R&D PROJECT SELECTION vs. R&D PROJECT GENERATION

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ABSTRACT

Much of the technical literature emphasizes R&D project selection. This paper reviews the author's experience with R&D project selection. In our view, this experience suggests that the emphasis should be on the generation of high quality R&D projects through effective communication of corporate priorities, implementation issues and related technical efforts.

1. PROJECT SELECTION

There has been considerable work on systems for selecting R&D projects. Such approaches generally evaluate existing project in order to recommend supporting the best projects and downscaling the rest. Thus the decision analytic approach[1] toward R&D project selection(Matheson, Menke & Derby,1989; Krumm & Rolle,1992; Buede & Bresnick,1992; Burnett,Monetta & Silverman,1993) first evaluates R&D projects by

(1) Quantifying the probability of a project being technically and commercially successful (using subjective estimates from the technologists and the potential implementers)
(2) Quantifying the benefits of the project if it is successful (based on estimates from the financial and marketing staffs)

and multiplying probability and benefits to get an overall expected net present value (after subtracting off the costs of developing and

implementing the project). It then targets those projects with both a low overall probability of successful and minimal benefits given success for modification or termination.   Of course, this represents only one of many approaches toward project-selection.

But the project-selection approach toward R&D management has not gone without criticism. After reviewing hundreds of articles on R&D

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[1]In 1992, the Strategic Decisions Group---a consulting company specializing in decision analysis---surveyed 200 R&D and business executives, interviewed 22 organizations, inferred 45 best practices for R&D and tested these inferences with 100 R&D executives at a two-day workshop and with a joint survey with the IRI Quality Director's Network in 1993. Many of these 45 best practices were inherent in a decision analytic approach to R&D project selection.

project selection(Cetron, Martino & Roepke,1967; Souder,1978), Schmidt & Freeland(1992) observed:

> Project-selection has traditionally been formulated as a constrained optimization problem...Benefit measurement models are often used as inputs to the optimization model...These models focus on a decision that is made at a particular organizational level at a particular point in time. They assume fixed criteria (obtained from above) and alternatives (obtained from below) and have no mechanism for altering the problem(i.e., obtaining new criteria, objectives, alternatives) within the planning cycle. Classical R&D project-selection models have been virtually ignored by industry...

What's going wrong with project-selection?   One perspective is suggested by Mitroff(1986) who wrote:

> …there are various kinds of problems. The easiest are those for which the various means are already known. The problem then is usually to find and then to pick which means or potential solution is the most effective... The most difficult and thorny problems are usually messy, ill-structured or ill-formed...The basic means (solutions) are not clear or clearly known...

In other words, project-selection algorithms may only be fully adequate solutions to the easiest kinds of R&D management problem.

This finding --- that project-selection is only a part of the solution to effective R&D management --- is not surprising. Indeed Ransles(1994) reviewed several studies on `best practices' in R&D, all of which indicated that project-selection is only one of the key factors associated with good R&D management.

To provide some further perspective on the issue, this paper describes the author's experiences with the application of the decision-analytic project-selection system to the management of GM's R&D projects. In the course of this application, we quickly discovered that the main benefits of the project-selection system were not in discovering the best projects to fund but in stimulating researchers to develop better projects. This finding is consistent with Souder and Mandakovic's observation that "today, there is a growing recognition that project-selection models should be used to ask questions of the entire organization" and that project-selection models are "decision aids" to facilitate communication.

2. COLLECTING DATA FOR PROJECT SELECTION

The author & his team were asked to use a Matheson-like decision-analytic approach to evaluate fifty research & development projects. While traditional decision analyses generally required several weeks (or months) per project, our team was mandated to complete the review in two weeks.   This required us to dramatically streamline the traditional decision-analytic approach. Because our DDP (GM's term for decision analysis) was so accelerated and hectic, our exercise became known as

``*THE DDP FROM HELL!*''

To implement the decision-analytic approach,we needed to collect information from the leaders of the fifty technical projects (i.e., the technologists), their organizational partners interested in potential implementing the project (i.e., the implementers) and individuals familiar with corporate priorities. To gather this information, we first called a two-hour meeting of the technologists and implementers for each project.   The number of individuals involved in an interview ranged from two to six.   A facilitator, generally the author, then gathered some of the required information by asking this group the following questions:

(1)-What is the Likelihood of the Project being Technically
   Successful?
   (i.e., What is the likelihood of your achieving your stated
        deliverables at the deadlines at which they were promised?)
   This question, generally answered by the technologist, called for an
   explicit statement of the project's technical targets (deliverables) and
   deadlines.The technologist was then asked to think through `what has
   to happen to meet these targets.'   After thinking about these targets,
   the technologist was then asked to think about his most challenging
   technical hurdle & then less challenging technical hurdles. Based on
   this reflection, he was then asked to subjectively assess the
   probability, $p(T)$, of successfully meeting his targets --- given his
   resource requests --- by the deadlines stated. (We also collected
   information on the resources,c_T, required to do the project.)

EXAMPLE: Consider a project---which we will call the Fuel Project---aimed at developing a new way of burning fuel. The technical goal of the project is to demonstrate a 0.3% increase in fuel economy using this new technique in 3 years. The two main obstacles to achieving this goal are:
 (1) an inability to achieve precise control of the vehicle's combustion,
 (2) the new process leading to unacceptably high pollution levels.
The probability of overcoming these two hurdles in 3 years is 0.3.
The research effort will require the full-time use of four scientists over these three years at an estimated total cost of three million dollars.

(2) If the Project were Technically Successful, Are the Results Likely

to be Used?
(i.e., if the project's deliverables were available at their promised deadlines, would the internal[2] customer actually use them?)
This question, generally answered by the implementer, required the implementer to
(a) clearly visualize the deliverable being promised by the technologist,
(b) think about all the issues that might prevent implementation
(c) think about the systems on which the technology might be implemented
(d) estimate the probability, $p(I|T)$, of implementation given that the technology is technically successful.
(e) estimate the costs, c_I, of implementing the technology

This exercise forces the implementer to clearly think about what he would do if the technology were actually developed. This imposes discipline on those implementers--- who might have originally supported the technology because it was interesting and because it was `free' --- with no serious commitment to implementation if the technologist were successful[3].

EXAMPLE:If the Fuel project is technically successful, then there will be an effort to implement it on all vehicles powered by conventional fuel.
There are two reasons why this implementation effort might fail:
(1) the fuel economy improvement demonstrated on a test-vehicle might not be achievable on production-vehicles,
(2) packaging the required technology might not be possible in the limited space available in a vehicle.
Given these two hurdles, the probability of successfully implementing the technology is only 0.5. When implemented, the technology will raise the cost of each vehicle by $50 per vehicle. The likely costs associated with developing and implementing this technology are thirty million dollars.

(3) If the Results of the Project are Successfully Implemented, How many External (paying) Customers are Likely to Use it?
This question asks the implementer to
(a) estimate the year of initial implementation, t_i, and the initial volume at that initial implementation, $N(t_i)$
(b) estimate how much time, t_D, will elapse between when the technology is first introduced to the market and when it reaches its maximum penetration. (We also ask for the volume, $N(t_i +t_D)$, at that point of maximum penetration.)

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[2] We also need to know whether the external customers, i.e., the real consumers of the product, will use it.
[3] In the acclaimed movie, Raiders of the Lost Arc, archaeologist Indiana Jones endures tremendous ordeals to secure the Arc of the Covenant at the request of the Government. He then becomes totally disheartened when his `implementer' ,the Government, just stockpiles it in a warehouse.

EXAMPLE: If these hurdles are successfully overcome, it will take another 2 years to engineer the technology for the first vehicle application. It will be initially deployed on 20,000 vehicles and then --- three years later ---be deployed across the entire fleet of three million vehicles

(4) What are the Key Dimensions on which your Technology Promises to Deliver Value to the Company (e.g., improved fuel economy, reduced emissions, etc.) per unit on which it is implemented? While this question seems simple, it became quite complicated when the R&D technology only contributed value as a part of a larger package of innovations. Thus an R&D project might be focused on developing cheaper sensors that would enable the implementation of an integrated chassis control system. In the absence of the sensor, the integrated chassis control system would not be introduced. This would suggest that we should attribute the entire value of an integrated chassis control system to this single sensor. Unfortunately one could make this same argument about every other critical system making up an integrated chassis control system. Mechanically summing up the benefits associated with each critical technology would then give a value exceeding the value of the whole system!

EXAMPLE: If succesful, the Fuel Project promises to improve fuel economy by 0.3% while raising piece costs by $50 per vehicle.

(5) If this Project is not Implemented, What other Technologies, if any, Will the Company Implement instead? How will this likely alternative perform on the key dimensions?
(i.e., if you're totally successful, will the results really matter?)
In many cases,neither the project leader nor the potential implementer had thought about this issue. In some cases, the project leader did have some insights based on his review of the technical literature. In other cases, the potential implementer knew of proposed alternatives suggested by other technology vendors. In this study, we only relied on the best guesses of technologists & implementers.This was later adjusted with information from the advanced purchasing group.

EXAMPLE: Suppose the company --- if it did not do the Fuel Project --- could improve fuel economy by 0.1% using a more primitive --- but technically demonstrated --- version of the technology which the Fuel Project had hoped to develop. Buying this primitive technology from an outside vendor will cost the company $40 per vehicle. Since an outside vendor is supplying it, there are no research or development costs.

3. SIDE-EFFECTS OF COLLECTING DATA

While we did successfully develop numerical estimates of the value of each project, we found that the actual process of interviewing individuals had some pronounced side-effects, e.g.,

(1) In some cases, projects did have well-established customers who followed their work with interest. But the projects did not have any well-established deliverables or deadlines. Fortunately the process of interviewing researchers and their internal customers about the project's value led the researchers and customers to rethink the project and generate focused deliverables and deadlines.

(2) In some cases, technologists hadn't really thought about all the potential showstoppers to their projects. This often led them to avoid confronting the ``showstoppers'' --- the real tough technical issues which might cause the project to fail ---and focus on resolving the easier technical issues. As a result, the technologist only confronted the ``showstoppers'' late in the history of the project --- after having invested a considerable amount of work. By focusing on ``showstoppers'', we encouraged the technologist to attack the tough issues early so that he could fail early --- rather than later.

(3) By forcing the implementer to think through implementation hurdles in the presence of the technologist, we sensitized the researcher to implementation concerns and, in some cases, caused the actual project to change (or even to be withdrawn). Thus a number of project leaders had ignored cost & quality, feeling that those issues were `continuous-improvement' issues best left to the implementers. Once the implementers indicated that high cost or low-quality was an obstacle to implementation, some project leaders began adjusting their projects to take on these added concerns.

(4) Similarly, sometimes implementers could only implement new innovations if they were available by certain deadlines. In the past, many technologists --- faced with shrinking resources---tended to work on many different projects, resulting in much longer times of completion for each individual project. Once technologists became sensitive to the implementer's deadlines, they could readjust their work schedules --- in some cases, focusing their efforts on only one or two projects --- so that they didn't waste time working on projects which wouldn't meet those deadlines.

(5) In some cases, technologists were working feverishly on a project whose success would help enable some major innovation. But unfortunately the innovation was actually contingent on the success of several projects in addition to the one being worked on by the technologist. In some cases, these other projects were understaffed.

Hence even if the technologist was completely successful with his project, the probability of its being implemented, i.e., the probability of the major innovation being implemented, remained fairly low. This led to the notion of the `value-chain'. In other words, one must define the project to include the major innovation being enabled and all the critical enablers and then systematically ensure that adequate resources were allocated to every critical enabler.

(6) In some cases, researchers had mistakenly thought that executive management assigned high value to certain attributes. (This reflected the fact that sometimes a scientist's main input about corporate priorities came from press releases.) The realization that these attributes were not high value caused some dramatic rethinking. In one particular poignant case, a scientist found that his project was considerably less valuable than the project of a colleague --- with similar skills and background--- in a neighboring office. He was amazed that a minor change in how he had targeted his work could have dramatically impacted its value.

(7) In some cases, researchers had not investigated whether or not the company already had a better alternative to the proposed results of their research. The researchers voluntarily withdrew their projects once they found that the company already had superior options.

In fact, the amount of qualitative insights that came out of the interviews was fairly astonishing. While the intent of the interviews was originally to assess some numbers to evaluate projects, it actually stimulated technologists into reworking projects with low implementation probabilities or low commercial value. Thus the very act of assessing the value of projects caused the projects --- and their value, to change --- in a sort of managerial counterpart of the `Heisenberg-Uncertainty-Principle'[4].

Indeed, as we soon found, this `side-effect' was, in fact, one of the main values achieved by our work[5].

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[4] The Heisenberg Uncertainty Principle in quantum physics mandates that the way in which we choose to observe the state of a quantum mechanical system inevitably changes the state of a system. Hence it is impossible to `know' the state of a quantum mechanical system, we can only know what that state would be if we observed it in a certain way.

[5] This was vaguely reminiscent of the children's story, STONE SOUP where three soldiers enter a village of uncommunicative hermits and indicate they plan to make a soup out of a stone. The somewhat curious villagers volunteer a pot, fire, water and a stone to make the soup. In the process of making the soup, the soldiers comment on how the taste of the soup would be improved with the addition of a few extra ingredients. This persuades some of the villagers to unlock their secret hoards of food and dump it in the soup to be shared with the rest of the community. When all the villagers have added their own private stores of food to the soup, the soldiers remove the soup and then share the stew with the villagers. Thus while the process of boiling the stone in a pot was of no value, it precipitated the sharing of food that led to a major feast. Similarly, while our NPV assessments would prove to be of some value, a far greater value was achieved by the dialogue they precipitated.

4. COMPUTING NET PRESENT VALUE

Estimating Potential Impact

   If all the value associated with the project were delivered at a single point in time, defining a single project discount factor would be trivial. Unfortunately a project delivers value over several years. Thus before defining a project discount factor, we need to define an alternate project which is implemented --- on some volume N --- in a single year and is not implemented in any later years. We must then choose N so that the net present value of this alternate project is the same as the net present value of the actual project.   In this paper, we will also assume that this alternate project is implemented in year t_I, the first year in which the actual project is implemented.

   Since our project interviews told us t_i --- the time at which the project would be first implemented --- we could use the corporate discount rate of R to compute a discount factor associated with the value of the benefits of the project at its first year of implementation, namely exp($-R\ t_i$).

   The project interviews also gave us

(a) $N(t_i)$--- the number of production units in which the technology is incorporated in the initial year of implementation,

(b) t_D --- the time until the initially introduced technology would be `ramped' up to its maximum volume--- and

(c) $N(t_i + t_D)$ --- the number of production units in which the technology is incorporated when the project reaches its maximum penetration.

We now assume that the number of units per year affected at time t (or $N(t)$) starts out as equal to $N(t_i)$ at time t_i and then increases linearly up to time t_i+t_D. At time t_i+t_D, $N(t)$ is assumed to remain constant at $N(t_i+t_D)$ units per year in perpetuity.   We then computed an effective total volume

$$(1) \qquad N = {}_{t(i)}\!\int^{t(i)+t(D)} N(t)\ exp(-R(t-t_i)\ )\ dt$$

and found, upon doing the integral, that

$$(2) \qquad N = ((1-s)N(t_i) + s\ N(t_i + t_D))/R$$

where

$$(3) \qquad s = (1-exp(-Rt_D))/Rt_D$$

measures how fast the innovation, once implemented at a small initial volume, is deployed across the rest of the company.

   Thus a typical project aimed at developing a new product feature might be initially deployed on 250,000 vehicles with an ultimate maximum penetration of five million vehicles. If the corporate discount factor is 15% and if the time to rollout the product, t_D, is about 6.67 years, then s equals .63 and the effective volume is 21 million units. On the other

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hand, if we have a faster rollout with t_D=3.33 years, then s=.79 and the effective volume is 26 million units.

EXAMPLE: The Fuel Project will be initially deployed on 20,000 units and then ramped up --- over the course of 3 years --- to the entire fleet of three million vehicles. For a discount factor of 20%, N equals
.02 + (.8)(1.02) + (.64)(2.02)+(.51)(3.00)+.4(3.00)+.32(3.00)
+ (.25)(3.00) + (.20)(3.00)+(.16)(3.00) + (1.3)(3.00)+...
or 9.5 million vehicles.

Estimating the Value of that Impact
     To gather enough information to assess a project's value --- following the paradigm of Matheson et al --- we need to ask one more question:

(6) What commercial value is associated with the benefits of this project, if it were technically successful and were successfully implemented?
     But this was not a question which either the project leader or the potential implementer could easily estimate since it implicitly requires some knowledge of the priority the corporation attached to various potential improvements. (And indeed, the fact that few people really knew the corporate priorities for fuel economy improvements versus cost-reduction versus lead-time reduction was, in itself, an important revelation[6].)
     Estimating these implicit values for various attributes was not trivial since marketing research had historically focused on estimating customer demand for attributes in the short & mid-term, not over the long-term time horizons associated with R&D. Nonetheless we did develop reasonable approaches toward estimating these quantities for many attributes. To illustrate our approach, we now discuss four examples:
(a) Fuel Economy :To assign a value to fuel economy, we estimated how much an average individual's refueling costs would decrease given a mile per gallon increase in fuel economy. If a driver refuels his car once a week, at an average cost of $15 per refueling, then his annual fuel costs will be about $800. If his current fuel economy is 25 miles per gallon, then a one mile per gallon increase in fuel economy reduces his annual fuel costs by 4% of $800 or $32 a year which--- discounted using a 15% discount factor --- leads to a total fuel savings per car of about $200 for each mile per gallon improvement. (A societal estimate of this importance might use a lower discount factor, say 5%, and might value each gallon of fuel economy at the world price.This would increase the importance estimate by almost an order of magnitude.)
     We then asked executives to adjust this estimate to reflect the added strategic priorities associated with fuel economy. Later clinic

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[6]As one individual put it, ``we're guns for hire but you have to tell us which way to shoot."

data was used to further refine this estimate.
(b) <u>Emissions</u>: To assign a value to emissions, we estimated the current cost of meeting anticipated emissions regulations and interpreted any new emissions technology as reducing the cost of meeting those regulations
(c) <u>Lead-time Reduction</u>: To assign a value to lead-time reduction, we identified the manufacturing processes on the critical path and then estimated the value associated with shortening overall vehicle lead-time by a day. To develop a benchmark estimate of this quantity, we estimated the total amount of engineering hours associated with one day of product design and engineering.
(d) <u>Cost Reduction</u>: We assumed that the value of cost reduction was just the net present value of the amount of cost reduction

We used this approach on many other dimensions of strategic value. Since we had already assessed a baseline for how much the company might expect to improve on each dimension --- in the absence of the project --- we can now estimate the incremental value of this technology per unit upon which it is implemented --- assuming it is completely successful. To do so, we first assumed --- consistent with many other studies(Jenni, Merkhofer & Williams,1995) that overall value is additive in the value contributed on each dimension. Given this assumption,
(1) let A(j) be how much the technology improves on attribute j per vehicle. Let $A^0(j)$ be the improvement on attribute j offered by existing proven technology.
(2) Let w(j) be the weight assigned to incremental improvements on attribute j (measured in units of money per units of that attribute.) Thus if attribute 1 were miles per gallon improvement in fuel economy, then w(1) might equal $200 per mile per gallon. If attribute 2 were cost reduction, then w(2) might equal $1 per one dollar reduction in cost.

Then the incremental value of this technology per unit on which it is implemented is given by

$$(4) \quad V = \sum_j w(j)(A(j)-A^0(j))$$

EXAMPLE: The Fuel project promises an improved fuel economy of 0.3%. Since the average fuel economy is about 27 miles per gallon, this gives us an average improvement of 0.81 miles per gallon. Since a mile per gallon improvement is worth $200, this improvement is worth $162 per vehicle. Since it will cost $50 per vehicle, the overall value is $112 per vehicle.

The Overall NPV Measure
Given these estimates, the overall NPV assigned each project was simply[7]

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[7] Recall that N is the total volume over all the years of the project, discounted to the year of initial implementation, t_i. Since the year of initial implementation is, by definition, t_i , years from the present, we

$$(5) \quad p(T)p(I|T) \exp(-Rt_i)\ N\ V - p(T)c_I - c_T$$

While somewhat crude, this NPV measure was fairly easy to explain. It indicates that project value is inversely proportional to project risk, implementation uncertainty and capital costs and directly proportional to the number of corporate units impacted and the strategic value of the benefit delivered to those units. Because of its simplicity, the criterion was easily communicated and thus had a very pronounced effect on the research community[8].

EXAMPLE: The overall NPV value for our Fuel Project was

(.30)(.5)(exp(-.15(3)) (9.5 million)($112) - .3($ 30 million)-($ 3 million)

or

$90 million-$9 million-$ 3 million = $78 million

This NPV must be compared with the NPV associated with using the outside vendor's technology.

5. Evaluating Projects Based on the Data Collected

At this point, we did explore using the NPV to determine which projects should be resourced and which ones not resourced. But because the NPV index was such a simple function, researchers given a low NPV would first identify whether the relatively low NPV was due to:

(1) unjustifiably high technical risk
(2) a poor chance of implementation
(3) a small potential volume (i.e., it's only usable on a few niche vehicles)
(4) high capital requirements
(5) strategic management assigning a low priority to the improvements offered by the project
(6) unduly slow ramp-up of the technology
(7) existence of superior alternatives to the technology
etc.

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then multiply it by $\exp(-Rt_i)$ to discount it to the present.

[8] Indeed flyers began appearing throughout the labs which read as follows:
``NEW SCIENCE CREATES NEW DEFINITIONS:
BORDLEY: A projected unit of measurement. Research projects and proposals are frequently denominated in BORDLEYS. Projects typically range from small fractions of a BORDLEY to several BORDLEYS in size. Projects which measure several tens of BORDLEYS are quite rare, but do exist. Projects which measure less than the critical value of 1 BORDLEY are unstable and tend to disappear."

or whether it reflected errors in data collection or the omission of critical other factors (e.g., the need to sustain a competency in the area.) If they felt the low-NPV-score was legitimate, researchers would frequently rework their projects or withdraw them.

But some disputed their low-NPV scores and noted that the variables used in computing the NPV were very hard to estimate(especially given the potential incentive for researchers and customers to provide overly optimistic estimates.) These kinds of pitfalls in using NPV[9] were discussed by Roussel, Saad & Erickson(1991) who noted,

> Although some companies try to impose net present value(NPV) or discounted cash flow(DCF) calculations, the range of uncertainties for research reaching out more than a year or two is so substantial that the rigor implied by NPV or DCF considerations becomes not only meaningless but possibly harmful.

Hence we spent considerable time cross-checking the NPV estimates. Because of the uncertainty in any individual NPV estimate, we decided to fit a regression curve to the NPV estimates within each of several generic project areas (like engine fuel-efficiency, composite vehicle bodies, etc.) We could then used this regression line to understand how value changed as we varied funding within a generic project area.

To do this, we grouped projects by general areas. To estimate how the value associated with funding in each area changes with funding in the area, we first estimated the resources required to complete each project and then constructed a productivity index by dividing project net present value by required resources. If we assume that no project requires an inordinately large amount of resources, then a good rule for project selection spends all the resources available on those projects with the highest productivity index[10]. As we increase the amount of resources available, we will include, of course, more and more projects with somewhat lower values for the productivity index.

Suppose we assumed we chose projects in this way and then varied the budget. At each budget level, our rule chooses a certain set of projects with some total aggregate value. If we plot how this aggregate value changes as we change the budget level, we get a function specifying how our expected payoff from research in a general area varies as we change the amount of money we spend in the area.

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[9] These pitfalls lead some to advocate options approaches(Mitchell & Hamilton,1988) to evaluating R&D projects.

[10] An exact programming solution to this problem would require us to deviate from this rule since we cannot assume that halving the resources for a project will give us half the net present value. But when we compared this solution with a more exact integer program, we found little difference.

Such a function specifies that we get a smaller and smaller incremental payoff with every incremental budget dollar. What was somewhat surprising --- in an effect reminiscent of the experience curve--- is that the NPV for a generic project area was related to the money,x, spent on projects in that area via the exponential curve:

$$(6) \quad NPV(x) = NPV_{Max}(1-exp(-x/x_0))$$

with NPV_{Max} and x_0 being constants. The R^2 for this equation exceeded 0.95 for each of our half a dozen project areas[11] (based on a sample of more than 200 projects.)

But what was really startling was that in every area, there were
(a) some projects of extraordinarily high value
(b) some projects of minimal, or even negative, value
As a result, it was not generally optimal to transfer budgets between project areas. Indeed these results superficially suggested that one could dramatically reduce funding without changing the overall value of the R&D portfolio.   This finding was consistent with what was observed in a similar study of how to allocate the Department of Energy's budget(Jenni, Merkhofer & Williams,1995).

Instead of recommending draconian reductions in R&D funding, we spent considerable time thinking about why there was so much disparity between the values of research projects. We realized that this kind of disparity is precisely what one would expect to get from a laissez-faire `let a thousand flowers bloom' culture. But we also reflected on the Heisenberg effect in which project leaders showed they could reconfigure their projects to dramatically increase their business value. Analysis indicated that inducing a Heisenberg effect on all the low-value projects could raise the value of GM's R&D Portfolio much more than could any simple reallocation of funds from low-value projects to high-value projects.

And then we realized what our data was trying to tell us. If all researchers had been trying to optimize their projects using our criteria, then there should have been no negatively valued projects since it's obvious that such a project should not be pursued.   So obviously the project leaders had not been trying to optimize our criteria.

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[11]To help interpret this finding, suppose that all projects had identical funding requirements and that the kth most important project was some constant fraction(exp(-a)) of the value of the (k-1)st most important project. If npv_k was the value of the kth most important project and NPV_k was the total value of the k most important projects($NPV_k=npv_1 + ...+ npv_k$), then elementary algebra gives $NPV_k=npv_1(1-exp(-ak))/(1-exp(-a))$ which --- with some rearranging --- gives the exponential result noted in the text.

And in hindsight, one should never have expected them to do so. As our consultant Dan Owen---a noted pioneer of the decision-analytic approach to R&D management(Owen,1986)---observed, the results suggested that our entire project-selection strategy was fundamentally unfair. It was as if

(1) each researcher had always been told to play BASKETBALL

(2) local research management set in place a system of communication and networking optimized for BASKETBALL

and then

(3) executive management came along and suddenly graded each researcher on how well he played BASEBALL.

Our results indicated that researchers were not playing baseball that well. But then again, they hadn't been taught how to play baseball and hadn't been given a baseball field. This explained the Heisenberg effect: why researchers were able to so dramatically improve the value of their projects once they thought about the new criterion. When we started to value their projects, they learned some of the rules about how to play baseball and adjusted their behavior accordingly.

As our data indicated, raising the value of individual projects would be of much greater value than simply redeploying resources. We now realized that we needed to raise the value of all projects before implementing the alternative-focused approach. In other words, our mandate was to
(a) FIRST create an environment which would generate high-quality alternatives,
(b) THEN select among those high-quality alternatives.

6.THE VALUE-FOCUSED STRATEGY
Should this result have been surprising? In his seminal book, Keeney(1992) distinguishes between

(1) alternative-focused thinking in which ``you first figure our what alternatives are available and then choose the best of the lot.'' This is the project-selection approach.

(2) value-focused thinking which involves ``first deciding what you want and then figuring out how to get it...value-focused thinking involves starting at the best and working to make it a reality.''

As he argues(pg.6),
``with value-focused thinking, you should end up much

closer to getting all you want...The easy way out of a
decision problem is to focus narrowly on obvious alter-
natives and select one. This `solves' the problem but
a price is to be paid later when the consequences accrue.
This is alternative-focused thinking. Value-focused
thinking is more difficult and meant to be more penetrating.
There are mental costs and time associated with the
exercise, but the benefits should reward the effort as the
consequences unfold...

This value-focussed approach applied to technology strategy is very
similar to Hamel and Prahalad(1994)'s insistence that a company must
specify its strategic intent, i.e., its overall long-term direction --- as a
means of directing all the corporation's long-run technology efforts.

As a result, there was a tremendous need to tell researchers
(a) about management's strategic intent (i.e., `what constitutes winning
in baseball')
(b) about implementation issues (i.e., who their fellow team-mates were
and the positions those team-mates played)
(c) about related projects (i.e., which teams they were competing
against)
(d) about corporate technology practices (i.e., the rules of the game)
before they generated their projects.

This can --- and, in the case of GM, did --- require a reorganization in
which project leaders with similar kinds of customers --- or with similar
kinds of implementation concerns --- were organized into the same
departments. Senior research management now assumed a much
greater responsibility for interacting with implementers and strategic
management and ensuring that the needs of implementation and strategy
were consistent both with the technical projects being pursued as well as
with how those projects were executed.

In summary, the GM experience suggests that project selection --- while
critical --- should only be applied after the proper groundwork is laid.

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