From Uncertainty to Risk: Reducing the Guesswork in R&D

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The R&D imperative for industry has never been more compelling. Virtually all of industry has felt the impact of increased competition, much of it technically based, as well as the accelerated pace of technological challenge and change. The cumulative R&D expenditures and output of competition grows apace. No single company can grow its R&D investment at a rate commensurate with the sum of the international R&D forces growing in competition with it. The answer to the imperative cannot therefore be simply to invest more in R&D. „More“ can never be enough. Instead, the answer for a growing number of companies is to deploy R&D investments more effectively, i.e., more strategically.

Decisions that determine what research a company will undertake, and with what level of priority, are coming to constitute some of the most complex and critical decisions general management faces. Note the key role of general management. The new imperative might be characterized by the paraphrase, „Research directions are too important to be left to researchers.“ Increasingly, managers recognize that the most decisive factor in overall research success is the selection of strategically worthwhile R&D goals. For many executives, however, uncertainty is highest at this most intimidating stage of R&D decision making.

One of the difficulties is that managers often lack a strong research background, while the R&D staff lacks the skills to communicate comprehensibly and credibly with managers. Communication between general management and R&D is often weakest when it needs to be the most effective – at the R&D strategic planning and decision-making phase. In its own planning, R&D commonly reasons from vision and scientific judgment; it offers management plans that are enveloped in technical jargon, hope, and uncertainty. Management reasons in terms of risk and reward and, lacking R&D's technical insight, finds little common ground.

Risk and Uncertainty

Since risk and uncertainty play distinct roles in the success of a technical project and since managers often confuse the two terms, a definition is in order.

Donald A. Schon says it well: „Risk has its place in a calculus of probabilities. It lends itself to quantitative expression – as when we say that the chances of finding a defective part in a batch are two out of 100. In the framework of benefit-cost analysis, the risk of an innovation is how much we stand to lose if we fail, multiplied by the probability of failure.

„Uncertainty is quite another matter: a situation is uncertain when it requires action but resists analysis of risks. For example, a gambler takes a risk in an honest game of blackjack when, knowing the odds, he calls for another card. But the same gambler, unsure of the odds or unsure of the honesty of the game, is in a situation of uncertainty.“

Schon relates the difference in meaning between risk and uncertainty to research planning and technical innovation: „Men involved in technical innovation in a corporation confront a situation in which the need for action is clear but the action itself is not.... So long as this situation exists, the corporation cannot function effectively, because it is not designed for uncertainty – a situation in which there are no clear objectives to reach, no measures of accomplishment, and no proper concept of control. A corporation cannot operate in uncertainty, but it is beautifully equipped to handle risk. It is precisely an organization designed to uncover, analyze, evaluate, and operate on risks. Accordingly, the innovative work of a corporation consists in converting uncertainty to risk.“1

The last sentence presents a provocative idea, but how does the process work? My aim in this article is to describe a method for transforming uncertainty into risk in long-term research plans. The research I am referring to involves at least some extension of the state of the art and some uncovering of new knowledge, because it is here that uncertainty reigns. I exclude research in which existing scientific and engineering principles and knowledge are applied to product or process design. A new clock radio, a new decorative pattern on a floor tile, a new proprietary drug, or a new disposable diaper requires creative design by skilled people, but no fundamental discoveries. Accordingly, companies can predict the course, timing, and cost of developing these kinds of products and processes through applied research and technology and subject these predictions to quantitative risk analysis. (For the same reason, competitors who are also fluent in the relevant scientific and engineering principles can easily and quickly imitate these products.)

Original research, on the other hand, reaches into the unknown, and the unknown is haunted by uncertainty. The deeper the reach, the more humbling the uncertainty. Who knew what was on the lunar landscape before the first space explorers ventured there?

Technologically innovative companies commit resources to the difficult task of exploring the unknown because success can give them knowledge that is unique, at least for a time. They can translate their advantage into products or processes that competitors cannot quickly imitate.

For management, the central question in planning research is how to arrive at an understanding of the technical uncertainty in R&D planning and how to transform uncertainty into risk. While there are no sure methods, there are principles that can help materially.

An Agrichemical Example

Consider a major chemical company that has an important position in the production of fertilizers and agricultural chemicals. It has R&D skills in plant physiology, entomology, botany, chemistry, biochemistry, molecular biology, and engineering. The company's long-range plans include an important new development in plant nutrition.

Its laboratory R&D staff has observed that a fungus growing on the roots of young corn predisposes the roots to accept certain soil bacteria (rhizobia). These bacteria form nodules on the corn roots, which then „fix" nitrogen from air, a process that occurs naturally in leguminous crops like soybeans but not in corn. They then convert the fixed nitrogen into ammonia, which the corn assimilates for nutrition.

In principle, a nitrogen-fixation process could replace the application of chemical forms of nitrogen – ammonia, nitrates, and urea – with vast economic and social benefit. Unfortunately, the symbiotic relationship between the fungus and the corn exists only when the corn is less than four weeks old, or until its defense mechanisms are sufficiently mature to repel the fungus. Scientists at several universities have studied the biochemical defense mechanism, so it is known in general terms.

The company's R&D staff had observed that a class of chemicals being tested as potential herbicides prolonged the life of the fungus and the associated nitrogen-fixing bacteria by several weeks, apparently by intervening in the biochemistry of the corn's defense. The staff developed the theory that it might be possible to design a chemical to intercede in the rejection process, enabling the bacteria to fix the nitrogen for the full lifetime of the corn. If this worked, and if the chemical were safe and economical, the benefit to worldwide agriculture and to the company could be profound. The concept of replacing 150 to 300 pounds of nitrogenous fertilizer per acre with a small amount of chemical is the stuff of which commercial dreams are made.

Many uncertainties, however, surrounded this idea. Was the description of the rejection process in the published literature accurate? Could it form the basis for design of the chemical? If the answer to these questions was yes, would the agricultural chemical be effective at low and economical concentrations? Would it be toxicologically safe? Could it be designed to be absorbed by plant leaves or roots, and would it then make its way to the site of rejection unaltered by the plant? Would it be patentable? Were there altemative, surer approaches to the problem that the company or its competitors might take?

In discussions within R&D and between R&D and management, both groups became optimistic that the answers to all the questions might be favorable. It was agreed that a successful result would consist of a chemical or class of chemicals, exclusively proprietary to the company, toxicologically safe, effective at application rates of one to two pounds an acre, costing $3 to $5 a pound to manufacture and producing a value to the farmer of $20 to $40 per acre based on the nitrogenous fertilizers it would replace.

Heady stuff. But the estimated costs of reaching such a goal were high, certainly in excess of $10 million and perhaps, but only perhaps, less than $100 million. Faced with such expenditures, management sought assurance that the money invested would be spent effectively and asked if, given the support, the R&D division could commit itself to a successful result, as just defined. R&D answered, not surprisingly, certainly not. Could it commit itself to a 50 percent probability of success? Twenty-five percent? Given all the uncertainties, no. Did R&D think the project should be initiated? Yes. Small wonder that management now reached collectively for the aspirin bottle. It was being asked to fund an expensive, open-ended project more on faith than on insight.

It is possible, however, to convert R&D uncertainties to risks, thus providing managers a rational basis for decision making. The project in this example, like all projects in original research, rests on a foundation of assumptions, postulates, or theories whose correctness is critical to success. Each of these factors has its own uncertainty, and each can be converted into risk expressed in probabilistic terms.

Converting Uncertainty to Risk

In the case of the proposed new chemical, this conversion of uncertainty to risk might work as follows:

1. Was the fungus rejection mechanism, vital to the design of particular chemical agents, accurate as described in the literature? The R&D scientists did not know; the evidence was sound but incomplete. They could not assert that it was correct and sufficient or offer a credible probability estimate of its correctness. They were confident, however, at a probability of 80 percent, that a team composed of a plant physiologist, a biochemist, and a chemist could design experiments that would conclusively test the rejection theory in 12 to 15 months at a project cost of $400,000 to $500,000. They would commit themselves to a high probability of a conclusive test of the theory underlying the commercial concept, but would not promise that the test would confirm the theory.

2. If the rejection theory proved sound, could R&D guarantee effective application of the theory in the design of a chemical agent to intervene in the rejection process for the lifetime of the corn? No. Given the clues from their work on herbicides, however, R&D staff could commit itself to determining whether the class of materials already identified as somewhat active could be modified to augment and prolong effectiveness. The probability estimate was 75 percent for a conclusive determination, with a staff comprising a plant physiologist, a biochemist, and three organic chemists for 15 to 18 months, at a cost of $600,000 to $700,000. Note that a conclusive determination of the soundness of a chemical approach was given a high probability, but success in finding a chemical was not.

$o far, two major uncertainties have been transformed into risks with probabilities attached.

3. If the chemical approach proved sound, would R&D commit itself to develop a chemical that was effective at low and economical concentrations? No. If the theory is confirmed, however, experience with potential herbicides strongly suggests that low concentrations might be effective. R&D will commit itself to a probability of 90 percent to 100 percent to determine whether low concentrations prove effective. If the test of effectiveness at low concentrations is positive, R&D is confident that the company's synthesis and manufacturing skills can provide the chemicals at satisfactory cost.

4. Will R&D make a commitment that the chemical it finds effective will be toxicologically safe? No. However, the potential herbicides had very low orders of mammalian toxicity. R&D will commit itself, at 100 percent probability and at modest cost, to an early determination of toxicity and to abandoning the approach if toxicity appears unmanageable.

5. Will R&D offer a probability estimate that the chemical found effective at low concentrations will be absorbed by the plant through leaf or root mechanisms? Yes. The class of compounds that are under study have an 80 percent probability of good absorption rates, and R&D staff feels confident in its ability to alter the chemical's absorption characteristics, if necessary.

6. Will the effective chemical be patentable? Not yet knowing what it is, R&D cannot answer this question. It is highly probable, however, that the process of application to corn will be patentable.

7. Are there alternative, less risky approaches that the company or its competitors might take to this problem? R&D scientists, with the help of academic and industrial consultants, considered genetic modifications to the corn to eliminate the rejection process, and to the fungus to confer immunity to rejection. They concluded that genetic modifications were far more treacherous and uncertain than a chemical approach.

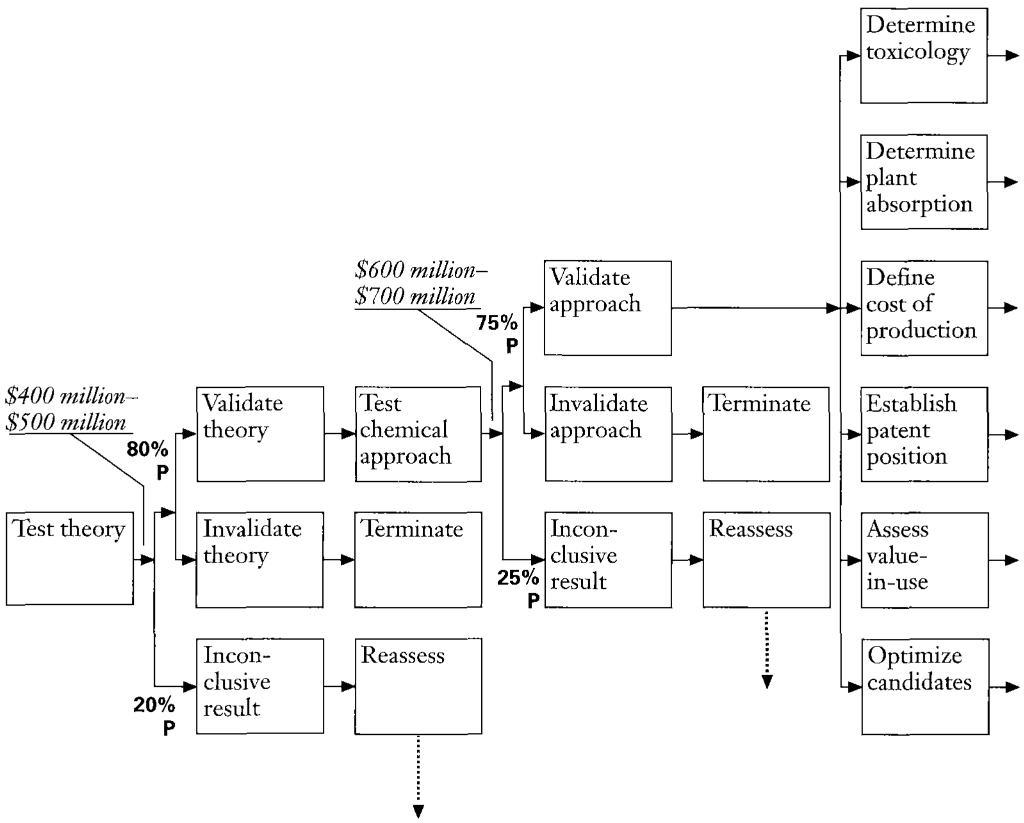
Practical Consequences

Let us examine how these questions and their answers will affect management in practice. In a typical case of original research, R&D is likely to offer management a statement of a research objective as presented, such as „a chemical or class of chemicals" that would have great commercial benefit to the company. But the project to attain this objective is riddled with uncertainty. By the analytical process just described, the uncertainties can be reconceptualized as manageable risks. Now the decision management faces is not whether to fund a highly uncertain, open-ended program, but whether to fund one phase of a structured program, with a high probability of reaching a definitive conclusion. The decision whether to fund Phase 1, then Phase 2, and then Phase *n*, is manageable. A diagram of the stages and costs of the risk analysis for the agrichemical example is shown in Exhibit 1.

Consider the process from a different point of view. Research can be described in terms of technical difficulty. If the task is well within the state of the art, the technical difficulty is modest. When it exceeds the state of the art, then technical difficulty soars. Donald Schon represented the relationship as a function of time, as in Exhibit 2.

Exhibit 1

Risk Analysis, Agrichemical Case



To make the project represented in Exhibit 2 succeed, the investigation must accomplish two principal tasks, which we can call A and B. Task B extends beyond the state of the art and is therefore highly uncertain, while task A is within it and has a high probability of accomplishment, given time and support.

We can apply this concept of technical difficulty to the agrichemical example. R&D cannot promise a high probability of being able to design a chemical agent that will be effective at low concentrations, toxicologically safe, readily absorbed by the plant, and able to make its way unaltered to the appropriate site. R&D staff knows that all those criteria cannot be satisfied at position A in Exhibit 2 but only in the shadowy reaches around position B.

R&D can, however, offer a rational, probabilistic estimate of success in determining whether a chemical approach is sound because a scientific test of this is within the state of the art. In Exhibit 3, determining whether the approach is sound is represented by point A; whether it is effective at low concentrations, B; toxicologically safe, C; readily absorbed, D; and transported unaltered to the correct site, E. All these determinations are well within the state of current knowledge.

Decisions on each step in the project can therefore be based on credible analyses of risk. Uncertainty has only been disguised, of course. It underlies the entire project, in the possibility of obtaining the desired experimental results; in whether the theory of intervention in the defense mechanism is not only definitive but correct; in rigorously determining that the chemical approach is both right and feasible; and in finding that the absorption, toxicity, and transport characteristics are favorable.

Nevertheless, applying risk analysis to even extremely uncertain programs provides a rational mechanism by which to make key decisions sequentially. Management will (or won't) appropriate the time of three scientists and $400,000 to $500,000 to complete Task A in 12 to 15 months. And so on. Instead of a commitment to a large but unknown open-ended expenditure, executives have manageable bites on their plates. R&D can make probabilistic commitments because success has been defined in terms that are within its capabilities to account for the results. The process illustrated in the agrichemical example can be stated in a series of simple tasks:

1. Define the research objective in commercial and technical terms, specifying all the successful technical steps required for commercialization.

2. By a series of approximations, estimate the potential commercial reward.

3. Redefine all the major technical steps in terms of the assumptions, postulates, or theories that must be found valid for each to succeed.

4. Determine whether the validity of these assump tions, postulates, or theories can be tested within the state of the art; redefine any that are found untestable.

5. Estimate the probability of conducting a conclusive test of each assumption, postulate, or theory with the desired level of research resources.

6. Put the various tests in a logical and chronological sequence.

7. Forecast the cost and time required for completion of crucial tests.

8. Decide whether the risk-reward-time strategic relationships justify the project.

Exhibit 2

Project Difficulty in Relation to the State of the Art

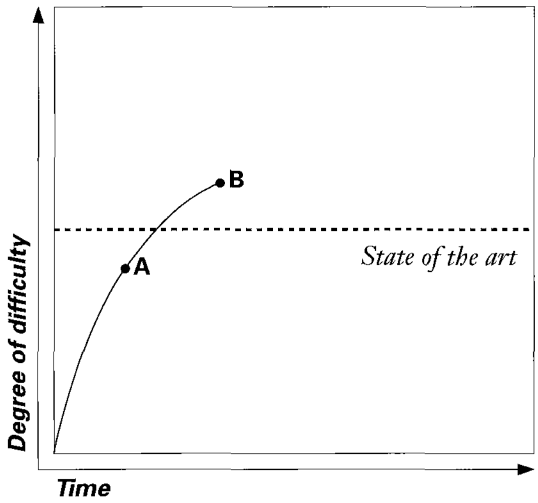
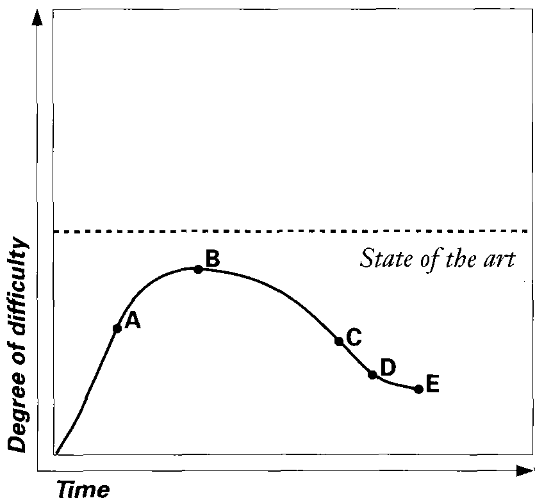


Exhibit 3

Project Difficulty in Relation to Technical Tests



Fortunately, project costs such as those in the agrichemical example are generally lowest when uncertainty is highest, one of the rare cases in which the laws of nature and economics balance difficulty in one aspect with gentleness in another.

My colleagues and I have applied these principles in real-world R&D planning and found that both nontechnical senior managements and R&D staff readily grasped and trusted them. The resolution of uncertainties in research – the uncertainty-to-risk conversion – takes time and money and requires justification in its own right. Its benefits must be balanced against its costs. In the general case of original research, it is often prudent to use this process.

[1] *Donald A. Schon, „The Fear of Innovation,'' *International Science and Technology*, November 1966. Reprinted in* Uncertainty in Research, Management, and New Product Development, R.M. Hainer, *S. Kingsbury, and D.B. Gleicher, eds., (New York: Reinhold, 19 67), p. 12.*

[2] *Ibid.*

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