portion of its output to one individual is difficult.

'Ibid.
'Cole, op cit., p. **38.**

'Ibid., p. 59

R&D) PROJECT SELECTION

Relatively more information exists about industry's use of formal, quantitative techniques to select R&D projects and shape research portfolios. An extensive literature on models and methods exists and it includes some surveys on their use. These algorithms or heuristic devices help managers assign values to projects, groups of projects, or other investments. The approaches fall into four categories:

1. scoring models,
2. economic models,
3. constrained optimization or portfolio models, and
4. risk analysis or decision analysis models.

Scoring Models. When scoring models are used, each project is rated against a series of relevant decision criteria. Scores for each project are combined through addition or multiplication to develop a single project score.

In a typical application, all candidate projects are scored and ranked from highest to lowest.

Since costs are associated with each project, the allocation decision involves simply going down the list of projects until the available funds have been exhausted. This procedure does not consider the effect of variations in project budgets, marginal returns from varying project funding levels, interactions among projects, or changes in annual budget levels over the life of the projects.

Scoring models have the least demanding input data requirements of the four categories of models. They are designed to incorporate noneconomic criteria, and can operate on input data in the form of subjective estimates from knowledgeable people. The assumptions underlying scoring models are relatively undemanding: only that explicit evaluation criteria and a way to quantify each evaluation be developed. While expert judges may be used as the source of qualitative input data, eventually these data must be expressed quantitatively to be used in a scoring model. The choice of algorithm to convert qualitative data to quantitative scores is arbitrary and subjective.

Economic Models. With economic models, projects are rated against a series of economic criteria such as expected rate of return. A single figure of merit is produced, typically reflecting the ratio of the present value of earnings from the project, including the probability of project success, to discounted money flow or investment. These are essentially capital budgeting models. Economic models accept only quantitative data based on estimates of the financial performance of the project over a specified planning horizon. These estimates are often generated by program or project managers or by panels of experts. They possess no greater intrinsic validity than data developed for scoring models. This is particularly true when uncertainties about the technical and market performances of the technology are high, a situation frequently encountered in early stages of applied research. Any estimate of future project benefits requires subjective input from some well-informed respondent or group of respondents. 10

Like scoring models, economic models produce a single figure of merit that is independent of the figures of merit for competing projects. The simplest application is to rank projects and fund those scoring highest until the available funds are exhausted. The narrow focus of economic models may limit their usefulness for government research, where cost and economic gain are only part of a much larger set of criteria.

Constrained Optimization or Portfolio Models. These models measure a program's potential to meet a goal, usually a series of economic objectives, subject to specified resource constraints. Unlike scoring and economic models, the focus is on a mix of projects rather than a simple ranking of individual projects. In portfolio analysis, mathematical programming techniques are used to evaluate the allocation of resources among candidate projects. The method requires an understanding of the relationship among resource inputs, technological performance, and marketplace response. The decisionmakers must agree that funding just the right mix of resources among projects is the key to effective R&D management.

While demanding better data quality and understanding of underlying technical and economic processes, portfolio models can handle multiple constraints and different budget levels over different years in the planning horizon. But the use of these models implies a level of management control and flexibility to reallocate resources that may not exist in many situations, particularly in government.[11]

Risk Analysis or Decision Analysis. These models produce an expression for the expected utility of each set of alternative budget allocations among a set of research projects. Models using decision analysis have the most complex data requirements, since inputs must be in the form of probability distributions. As in the case of portfolio analysis, considerable understanding of underlying processes must exist if the benefits of decision analysis are to be realized. Decision analysis incorporates expert judgments as well as "objective" data.

N'. R. Baker, "R&D Project Selection Models: An Assessment," ***IEEE Transactions on Engineering Management,*** EM-21, November 1974.

'K. G. Feller, *A Review of Methods for Evaluating R&D* (Livermore, CA: Lawrence Livermore Laboratory, 1980), p. 19

Use of R&D Project Selection Models in Industry

A 1964 survey of the use of quantitative R&D project selection methods concluded that while numerous models and techniques had been proposed, available data showed "little thorough testing and only scattered use of the proposed methods."[12] A second survey published 2 years later led to a similar conclusion:

> The practice of project selection in industry and government is dominated by . . . methods depending heavily upon individual or group judgment and using very little quantitative analysis. The use of cost and return estimates is common, but very few organizations employ any formal mathematical model for combining these estimates and generating optimal project portfolios.[13]

Rubenstein pointed out in 1966 that the use of quantitative methods by R&D organizations had not increased appreciably since 1950, and that the reasons for this had more to do with the nature of the available data and the R&D decision process than with the sophistication of available models.

In a 1968 study of the R&D project selection practices of 36 firms, Dean found that formal, quantitative models were not widely used. Simple scoring models employing only a few criteria such as probability of technical success, estimated time to completion, cost, and size of net market gain were the only mathematical models that had been tried.[14] Meadows' 1968 report on practices in the R&D labs of five major companies found that the margin of error in estimates undermined the usefulness of the methodologies.[15] He provides a telling example:

> If estimates with only a ten percent error were inserted in the formula, they could conceivably lead management to calculate a higher profit ratio for a project actually expected to lose money for the firm than for one expected to return 230 percent on the money invested in its development. This sensitivity of the model's output to error is important in view of the fact that no laboratory yet studied has had estimates (of error) averaging as little as ten percent.[16]

Mansfield, who has conducted numerous studies of R&D and innovation in industry, noted in a more recent article that:

> . . . most companies . . . have found it Worthwhile to make economic evaluations of project proposals and continuing projects, often adapting such capital budgeting techniques as rate of return or discounted cash flow to the task at hand.[17]

But he goes on to say that the nature of the techniques used will vary, depending on the stage of research. Early on, when costs are low and uncertainty high, project screening will be quick and informal. Later, the larger labs make some use of quantitative methods:

> In some labs, they [quantitative methods] are taken quite seriously indeed; in others they are little more than window dressing for professional hunches and intra-company politics . . . The more sophisticated types of models have not been extensively used.[18]

Although it is important to distinguish among the various research activities when discussing the use of such models, surveys rarely do so. One notable exception is a 1971 study of project selection practices by a task force of the Industrial Research Institute. The task force studied 27 companies and classified their R&D programs into three types: exploratory, high risk business development, and support of existing business,[19] Among the firms studied, those engaging in exploratory R&D generally used simple, unsophisti-

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[12] N. R. Baker and W.H. Pound, "R&D Project Selection: Where We Stand," *IEEE Transactions on Engineering Management,* EM-21, November 1974, p. 130.

[13] A.H. Rubenstein, "Economic Evaluation of Research and Development: A Brief Survey of Theory and Practice," *The Journal of Industrial Engineering, vol.* 17, **November 1966, p. 616.**

[14] B. V. **Dean, "Evacuating, Selecting and Controlling R&D Projects," *Research Study 89 (New*** York: American Management Association, 1968).

[15] D. L. Meadows, "Estimate Accuracy and Project Selection Models in Industrial Research," *industrial Management Review,* spring 1968, pp. 105-119

[16] Ibid., p. 116.

[17] Edwin Mansfield, "How Economists See R&D" *Research Management, vol. 25, 1982,* p. 25

[18] Ibid.

[19] R, E. Gee, "A Survey of Current Project Selection Practices *Research Management, vol.* 14, September 1971 pp 38-45

cated selection procedures. Decisions on funding were based on a page or two of qualitative information or a simple rating scheme. Decisions on high risk business development projects were occasionally supported by more sophisticated, quantitative techniques such as standard economic projections. There was very limited use of quantitative methods for dealing with uncertainty. For decisions about projects in support of existing business, on which quantitative data with very low uncertainty could be brought to bear, standard economic projections were widely used. A 1985 survey indicates little has changed: industry managers rely on qualitative evaluation of basic research programs and proposals.

There is substantial agreement that the less complex scoring models with less demanding input requirements are more appropriate for earlier stages in the R&D process than the more analytically sophisticated economic and linear programming models. One literature review concluded that economic models are too quantitative for evaluating even applied research efforts; they can help only in identifying the information needed to make a qualitative estimate of the economic merit of applied research projects.[20] Twiss, in the second edition of his respected text on the management of technological innovation, finds little value in sophisticated analytic models for R&D project selection:

> While the formulae may give satisfactory symbolic representation, it is doubtful whether they provide a mechanism of much operational value. The judgments involved are so complex there is a great danger of the formulae being used to apply a veneer of pseudo-quantification to support decisions which have already been taken on different considerations . . . If the data is poor they are little better than descriptive representations of the problem. When applied to estimates of the order of inaccuracy discussed earlier, they can do a positive disservice by concealing in a simple index the magnitude of uncertainties. However, there are some types of R&D work where it is possible to assess both the benefits and costs to a high degree of accuracy. These are usually development projects.[21]

The basis for judging the appropriateness of different types of project selection models for different stages of R&D should not rest on distinctions between quantitative and qualitative data, since it is always possible to assign a number to qualitative data using some arbitrary algorithm. Rather, the association should be based on the level of certainty involved in the estimates of the probability that technical and market performance goals will be achieved at a certain cost within a specified time. In basic research and in the early stages of applied research, these factors can be predicted with little certainty. (See box B.)

Reasons for Levels and Patterns of Observed Use

Industry managers recognize that attempts to link basic research activities directly and quantitatively to any kind of "payoff"—new products, profits, corporate image internal consulting, scientific knowledge, or personnel recruitment—are flawed and of limited value. The uncertainties are too great, the causal paths too diffuse, the benefits too difficult to measure, and the timeframe too extended. Basic research usually represents a small fraction (5 to 10 percent) of R&D budgets, and is not subject to the same financial scrutiny as applied research and development activities.

The realities of the research process account for the limited use of quantitative project selection models. The increasing sophistication of the models has not improved their acceptance. In fact, the increased sophistication may create as many limitations as it removes.[22] Fundamental inadequacies in the data required greatly limit their value. Studies of company estimates of project cost and time requirements show that they are usually highly inaccurate. Mansfield, et al.,[23] found that in one drug firm, the average ratio of actual to estimated development costs exceeded

[20] W.M. Burnett and D.J. Monetta, ***Applied Research Project Selection in Mission-Oriented Agencies**: An Approach* (Washington, DC: U.S. Department of Energy, Assistant Secretary for Energy Technology, Division of Power Systems, 1978).

[21] Twiss, op. cit., p. 135.

[22] E. P. Winkofsky, et al., "R&D Budgeting and Project Selection: A Review of Practices and Models," *TIMS Studies in the Management Sciences*, vol. 15, 1980, p. 192.

[23] Mansfield, op. cit.

Box B.—Differences Between Implicit Assumptions of Project Selection Models and Typical Decision Environments

Implicit Assumptions	Typical Decision Environment
1. A single decisionmaker in a well-behaved environment	1. Many decisionmakers and many decision influencers in a dynamic organization.
2. Perfect information about candidate projects and their characteristics; outputs, values, and risks of candidates known and quantifiable.	2. Imperfect information about candidate projects and their characteristics; project outputs and values are difficult to specify; uncertainty accompanies all estimates.
3. Well-known, invariant goals.	3. Ever-changing, fuzzy goals.
4. Decisionmaking information is concentrated in the hands of the decisionmaker who has all the information needed to make a decision.	4. Decisionmaking information is highly splintered and scattered piecemeal throughout the organization, with no one part of the organization having all the information needed for decisionmaking.
5. The decisionmaker is able to articulate all consequences.	5. The decisionmaker is often unable or unwilling ,to state outcomes and consequences.
6. Candidate projects are viewed as independent entities to be evaluated on their own merits.	6. Candidate projects are often technically and economically interdependent.
7. A single objective, usually expected value maximization or profit maximization, is assumed and the constraints are primarily budgetary in nature.	7. There are sometimes conflicting multiple objectives and multiple constraints, and these are often noneconomic in nature.
8. The best portfolio of projects is determined on economic grounds.	8. Satisfactory portfolios may possess many noneconomic characteristics.
9. The budget is "optimized" in a single decision.	9. An iterative recycling budget determination process is used.
10. A single, economically "best" overall decision is sought.	10. What seems to be the "best" decision for the total organization may not be seen as best by each department or party, so that many conflicts may arise.

SOURCE: Adapted from William E. Souder, "A System for Using R&D Project Evaluation Models," *Research Managem ent*, September 1978, pp. 29-37.

2 to 1 development time required exceeded estiates by a factor of almost 3. A more recent study of major innovations developed during a 5-year period by a large U.S. company indicated that initial estimates of an R&D project's expected profitability were no more reliable than the drug firm's cost and time estimates. "The chances were about 50-50 that the estimated discounted profit from a new product or process would be more than double or less than half the actual discounted profit ."[24] The inaccuracies of such measures point to the potential unreliability of quantitative evaluation of basic research activities.

The reasons for lack of reliance on models for research decisionmaking include:

- inadequate treatment of multiple, often interrelated criteria;
- inadequate treatment of project interrelationships;
- lack of explicit recognition and incorporation of the experience and knowledge 01 the researchers and managers;

"Mansfield, op. cit., p. 26

- inability to recognize and treat nonmonetary aspects of research programs that are difficult to understand and use; and
- inadequate treatment of program and staff evolution.[25]

[25]Winkofsky, et al., op. cit., pp. 191-192.

Mansfield adds that many models fail to recognize that R&D is a process of uncertainty reduction—in effect, buying information.[26] Thus, technical failures are successes in that they provide valuable information.

[26]Mansfield, op. cit., p. 25.

STRATEGIC PLANNING AND RESOURCE ALLOCATION

Throughout the 1960s, company managers generously funded "open-ended" research—research that was not necessarily directed toward increasing corporate profits. However, corporate investment in R&D decreased significantly in the 1970s. Dr. Alan Frohman, a management consultant and faculty member at Boston University's business school, believes management's attitude towards investing in R&D:

> . . . has seesawed from unquestioned support and optimism in the 1960s to withdrawn support and discouragement . . . in the 1970s.
>
> " In the 1960s, the attitudes were evidenced by the building of large, well staffed laboratories, often remote from the businesses. During the 1970s, the major, painful cutback in both expenditures and staffing documented management's discouragement with the contribution of technology to the bottom line.[27]

Giorgio Petroni, a professor who has studied the history of management, contends that this "discouragement" was caused by the lack of attention given to strategic planning.

> During the 1970s, management, even in "technology intensive" enterprises, showed little understanding of the need to develop technological expertise within their organizations. Top management often did not understand the full importance of technology as an element of competitive strategy.[28]

Petroni and Frohman are not the only scholars to cite the failure of management to plan for technological innovation as a major factor in the reduction of profits in American industries. In fact, in 1980, when Frohman's study was published, several major literature reviews emphasized what Alan Kantrow has called "the strategy-technology connection."[29]

After reviewing the research literature from the 1970s, Kantrow concluded that there is no rational justification for separating technology from strategy:

> Technological decisions are of fundamental importance to business and, therefore, must be made in the fullest context of each company's strategic thinking. This is plain common sense. It is also the overwhelming message of this past decade's research.[30]

Based on the little that was known, Kantrow tentatively identified the key elements of corporate technology strategy:

> . . . good communications, purposeful allocation of resources, top-level support within the organization, and careful matching of technology with the market.[31]

While scholars and managers alike knew little about technological planning in 1980, they knew a good deal about the theories and practices associated with strategic management. In the same issue of the *Harvard Business Review* that featured Kantrow's article, a review by Frederic W. Gluck, et al., stated: "for the better part of this decade, strategy has been a business buzzword."[32] The in-

[27]Alan L. Frohman, "Managing the Company's Technological Assets," ***Research Management,*** September 1980, pp. 20-24.

[28]Giorgio Petroni, "Who Should Plan Technological Innovation?" ***Long Range Planning, vol. 18, No. 5, 1985,*** pp. 108-115.

[29]Alan M. Kantrow, "Keeping Informed: The Strategy-Technology Connection," ***Harvard Business Review,*** July-August 1980, pp. 6-21.

[30]ibid., p. 6.

[31]Ibid, p. 11.

[32]Frederic W. Gluck, et al., 'Strategic Management tor Competitive Advantage," ***Harvard Business Review,*** July-August 1980, p. 154.

crease in the use, and misuse, of this term is important because it signifies a shift in managers' planning approach from technicalities "to substantive issues affecting the long-term well-being of their enterprise."[33] Despite this change, most managers did not apply these long-term strategies to their R&D divisions in the 1970s.

However, it is important to note that during this period there was a significant countertrend.[34] The verbose title of a 1973 *Chemical Week,* tells the story: "Research Gets the Word: If It Doesn't Fit, Forget It—It's The New Way of Life: R&D Must Mesh Closely With Corporate Goals."[35] In 1976, Union Carbide's R&D director told a reporter: "R&D is too important to be left to the R& D'ers; R&D is the future analog of today's capital expenditures."[36] The vice president of Celanese agreed: "At the high cost of R&D, we can no longer afford to plan and manage it in a random manner. It has to be very closely tied to strategic business planning."[37]

Eastman Kodak, a pioneer in industrial research, was one of the first companies to make a concerted effort to incorporate R&D issues into its strategic planning process in the 1970s. This represented a major change in the company's R&D policy from the 1960s when research director C.E. K. Mees articulated Kodak's hands-off policy:

> The best person to decide what research work shall be done is the man who is doing the research, and the next best person is the head of the department, who knows all about the subject and the work; after that you leave the field of the best people and start on increasingly worse groups, the first of these being the research director, who is probably wrong more than half of the time; then a committee, which is wrong most of the time; and finally, a committee of vice-presidents, which is wrong all the time.[38]

In 1978, Kodak formalized the relationship between corporate management and R&D by establishing a technological affairs committee.[39] Former Kodak Vice President W.T. Hanson, Jr., outlined the five tasks given the committee:

1. To assess long-term technical opportunities as they emerge from the basic research environment.
2. To establish broad goals for the commitment of Kodak R&D resources. These goals should meet short-term product and process needs as well as long-term technology needs.
3. To ensure that resources are properly allocated to develop the technology necessary to support the longer range business objectives.
4. To approve major corporate product programs including specific goals which encompass the following:
   —schedule,
   —specifications of features and functions,
   —resources required,
   —corporate return, and
   —assessment of risk.
5. To monitor progress in corporate projects and approve any changes which have an impact on corporate goals.

Corporate projects as well as the committee's progress were monitored in weekly meetings, which were chaired by the Chief Executive Officer (CEO). The meetings included top management and the staff directly responsible for projects under review. To Hanson, these sessions represent top management's "real commitment" to R&D planning, an idea that was almost unheard of during Mees' tenure.

Kodak's decision to establish a technical advisory committee was not unique. DuPont and Monsanto, two major chemical manufacturers that are now entering the field of life sciences, increased the power of already existing committees in order to streamline the R&D budget allocation process. Prior to 1979, DuPont had established an executive committee to oversee the activities of the R&D divisions. According to Robert C. Fortney, Executive Vice President of R&D, "the

[33] Ibid, p. 154.

[34] Kantrow, op. cit., does believe that managers' awareness "of the need to **Incorporate technological** issues within strategic decision making" (p. 6) grew during the 1970s. However most of his article emphasized management's refusal to see the connection between technology and strategy, Kantrow (personal communication, 1985) said that *he* believed **corporate managers' views of planning technological innovation have evolved over the past three decades** (as opposed to swinging from one extreme to another).

[35] Edward D Weil and Robert R. Cangemi, "Linking Long-Range Research to Strategic Planning," *Research Management,* May-June **1983, p 33**

[36] Ibid

[37] Ibid.

[38] Ibid.

[39] W. T. Hanson, Jr "Planning R&D at Eastman Kodak *Research Management,* July 1978, p 24

head of central research and each of the individual operating departments had a liaison arrangement with different members of the executive committee who loosely kept track of the whole thing.” In 1979, the company was reorganized, and the loose arrangement between the various divisions and top management was replaced by one that was more structured. Fortney believes this change led to “greater corporate involvement in deciding how much of [the total budget] would be in one field, and how much of it would be in another field. ”[40] This increase in corporate involvement has forced the members of the executive committee to become more informed. For example, Fortney meets bimonthly with the 11 research directors in the company, reads quarterly research progress reports from the operating departments and the engineering department, reviews monthly reports from the central research and development department, and listens to presentations from the first line people on the results of their research, usually two or three times a month. In addition, he talks informally with the research directors about their budget plans .41 Through these formal and informal meetings, which enable corporate management and R&D managers to share information, DuPont has tried to include technological issues in its corporate strategy.

Like Fortney, Howard Schneiderman of Monsanto strengthened an underutilized committee structure when he became the Senior Vice President for R&D in 1979. Three committees are now involved in the budget allocation process. The technology advisory council, chaired by Schneider-man, is composed of the directors and general managers of R&D and technology. Most of the council meetings focus on the administrative concerns that effect the management of the scientific enterprise. The council is also a forum in which proposals for new or continuing research programs are suggested and discussed. Then, the technology review committee, which is also chaired by Schneiderman and which includes the company’s senior executives, evaluates these programs by determining whether or not they are commensurate with the corporation’s goals. Finally, the programs’ budgets are evaluated and then approved or rejected by the executive management committee, which is composed of top management (including Schneiderman) and which is chaired by the company’s CEO. Schneiderman contends that the change in Monsanto’s view of the importance of R&D (which is reflected in the change in the budget allocation process) came about as a result of the company’s decision to shift its emphasis from producing industrial chemicals to the field of life sciences:

> One way to put it is that Monsanto is now into more brain-intensive and less raw material and capital-intensive businesses than we have been before. And that has some enormous consequences for the way the corporation thinks about research. That is why . . . R&D suddenly moves forward in the corporation’s thinking.
>
> In the case of, say commodity chemicals, polystyrene, you don’t spend an enormous percentage of your sales on research. You have to spend a reasonable amount, but not 5 or 6 percent. Certainly not 10 percent! An awful lot of money goes into building a plant, and you’re spending a lot on your cement in the ground. So the big decisions are capital decisions.
>
> Now the really big decisions are R&D decisions. You’re going to see this not in Monsanto alone, but in other companies too. ’2

The use of committees and other formal means of communication at Monsanto, DuPont, and Kodak is not at all coincidental. What Schneiderman, Fortney, Hanson and others have learned is that R&D budgeting is “an information and communication process. ” While it is true that each company allocates its resources differently, several generalizations about the budget process can be made. After reviewing the literature from the 1970s, Winkofsky, et al., formulated and substantiated these observations:

- the processes are multiperson, involving many persons throughout the organizational hierarchy;
- the processes are multilevel, involving organizational entities at different hierarchies;

[40] Michael F. Wolff, “An Interview With Robert C. Fortney,” *Research Management,* January/ February 1984, p. 16.

[41] Ibid., p. 17.

[42] David Webber, “Chief Scientist Schneiderman Monsanto's Love Affair With R& D,” *Chemical & Engineering News,* Dec. 24, 1984, p. 11.

- the processes are iterative;
- proposals are passed upward through the hierarchy;
- resource allocations are passed downward through the hierarchy;
- goals in implicit or explicit forms are passed downward through the hierarchy;
- the processes are multicriterion in nature;
- there is no unique set of criteria used by all firms;
- areas of concern which are considered by many firms include: R&D costs and the probability of technical success, manufacturing costs and the probability of commercial success, market potential and the probability of market success, and contribution to corporate goals;
- different levels of the hierarchy may use different evaluative criteria; and
- the R&D budgeting process may be periodic, continuous, or periodic-continuous.[43]

This list contains no mention of the use of financial models. Winkofsky, et al., believe that the complexity of the allocation process "often confounds the formulation of mathematical models." Because different levels of the hierarchy may use different evaluative criteria, there is no firm base on which a financial model can be built and utilized effectively by all parties involved in the decisionmaking process.

Managers and scholars now approach budgeting as a process that relies on shared information and communication for its success; thus, it cannot be easily reduced to mathematical formulae. Despite the plethora of financial and technological forecasting models that have been introduced in the last 15 years, managers have been reluctant to replace qualitative measures with strictly quantitative ones. Most managers use a combination of qualitative and quantitative techniques, depending on the stage of research of the project. Models are used mainly to explore policy alternatives. Qualitative evaluation techniques work best at the level of basic research. A mixture of quantitative and semi-quantitative techniques works well at the level of applied research. Reliance on strictly quantitative techniques greatly increases when a project enters the last stages of product development.

Many executives shy away from using formal analytical methods because the data generated by these models often do not reflect assumptions that are shared throughout the organization. For example, Robert L. Bergen, Jr,, manager of corporate R&D at Uniroyal, is "leery about individuals becoming so committed to a number that they will find it hard to reassess a project later on, adding that there is always a problem when the boss ranks things one way and a subordinate ranks things another."[44]

The degree to which executives and their subordinates' views are commensurate reflects the level of communication between individuals and divisions within the corporation. Most of the literature in the past 5 years points to gaps in communication as the most important difficulty that has to be overcome if strategy and technology are to be linked. New corporate efforts to mesh strategy and technology are occurring at a time when scientific and technological information is growing exponentially. Management wants information from corporate scientists and engineers and to share this information with representatives from the marketing and manufacturing divisions as often as possible.

[43] Winkofsky, et al., op. cit., pp. 185-187.

[44] Michael F. Wolff, "Selecting R&D Projects at Uniroyal, *Research Management,* November 1980, p. 8