Traps, Pitfalls and Snares in the Valuation of Technology

F. Peter Boer

Peter Boer is President and CEO of Tiger Scientific Inc. and a professor at Yale University. He is a former Executive Vice-president and CTO of W.R. Grace & Co. He has also held business management and technical positions at The Dow Chemical Company and the American Can Company. Dr. Boer holds an A.B. in physics from Princeton University and a Ph.D. in chemical physics from Harvard University and is a member of the National Academy of Engineering. He currently teaches a course "*The Financial Impact of Technology"* for the Industrial Research Institute, of which he is past-President, and is publishing a book on *"The Valuation of Technology".*

In my personal experiences in teaching and talking about business issues in R&D, no topic evinces keener interest from technical audiences than the financial valuation of technology. Their concern is not only that the application of financial tools to technology issues is problematical, but, when attempted, it often leads to the wrong answer, and usually to the undervaluation of technology.

This article will first review briefly why technology valuation is inescapable, and what financial methods are applied to it, and then address seven hidden traps that can lead to the wrong answer.

Why is the valuation of technology important?

First, because technology is economically important. Economists have calculated[1] that a full fifty percent of the economic growth of developed countries arises from technology. The balance is labor and capital.

Of course, the link between technology and growth extends beyond national economies to individual corporations, which are the economic units that produce most of such growth. And the link between growth and wealth is very powerful – it can be shown[2] that for an average U.S. corporation, an additional 1% more sustained growth translates to about 10% in market capitalization (a code name for stock price). Securities analysts routinely use estimates of growth rates of free cash flow[3] to make their value calculations and base their recommendations. When the perceived earnings trajectory changes big changes in stock price are sure to follow.

Thirdly, companies possessing few physical assets and rudimentary operations, but owning commanding technology, have been valued in the marketplace at hundreds of millions, or even billions of dollars – examples range from Genentech to Netscape.

Investment decisions make the need for valuation of technology inescapable. And financial analysis may be too important to leave to the financial analysts.

What is the conventional approach to valuing assets?

There is a well established textbook method[4] based on discounting future free cash flow from an investment at the rate that can be earned by alternative investments of comparable risk. The author does not quarrel with this approach. Readers unfamiliar with the method it would be advised to consult a finance text; we will summarize it concisely below to refresh as a starting point for subsequent discussion.

Briefly, **free cash flow** is the sum of net income, and, if applicable, depreciation, less capital investments required to sustain the asset. The discount rate is often also referred to as the **cost of money (C)**. Two terms are often used in association with this form of analysis – **net present value (NPV)** and **internal rate of return (IRR)**. NPV is calculated by discounting the cash flow of each successive year **(n)** by the cost of money, at a rate $(1/C)^n$. Case A in Table 1 calculates the NPV of a $1000 investment with cash flows of $300/yr for five years, assuming a discount rate of 12%. The value is $81, considerably less than the nominal profit of $500.

The internal rate of return is defined as that discount rate for which NPV = 0; in Case A it is 15.2%. Conventional financial wisdom states that this investment is sound if the cost of money is 12%, and for any other discount rate less than 15.2%.

Table 1

	Investment	-------------------Free Cash Flow				----------------------	Horizon	C	NPV(C)	IRR
Year	**0**	**1**	**2**	**3**	**4**	**5**	**Value**			
Case A	($1,000)	$300	$300	$300	$300	$300	$0	12%	$81	15.2%
Case B	($1,000)	$300	$300	$300	$300	$300	$0	30%	($269)	15.2%
Case C	($1,000)	$300	$300	$300	$300	$300	$2,500	12%	$1,348	37.8%
Case D6	($1,000)	$50	$100	$150	$200	$250	$5,297	12%	$2,184	38.5%
Case D0	($1,000)	$50	$100	$150	$200	$250	$2,500	12%	$767	25.0%
Case D3	($1,000)	$50	$100	$150	$200	$250	$3,431	12%	$1,238	30.4%
Case D9	($1,000)	$50	$100	$150	$200	$250	$10,893	12%	$5,019	53.9%

The cost of money, C, for a typical corporation is the blended after-tax cost of equity plus debt – for industrial companies whose stock generally tracks the S&P 500 it is today typically about 12%, but may be considerably higher for companies with more volatile securities such as technology firms.[5]

How does valuing technology differ from valuing ordinary physical and financial assets?

First, innovative technology is disturbingly **intangible** and is often financially invisible. Much of it is embodied in the skills, experiences, and records of scientists and engineers. Most corporations write off R&D expenditures as they occur, and so carry them at zero book value, even though it is allowable to capitalize and then depreciate them.

Secondly, a technology asset only realizes its value when it is linked to other technology assets and/or physical assets. Valuing technology is all about valuing **linkages**. As such, a mathematician would describe the relationships as non-linear, unlike the linear models used by financial analysts in developing cash flow valuations of securities or physical property analyses. The possible linkages may include scores of past, current and future developments in the technical world – owned internally, by competitors, by customers, by vendors, and by other third parties. The fate of these technologies will rise or fall on these relationships – one patent can be the difference between fortune and failure.

Thirdly, the **degree of unique risk** in the R&D marketplace, where new and innovative ideas are conceived, patented, and developed, is extraordinarily high compared to the normal degree of risk encountered in financial markets. Only in highly leveraged options markets are similar risk levels encountered, and we shall see below that the parallels with options markets are far more than superficial. Research scientists are used to working in a world where the *chances of success* for a new idea may be less than one in one thousand[6] – far different than a conventional financial investor who may consider a risk of one in ten *of failure or bankruptcy* to be totally unacceptable.

These three points are deliberately overstated for clarity. There are active marketplaces where technology is bought and sold on a tangible basis. One of these is the **licensing arena** – where intellectual property ranging from patents to fully developed engineering packages is sold commercially. Searle's aspartame patent was valued at over one billion dollars when Monsanto acquired the Nutrasweet business. Another such marketplace is in the world of **venture capital**, where experienced, risk-oriented financiers match wits valuing the intellectual property of science and technology start-ups[7] with each other and with the inventors. However, these markets are tiny in the context of the many trillions of dollars of annual transactions in global financial markets, and small even in the context of the hundreds of billions of dollars of global R&D spending.

Nonetheless, the players in these markets are very sophisticated at valorizing intellectual property assets and there is much to be learned from them, even if they play by quite different rules than do mutual fund money managers or real estate syndicators.

TRAPS, PITFALLS, AND SNARES

1. Confusing hurdle rate with discount rate

This is a common trap in technology decision making. It arises because of the misuse of a valid and powerful concept – the translation of higher degrees of risk into a higher cost of money.[8] The misuse occurs because financial analysts unfamiliar with R&D do not explicitly recognize that a central part of the R&D process is risk reduction, and that most of the investment will be made, and only made, after the key risk issues are resolved.

It can take the form "We want a 15% return on low risk projects, a 20% return on medium-risk projects, and 30% or better on high risk projects." Most long term projects are of course classified as high risk. Applying a discount rate of 30% has serious consequences. If Case A is discounted at 30% instead of 12%, its NPV becomes ($269) – a clear loser, shown in Table 1 as Case B. Furthermore, a dollar of revenue earned ten years out, discounted at 30% is worth only 7 cents. Very few long-term projects can stand discounted cash flow analysis at these rates unless the champions are willing to make outlandish income assumptions.

This approach also flies in the face of the common observation that some of the most successful products take ten or more years from invention to commercialization. Consider Nutrasweet, whose patent term of seventeen years was about to expire when the FDA approved its use as a sweetener. Yet Nutrasweet did not destroy wealth, it created it.

What are the fallacies involved?

a. After the fact, it is clear that the **economic profit** from a project will be related to the amount its rate of return (IRR) exceeds the cost of money C, times the amount invested. Simply put, if the project actually earns 17%, and the company's cost of money was 12%, the economic value added will be 5% times the investment, not –13% as a 30% discount rate would seem to require.

b. **R&D is** in large part **a process of risk reduction**. Risks can be extremely high in the early stage of a project – for example when a new molecule is synthesized to be screened for pharmaceutical activity the odds of successful commercialization may be 0.01%.

But risk is systematically lowered in each subsequent stage of a project. At some stage of development enough issues will have been resolved such that the chances of failure drop to 50/50. In even later stages, when customers have seen product prototypes and agreed to buy them, and manufacturing facilities have been thoroughly engineered the risks are yet lower – although it is at this time that the level of investment becomes very high. Finally, in the years in which profit is harvested and further investment is made to support growth, the risks will have reverted to the normal level of business risk the firm accepts. Certainly, it makes no sense to apply exceptionally higher discount rate to these later stage cash flows. In mathematical terms, it is more appropriate to handle risk using either probabilities of success at each stage of the project, or different discount rates at each project stage. In financial terms, markets have no memory, and project risk is more closely related to the current stage of development than to question of whether it once started life as a long-shot.

c. The cost-of-money concept itself begins to break down at very high rates of return – the universe of "alternative investments of comparable risk" yielding 30% per annum may not even exist. This return is beyond the typical yields of "junk bonds" or even venture capital, where skilled investors achieve returns in the low 20's by pooling very high risk investments.

One counter-argument for using a high discount rate for a high-risk R&D project cannot be lightly dismissed - it is the idea that each separate business within a corporation should be assigned its own cost of money. In other words, a (wholly) hypothetical corporation which is 50% a utility and 50% a biotechnology boutique, might apply a C of 9% to the utility and 21% to the biotech enterprise, in line with the typical volatility of biotech vs. utility stocks. In this model, while the company's overall cost of money might be 15%, it would accept utility investments yielding 9%, but would only accept biotech investments earning 21% or better. Hence different **hurdle** rates should apply to the two parts of the business. This is valid enough, but of course a utility project earning 10% will in the end destroy value at C = 15%, while a biotech project earning 18% will increase it.

Extending this argument, a single "blue sky" project can be considered as a very risky mini-business, and might seem to justify a 30% hurdle rate or higher. But what this approach overlooks is the well-proven financial concept of reduction of risk through **diversification**. A well-managed research enterprise will include large numbers of early stage projects, each individually still bearing high risk – but the **portfolio** as a whole will have much lower risk. Hence, it does not make sense to apply the risk rate for individual projects to that of a balanced portfolio, particularly when the company involved has a track record of successful innovation, and the unique risks of many individual projects are diversified over an R&D portfolio that includes hundreds of projects. Indeed, modern portfolio theory[9] *does not include* unique risk in the cost of money precisely because it is diversifiable – the relation between risk and return in financial markets is based on observed differences in volatility among different types of assets. Venture capitalists, too, understand the value of R&D portfolio diversification – not only do they diversify their own holdings, but they shun "one project" companies.

Do hurdle rates make sense? Perhaps so - as an initial screening tool for sorting investment opportunities into categories of risk. However, they are much too crude to function in valuation of a technology portfolio where risk factors change dramatically with time.

2. Using the status quo as the baseline

The decision to do nothing is itself a decision. The decision not to pursue new technology may not only imply lost opportunity, it may carry with it a deterioration in the current position. This rule does not apply to a financial portfolio. The decision not to invest in a stock or an option does not imply deterioration in the alternative investment, say an interest-bearing bank account.

However, for technology intensive firms, the decision to forego investment in innovative technology implies the maturing of existing technology assets, which will in time be reflected in lower margins or lost market share. These consequences will be the result of both competitive activity, and the fateful implications of the technological S-curves, which imply ever-diminishing returns for incremental technology investments in an established technology.

The dynamics of competitive activity is obvious enough – success with a new ethical drug, for example, will spur competitors to develop one that is even better. And when the original drug goes off-patent, generic producers are certain to enter the market and destroy asset value.

Less obviously, the attacker[10] riding a new technological S-curve enjoys an impressive competitive advantage. He will be viewed favorably by investors as he demonstrates above average growth rates and gains market share. His market capitalization will rise, and plans for raising additional capital for expansion will be welcomed by the equity markets. The defender's picture is gloomy. With lower growth rates his price/earnings multiple must drop, and management will be perceived as having destroyed shareholder value. Their reaction may be to cut costs to maintain profitability as they attempt to protect an eroding franchise – but cutting costs means having a smaller army to bring to the battle in the marketplace.

These dynamics are well understood in technology-based industries. However, in project valuation, it is seldom recognized that the base case is not simply adding the cash flow from the new project to the existing business base – it is the case where the star drug now has four generic competitors or that the existing product's margins are eroding by one percent per year. In technology businesses, standing still is not an option.

3. Miscalculating horizon value

Horizon value, also known as **terminal, continuing,** or **residual value**, represents the value of all future cash flows beyond those explicitly included in the cash flow table discounted to the horizon year. Its treatment is critical, because for a rapidly growing enterprise (the objective of many R&D projects) **much or most of the value will be incorporated in it**, and it is imperative to review the basics.

The choice of horizon year is essentially arbitrary provided that growth occurs at a constant rate thereafter, and the value will not be affected by this choice.[11]

Consider Case C in Table 1, where we have chosen five years for the planning horizon. Let us assume now that the cash flows of $300/yr continue into the indefinite future – if Case A was a five year note, Case C is a **perpetuity**. The horizon value of a perpetuity[12] is (1/C), which for C=12% is 8.33 times the annual cash flow of $300, or $2500. This perpetuity is obviously much more valuable than the 5-year note (NPV = $81), and has an NPV of $1,348 (since the $2,500 must be discounted back to the present). In this simple example 94% of the NPV is in the horizon value.

We are ready now to look at a *growing* enterprise, typical of an R&D project. Let us assume, cash flow tracks sales and builds quickly off a small base for five years and then levels off to 6% per annum growth. This is Case D-6 in Table 1, where cash flow is $50 in year 1, $100 in year 2, $250 in year 5, $300 in year 6 and then grows indefinitely at a rate G=6% to $318, $337, etc. in subsequent years. There is a mathematical formula[11] for valuing a growing perpetuity – it is the cash flow in the horizon year times the factor F of X/1-X, where X=(1+G)/(1+C). For Case D, C=12%, G=6%, X=0.9464, and F=17.65. Hence, the horizon value in year 6 is $300x17.65 or $5,297. Clearly, the horizon value of a growing perpetuity is substantially higher than the zero growth perpetuity (Case C), with degree of substantiality a function of the growth rate chosen. And despite initially lower cash flows (the NPV through year 5 is strongly negative), Case D is a more valuable property than Case C, as measured by NPV.

How sensitive is the valuation to the assumption regarding future growth rate? Very. Cases D3 and D9 in Table 1 reflect horizon values of $3,431 and $10,893 for growth rates of 3% and 9% respectively, and we have already seen that the horizon value for zero growth is $2,500 (Case D0).

It may also be asked whether it is even reasonable to assume growth in perpetuity – infinity is a long time in business terms. In fact, care must be taken in using growth-in-perpetuity mathematics, since as the growth rate G approaches the cost of money C the series converges less quickly – until the calculated horizon value becomes infinite for the unreal case where growth in perpetuity exceeds the cost of money. For case D6 (G=6%,C=12%), convergence is reasonable - more than half of the horizon value is captured in 15 years and about two-thirds within twenty.

What conclusions can we draw from this exercise? First, cash flow streams in the early years of a project are significant, but do not begin to capture the full value of a growing enterprise. Secondly, the assumptions used in treating horizon value are absolutely critical to the value of the project. “Conservative” assumptions, such as treating the project as a zero growth perpetuity can lead to poor decisions. So can excessively optimistic assumptions such as 9% growth rates forever, or failure to recognize the impact of patent expiration on future cash flow.

4. Focusing too narrowly on cash flow

The myopic use of cash flow models can also lead to poor decision making, although if financial analysis is carried out rigorously it makes little difference whether valuation is based on cash flow or earnings. What is the source of the problem? Simply that profitable, rapidly growing businesses may have negative cash flow as long as growth is rapid. This can lead to the silly conclusion that the business has no value, especially if one is too literally into the “cash is king” school of thought. While the error in reasoning is easily demonstrated in isolation, when negative cash flows from technology projects or growing businesses are embedded in an operating business with lower growth characteristics, cash flow oriented analysts are likely to calculate their value as *negative by difference.*

Let us look at an example that illustrates the issue. Consider a successful business growing at 25% per year, as in Table 2, and earning a return on total capital of just over 20%, over a planning horizon of 5 years.

Table 2

			Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pro forma financial projection (M$)						Year			
Line		Comment		1	2	3	4	5	5	5
	Growth Rate			25%	25%	25%	25%	25%	0%	6%
(1)	Revenues			$1,000	$1,250	$1,563	$1,953	$2,441	$1,953	$2,070
(2)	Variable Costs	60% of Revenues		($600)	($750)	($938)	($1,172)	($1,465)	($1,172)	($1,242)
(3)	Gross Fixed Capital	75% of Revenues		$750	$938	$1,172	$1,465	$1,831	$1,465	$1,553
(4)	Depreciation	10% of Fixed Capital		($75)	($94)	($117)	($146)	($183)	($146)	($155)
(5)	Working Capital	30% of Revenues		$300	$375	$469	$586	$732	$586	$621
(6)	Pretax Profit	(1)+(2)+(4)		$325	$406	$508	$635	$793	$635	$673
(7)	Aftertax Profit	60% of Pretax Profit		$195	$244	$305	$381	$476	$381	$404
(8)	Increase in Fixed Capital	Compare Successive Years		($188)	($234)	($293)	($366)	($458)	$0	($88)
(9)	Increase in Working Capital	Compare Successive Years		($75)	($94)	($117)	($146)	($183)	$0	($35)
(10)	Free Cash Flow	(7)-(4)+(8)+(9)		$8	$9	$12	$15	$18	$527	$436

			In Year 5	Present Value
	Calculating Horizon Value (12% discount rate)			
(11)	Actual Free Cash Flow valued as perpetuity in Year 5		$150	$85
(12)	Free Cash Flow assuming 0% growth after Year 4		$4,393	$2,493
(13)	Free Cash Flow assuming 6% growth after Year 4		$7,692	$4,366
(14)	20 x Year 5 Earnings	assuming 6% growth	$8,074	$4,583
(15)	12 x Year 5 EBIT	assuming 6% growth	$8,074	$4,583
(16)	NPV Free Cash Flow	Years 1-4	NA	$32

There is only one problem with this business – its free cash flow, line (10), is anemic. The positive cash flows from earnings, line (7), and depreciation, line (4), are almost offset by the increasing working capital, line (5), and fixed capital expenditures, required to grow the business at 25% per year.

Valuing the business based on the actual free cash flow is fraught with problems. The net present value of the tiny free cash flow in years 1-4 is only $32M, line (16). And the horizon value of the free cash flow of $18M in Year 5 , if treated as a perpetuity, is only $150M, which nets back to $85 in the present. The NPV of the business would then be the sum of $32M and $85M or $117M. This is clearly wrong: for if the business were to be turned into a perpetuity, it would no longer be eating cash to expand, and the free cash flow would be much higher. It is also wrong to value a business earning over $400M per year at less than one times earnings. (Using the growth in perpetuity approach for valuation is not here an option for the business reason that 25% per annum growth is not sustainable, and the mathematical reason that the series does not converge.)

Let us resolve the dilemma by assuming that management elects not to grow the business, and freezes revenues in Year 5 at the Year 4 level. No more capital expenditures and no increase in working capital. This case is shown in column (6). Year (5) earnings are down of course, but cash flow is up, from a tiny $18M to a whopping $527M. And the valuation problem goes away – we now have created a no-growth perpetuity, and its horizon value is 8.33 x $527M or $4,393M, which nets back to the present at $2,493M. Management clearly is not going to want to do this (absent external constraints or even better opportunities) and would attempt to forego the opportunity to generate cash for another year of 25% growth at which time it can revisit the option to stop growth with even higher rewards. In fact, management will continue to grow the business indefinitely as long as it remains an outstanding performer. Any other course destroys value.

The day will come, however, that 25% growth cannot continue, and the business settles into a sustainable but more mature growth phase – say at 6% per annum. Let us assume this happens in Year 5, a case shown in column (7). The cash flow for this case is nearly as high as for the zero growth case, and assuming growth in perpetuity of 6%, the horizon value is again 17.65x the free cash flow or $7,692M. Netting back to the present and adding in the small NPV of the cash flow from years 1-4, one obtains an NPV of $4,398M.

A simpler approach to the issue would have been to focus on earnings to begin with. At a price earnings multiple of 20 x Year 5 earnings, the horizon value is $8,074M. The same $8,074M value could be obtained as a ratio of 12 x Earnings Before Interest and Tax (EBIT) which in this case equals pre-tax profit. The valuation is about the same as the Free Cash Flow method, which gives a PE ratio of 19 instead of our assumption of 20.

To summarize, *the free cash flow approach is preferred by financial analysts in part because the discount rate is more predictable than price earnings ratios, which fluctuate rapidly with market conditions. The use of either earnings or free cash flow should give equivalent results if the long-term horizon is considered correctly. However, when high growth situations are embedded in a company's or a division's financials, significant undervaluation can inadvertently occur.*

5. Confusing investment with operating expense

The early stages of commercialization typically require extra R&D efforts to solve unforeseen problems, investment in market development, in the training of personnel, and plant start-up costs. From a book accounting viewpoint, these costs are usually treated as expenses and charged against earnings, when in effect they are investments. (From a cash flow point of view it of course makes no difference.) As the business grows, as one-time problems are solved, and as the market reaches equilibrium, the expense levels associated with driving rapid growth will diminish and will in any case be spread over a considerably larger business base. The long-term profitability of the business will look considerably better in time than it may have appeared in the early growth years.

6. Overweighting the analytic (versus the synthetic) approach

Analysis implies the separation of a whole into its parts, synthesis the construction of a whole from available parts. Technologists and financial analysts have very different mindsets, arising from their training, in dealing with linkage between projects and between technologies.

The classic financial mindset arises from the premise that maximizing net present value[13] is the best criterion for making investment decisions when given a limited amount of cash available for discretionary investment. This is the common capital budgeting decision facing most corporate managements. In this model projects are treated independently, and the combination of projects that maximizes net present value is the correct answer in terms of shareholder value.

Indeed, this analytic approach often goes one step further: it is pointed out that project proposals often contain several pieces, each of which has its own net present value, and while the net present value of the combination is positive, some of the individual pieces may not be. Therefore, shareholder value can be increased by doing only those subprojects which maximize value.

There is good reason for this view, for it is fairly common behavior for operating people to submit capital project proposals that just meet the corporation's predetermined hurdle rate. This may involve adding capital that is difficult to justify on a rate of return basis, but which is desired for operational or strategic reasons. It is part of the financial analyst's job to separate the total project into its components to ensure that the company's limited capital is all used productively. Hence, the mindset, while narrow, is not inappropriate.

However, even in capital projects there are often strong business linkages between the pieces. Consider a management proposal to build a styrene plant and a polystyrene plant, which will use styrene from the former as a feedstock. Let us assume that the styrene plant does not earn the cost of capital when styrene is priced on the merchant market. However, the combination of the two plants does earn the cost of capital. The business decision from the analyst's viewpoint is whether to make or buy the styrene (on the merchant market). But for a number of reasons the projects are not independent. For example, enough styrene may not be available on the merchant market to supply the proposed polystyrene plant, and customers may be reluctant to enter purchase agreements (or demand a lower price) for polystyrene if the styrene supply base is not secure. Therefore, these projects are linked and are not amenable to independent analysis.

In technology projects, the role of linkages is much stronger. It is also by no means as simple to analyze technology linkages as a basic make-or-buy raw material decision.

In fact, technology is all about linkages. Scientists are trained to think about linkages from the onset of their careers. No research project should be begun without a literature search – that is establishing where the technology to be developed fits into the context of all the work that has been done before. The scientific context may be scientific papers in the literature; patents held by your firm, competitor firms, or third parties; or research results within your own company. The technology context deals with the available technologies to build on, the technologies that could incorporate the proposed development, and the competitive technologies one will meet in the marketplace.

In this sense, everyone's technologies are linked – customer technologies, supplier technologies, competitor technologies and internal technologies. The technology decision-maker is required to assess this information and make astute choices as to which technical developments will create promising positions, and which would be highly compromised. His process is *synthetic*.

Often, he will wish to use internal technologies to the maximum extent possible, since it is there he has an "unfair advantage." His creativity, experience, and depth of training will largely govern those linkages he chooses to exploit and the much larger set he will ignore. The independent model simply doesn't fit.

Finally, a large unfair advantages occur when his firm exploits a new and proprietary technology that is linked to his corporation's core technologies. Even greater leverage occurs if he develops proprietary technology which enables the technology of other firms to create great future products under conditions where they must pay him dearly for access. These are the situations that create great wealth, and are the stuff of which industrial technologists dream.

In summary, financial analysts are analyzers – they are comfortable dissecting projects into their components. It is a narrow but useful discipline.

The best technologists are synthesizers. They think broadly, are often in a domain where there are no quantitative tools, and use the language of technology. In this game, gut feel and a sense for the future, and **connectedness to the larger technical community** count for as much as technical competence.

5. Neglecting the Spectrum of Possibilities

A financial analysts may choose to view a project as a "rifle shot" – starting with R&D and capital investment and ending with free cash flow. (This is the "base-case" discussed below, which would be assigned a large hurdle rate owing to its risk.) The experienced technologist sees a spectrum of possibilities, from quick and total termination after a definitive unsatisfactory laboratory result to commercial exploitation in a broad range of markets. Between these extremes are many other possibilities such as the spectacular flop or domination of attractive but narrow markets. All of these potentials can be mapped using **decision trees**, a technique where the net present value of each outcome is weighted by the probability of its happening .

Chart 1 provides a simplified illustration of a research project and its branch points. We assume that if the project is authorized, we will spend $1M *on a net present value basis* to establish its feasibility with a 50% chance of success. Then, if feasibility is established, we will spend an additional $3M on development. The chances of failure at this stage are rated as 40%, or 20% among the total spectrum of possibilities.

If the development succeeds, we will enter commercialization. We foresee a spectrum of possible outcomes – one is the most likely or base case, which we assign a probability of 50% (or 15% of all possible outcomes. In this case the commercial rewards have an NPV $14M, which nets to $10M after R&D costs are deducted.

We have also constructed a low side case with a probability of 25%, corresponding to a situation where vigorous competition limits both market share and margins more strongly than anticipated. We assume this case has a commercial value of only $3M, which nets to ($1M) after sunk R&D costs, but that it proceeds because it earns a margin above the cost of capital. We would not have pursued this project if this outcome were known at the outset.

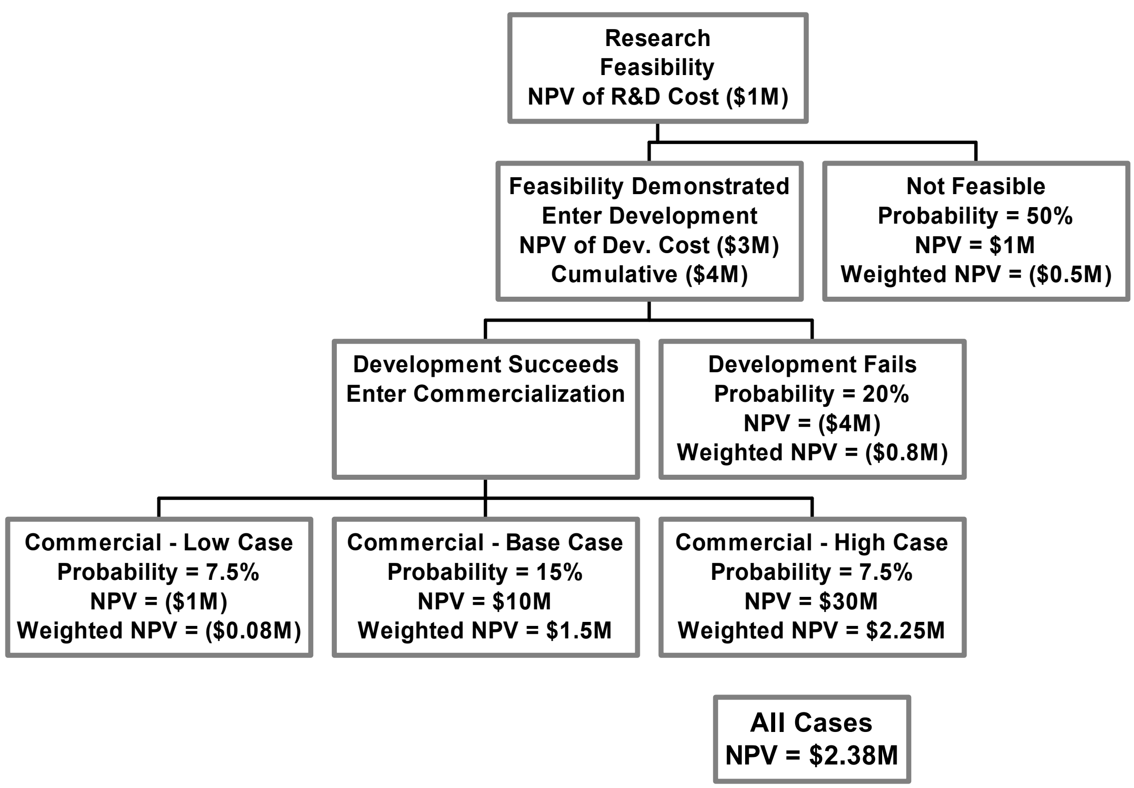
Finally, we have constructed an upside case where competition proves less vigorous than anticipated (perhaps because a strong patent has been issued) and margins and volume are higher. These conditions leverage the internal rate of return and the net present value, as we have seen above, is sharply higher, $30M net of R&D costs.

Chart 1 considers only five possible outcomes. In particular, we have considered only a two-gate process[14] for R&D effort – versus the five or more that are frequently employed. In the real world there are also a myriad of possible commercial outcomes, and their values can be computed using Monte Carlo techniques.[15]

However, the decision tree approach highlights two important features that the base case alone does not address. The first is **the option to terminate the project**. This option, if exercised early, costs only $0.5M – far less than the projected R&D cost of $4M *plus* the capital at risk during commercialization. It is also likely that some of the costs will be recovered in the form of intellectual property. The firm now knows that a plausible technological approach is a loser and can focus its efforts elsewhere. The project has provided training to scientists and engineers and increased the firm's knowledge base. And if it is lucky, the exercise has revealed new opportunities not hitherto considered. A few patents of uncertain value may also have been acquired.

The second key feature[16] is that **most of the reward may be concentrated in the upside case**. This situation is indeed common in hindsight as one looks at some spectacular examples of wealth creation through technology – aspartame, erythropoietin, Tagamet, DOS, where the field somewhat improbably was swept clear of effective competition. Project champions are usually aware of the upside case, but are understandably reluctant to *forecast* it versus a more conservative and credible base case that just meets the hurdle rate. Too bad, because that's where the value is.

Chart 1
Project Decision Tree



6. Neglecting the options approach to valuation

The **options approach to valuing R&D** has been the subject of seminal thinking[17], and while possibly too complex for quantitative decision-making, may be extremely useful in creating robust technical strategies.

Financial options, such as puts or calls, are bought and sold daily in the marketplace, and are typically priced by dealers on the basis that markets are efficient and price fluctuations are random. Valuation is done using the Black-Scholes formula[18], or close cousins of it, which define a volatility parameter called β (beta) reflecting the historical standard deviation of the price of the security. The method clearly works for financial securities since marketing of options is a competitive business and a material conceptual error would be ruinous.

Perhaps the most interesting characteristic of financial options is that the more volatile the stock, or **the greater the β, the more valuable the option**. Risk enhances value. Also, the longer the option has to run, the greater its value.

This is in sharp contrast to conventional securities such as stocks and bonds, which are often valued using the Capital Asset Pricing Model (CAPM), where **risk or β, decreases value**.[19] The future earnings of a utility like Duke Power will be discounted less steeply than a biotechnology firm such as Amgen.

What are the implications for R&D? To the extent that we have treated an R&D project as a mini-enterprise, and a stock is a proxy for an enterprise, risk is bad. This is another way of saying that risky projects should be discounted at high rates. The options approach leads to the opposite conclusion – that a diversified portfolio of higher risk projects is more valuable than lower risk projects[20]. This makes some sense – since in the extreme case where R&D risk is virtually nil (known technology), it is likely the company will earn only the cost of capital and the economic profit will also be nil. Returns correlate with risk. No pain, no gain. Unique risk should be managed through diversification.

In fact, R&D projects have both characteristics: they can at the same time be mini-enterprises aimed at generating free cash flow, and options to invest in new opportunities. In some cases, such as incremental new product research, the former may dominate, in other cases such as early-stage high-risk research it will be the latter. In any case, neglecting the option value undervalues the technology, perhaps grossly so.

A proxy for β in the technology world may be the uncertainty of that future world in which the technology may emerge. Investors in micro-electronics are uncertain as to whether future communications among personal computers will occur via telephone, cable, or wireless, but must buy their technology options now, just as a buyer of a put or call on 3Com stock makes a bet regarding the future for modems produced by that company.

It is suggested that the following four factors be considered in valuing the options created by a proposed new technology.

1. Technology Pairing. A new technology can be paired with an existing or future technology to create value. It is useful to look at the set of such pairs and begin the analysis of where the greatest value may be created. **Technology platforms**[21] create enhanced potential for technology pairing.

2. Size of current and potential markets. The value of the technology option will relate both to the value added per unit, and the number of units in the target markets.

3. Strength of Linkage. The value of the innovative technology may be quite different in different markets – that is the linkage between the technology pairs can be strong or weak. An enabling technology is particularly valuable.

4. Polarization of the linkages. Polarization is important in the sense that it will govern whether the owner of the new technology or of the existing technology will be dominant. Rewards must be shared between parties, but the ratio by which they are likely to be shared will depend on their relative technological and market positions.

An example might illustrate these points. Let us postulate that a new technical concept is believed to be capable of creating an inexpensive rechargeable battery with radically lower size and weight than existing batteries.. A successful R&D program would create several kinds of **options** to commercialize the technology in an uncertain future world. Two technologies with which the battery may be linked include laptops and electric vehicles (EV's), as well as several others. The technology could be enabling (the upside case again!) for high performance EV's, but would never be so for laptops. The **size** and growth of the laptop market is readily estimated, whereas the **potential market** for EV's is highly uncertain and would in fact be conditional on the cost and performance of this technology. Therefore, the **linkage** to EV's is very strong, whereas in laptops it is moderate. The inventor may even have an opportunity to dominate EV's using toll manufacturing agreements (**polarization** works in his favor), but has no such opportunity in laptops, where established manufacturers are expected to dominate the relationship.

CONCLUSION

Managing technology is about managing risk. It is easy to fall into the conceptual trap that risk is bad and to forget that risk should correlate positively with reward. It must be recognized that risk can be reduced through diversification as well as through a quest for certainty. Managing technology is also about creating opportunity, and to the degree that one focuses on certainty, opportunity may slip away.

NOTES

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