Working Paper Series

20/2006

Technology strategy

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V1.3 – 12/07/06

Technology Strategy

Arnoud De Meyer

1. Introduction

A technology strategy, like any functional strategy, has two purposes. It is on one hand the translation of the overall strategy of the organisation into a coherent set of long term instructions for investments for the sub-organizations that are active in technology development, be it through product or process development or through the development of more general technological know how that can be used in product and process development. But at the same time it is also the development of technology based opportunities or options for the organisation to steer future developments, i.e. provide the capabilities that enable the organisation to shape its future.

In practice such a strategy is expressed in a set of research and development projects to be implemented by the organisation. These projects can be carried out in one organisation, but more often they are distributed over a set of laboratories spread out over different locations and organisational subdivisions. In many cases they entail the cooperation from representatives from different functional departments or organisational roles. But whatever the organisation the focus of a technology strategy remains on the definition and the development of the portfolio of projects. The key decisions in technology strategy are thus the choice of the individual 'attractive' projects, but also determining the shape of the portfolio of projects that will support the organization's strategy.

Decisions like these are taken in a context that determines the success of their implementation. Providing insight in technology strategy requires discussions on how the choice of projects and project portfolio is made, but also on some of the issues of implementation. In order to discuss these we will use a very simplified framework that is summarised in figure 1. In this framework one can see that the determination and implementation of a technology strategy is embedded in an organisation where there is clear leadership that sets an overall strategic context. Such an organisation may create the conditions where creativity can blossom and where market and user information may meet the technological capabilities developed within the organization, leading to the generation of lots of ideas. Normally such an organization will have an overload of ideas and one of the essential tasks in the determination of the technology strategy is to evaluate project on their own merits as well as their contribution within the portfolio. Projects thus selected are prime candidates for investment, but in order to succeed those investment opportunities needs to be checked with the available capacity of the technology organisation. The final project programme will be the result of these three evaluations. Finally the execution of this programme needs to be evaluated and compared to the guidelines that emerged from the leadership and the vision.

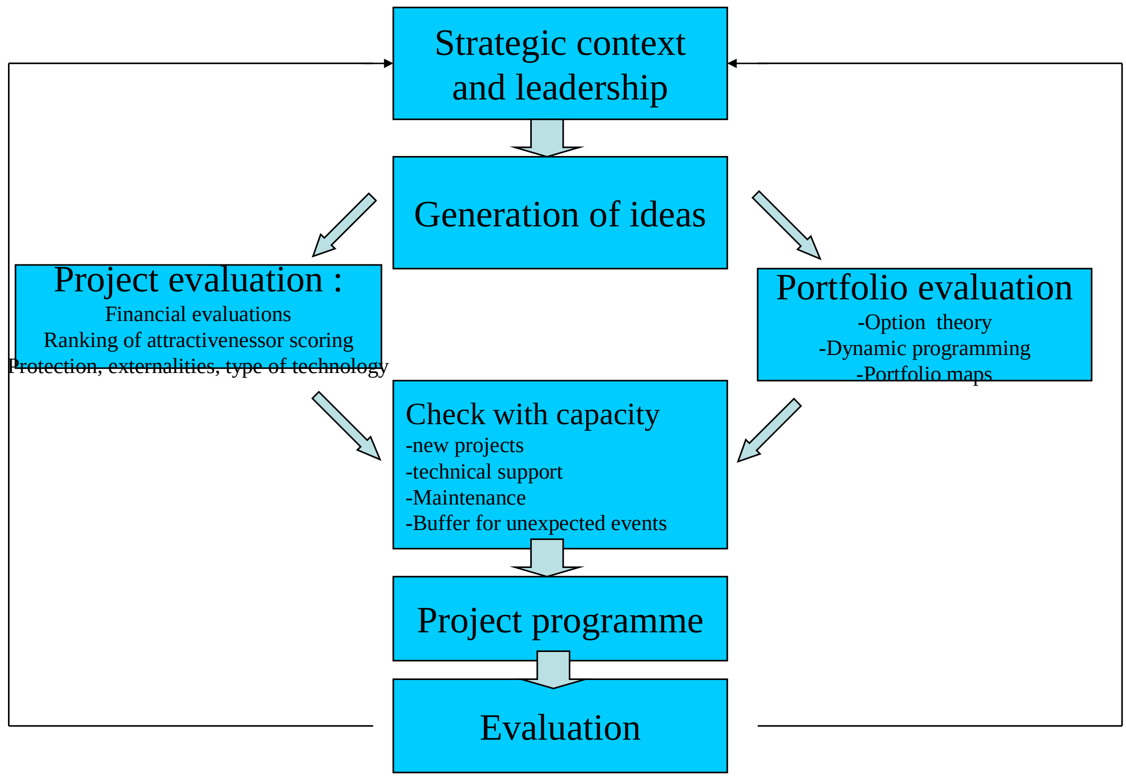


Figure 1: a simplified description of technology strategy

2. Creating the Strategic Context and Providing Leadership

Building a successful technology strategy can only happen when the organisation is clear about the direction it wants to go in. The technology strategy needs to be tailored to the overall strategy of the organisation[1]. This requires a clear vision defined by the leadership of the organisation as well as the creation of an environment where this vision can be shared by colleagues and collaborators.

The leadership needs to set the goals: what kind of business does the firm want to be in and how do you want to position the firm vis a vis the competition. By doing so it also defines what should and should not be pursued as innovation projects. A clear vision is the best way to help to define the portfolio of projects and the criteria that you need to use to evaluate new opportunities. And it helps also when the organisation needs to say no to a new or ongoing. Let's not forget that some of the most difficult decisions in innovation are precisely to say no to a project or to stop a project that does not deliver the results one had counted on[2] [3].

A good vision that can enable the development of a technology strategy should live up to two conditions: it has to combine a long term view with concrete short term goals and it should not be too constraining. The organisation should not feel too comfortable because the challenges are defined too far in the future. Technology strategy needs to stretch the organisation beyond its comfort zone. But a too constraining and too focused vision is not helpful either. A too narrow tunnel vision which constrains technology development to a very narrow path will kill creativity and create a false sense of security because the organisation knows too well what it needs to do.

Simply providing that clear vision is not sufficient. Real leadership is also ensuring that the rest of the organisation has taken ownership of the goals, understands them and acts according to them. Innovative leadership requires a lot of communication, convincing and cajoling until the vision has been absorbed throughout the organisation.

This combination of defining and communicating the vision is what we call *the strategic context*. Organisations rely on it to harness their creativity. Without a clear strategic context, creativity may blossom, but it will be disjointed. Strategic context gives purpose and direction, benchmarks and role models. It measures progress and shows the way ahead.

3. Generating the ideas

Defining a technology strategy requires the existence of raw materials to carry out the evaluation and selection of projects and to determine the optimal portfolio. In other words the organisation needs good ideas for projects to choose from. Increasing the stock of good project ideas requires two things: having access to stimulating information and an environment that stimulates creativity to transform this information into project ideas.

Discussing here what the very rich literature on creativity can offer on how one can stimulate creativity to generate project ideas would take us too far away from technology strategy. There is however one practical concept that deserves to be mentioned in passing. Over the last ten years Kim and Mauborgne[4] have developed their ideas on value innovation, i.e. a structured method to discover hidden and underemphasized as well as obsolete performance parameters for a product or a service. Once these are known one can redefine the rules of the competitive game by innovating by reducing the performance offer on obsolete parameters and investing ahead of the competition in the yet undiscovered performance parameters. This is for all practical purposes a more strategic view on what the quality movement in the late eighties and early nineties argued about design quality. In that earlier view it was argued that any product or service could be characterised by eight performance parameters: functionality, ease of use, durability, serviceability, the operating cost, and the cost of complementary assets, system compatibility and aesthetics. The customer expectations for each of these performance parameters can be drawn as a function of the price the customer is willing to pay for them (figure 2). If the performance parameter is very price elastic (i.e. a steep curve) there is a good opportunity to invest in technology development through R&D. If on the contrary the performance parameter has low price elasticity (i.e. a flat curve) there is little scope for innovation through technology development. What was usually less emphasized in this literature was that the shape of the curves can and probably will change over time and that what used to be in the past an unattractive performance parameter for innovative investments, could well turn out to be a very attractive one in the years to come. Kim and Mauborgne deserve the credit for having made the implications of this dynamic far more operational.

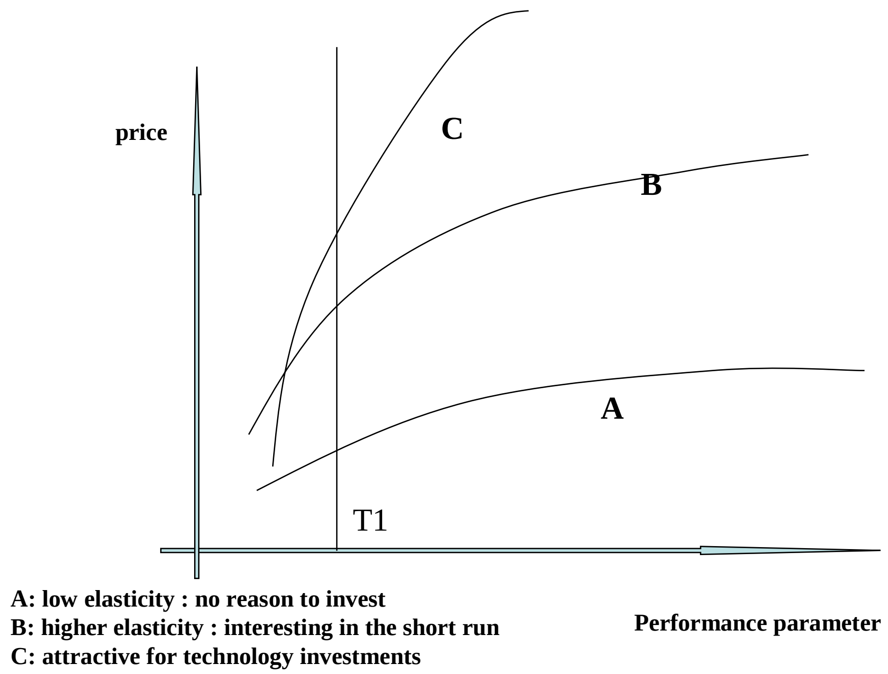


Figure 2: Elasticity of Performance functions

On the second issue of getting access to stimulating information there are two important points to be made. The first one is that the development of a technology strategy is the result of the interaction between the stock of tacit and explicit organizational knowledge created by the firm and the latent and explicit needs of the customers or users. From the earliest studies on innovation [5] the observations have constantly been pointing in the same direction: most of the information used by innovators was personal knowledge, rather than personally researched knowledge. Only 8% of innovative information came from experimentation and calculation and 7% from printed materials. Such empirical findings, and those of countless studies that followed this first work, suggest that “science and technology are vital tools that need to be applied effectively and developed selectively. But…innovation is more a matter of flexible, productive and focused employee relations in the workplace than it is the result of technological resources or the impact of science…”[6]. This seems to suggest that the organizational knowledge, which is embedded in the interactions between the employees of the firm, is an important source for project ideas that provide the input for a technology strategy.

But one needs to go further than to see the stock of wisdom about technology, administration and management systems as the only or main source of project ideas. The set of projects out of which a strategy can be built, is the result of the interaction between this organizational knowledge and the experience and tacit knowledge that users and customers have about the fulfilment of their needs: the fulfilment of the explicit and tacit needs of the users define whether a new product, process or system provides a significant change in the value/price relationship. Innovation exists only when one can couple the organizational know how with the users needs. The strategic choices about technology can only be appropriate if they are made in a way that is consistent with the evolving interests of the firm and the users. The managerial challenge for the development of a technology strategy is thus to mobilize both the organisational know how and the user’s know how and have a healthy interaction between them.

The second point is precisely about the need to listen to this information coming from outside the organisation. Innovation without intimate customer and user knowledge is not possible. Von Hippel[7] made as one of the first the point that in many cases the source of innovative ideas lays outside the organization, often with users. They have a stake in the development of the innovation because they can reap the benefits of it. His original example was that of scientific instruments. In that case the user often develops a handcrafted prototype that is meeting his or her unique specifications. With creativity the supplier of scientific instruments can probably see the wider applications and transform this prototype into an industrial product. A similar process happened with internet usage. In many cases it is a frustrated user that develops a software improvement or an additional service and many internet based companies have been successful by exploiting the ideas of the users.

Often one thinks that this knowledge is available only in sophisticated markets. Or at least that was what the proponents of the international product life cycle argued[8]. But today this is no longer true. We know from empirical studies that emerging markets in Asia, South Africa or Latin America are not the most supportive for an innovator[9].

Customers tend to be more conservative, markets are heterogeneous and market data is often not available. But they do have often needs that are different from the users in the traditional industrialised countries and that can be sources for new projects in technological development.

Doz et al [10] have developed the concept of the metanational organization, or an international organization that is able to take advantage of its global presence to combine information and knowledge from different parts of the world in order to come up with an innovation. Let us take a stylized example to illustrate this. Assume you want to come up with a new mobile phone that combines the sophisticated use of SMS as one finds it in the Philippines (which is one of the most sophisticated market for mobile messaging), the patents of Qualcom in the US, the fashion trends for electronic gadgets as it is prevalent in Los Angeles, the technology of miniaturisation developed in Japan or Korea and the competitive benchmarking with Nokia in Finland. You need antennae in different parts of the world to capture the knowledge and you need the ability to combine this knowledge and roll it out. Doz et al call these three activities sensing, melding and deploying. Sensing is the activity whereby a firm attempts to gather knowledge about user needs all over the world. In the 'melding' (a combination of welding and melting) one needs to have the entrepreneurial insight to identify an opportunity to create an innovative product, service or process. The deployment also requires the cumulated wisdom of the organization. In order to roll out the innovation and get global leverage as quickly as possible one needs to be flexible about building the most efficient and rapidly scalable global supply chain.

4. Evaluating the individual projects

In the simplified diagram in figure 1 project evaluation and portfolio evaluation are shown in parallel and not sequential. This reflects the interactive nature of these evaluation procedures: often individual projects need to be evaluated within a context of other projects e.g. because of their spill-over effects. And the evaluation of a portfolio may show a gaping hole in the portfolio, triggering the development of a new project proposal. But in order to have a portfolio one needs first projects. Therefore we will start first with the evaluation of individual projects.

The most straightforward approach is that technology projects are to be evaluated as any other investment made by the organisation, i.e. through some kind of a net present value calculation. While the logic is correct, quite a few authors have pointed out that these NPV analyses overlook the value inherent in the strategic flexibility that is created by technology projects, in particular when they are seen as sequential investments, i.e. where the knowledge built up through one project (or a phase in the project) may lead to new insights and adjusted projects and new investments. As a consequence the traditional NPV methods are often seen as too conservative[11]. Nevertheless they do provide some insight in the value of a project, in particular, as we argued elsewhere, when they are used as a tool for sensitivity analysis. By evaluating what the most conservative hypotheses are that need to be fulfilled to make a technology project worthwhile, one can get a good idea of the risks involved in the project.

Given the often uncertain nature of projects such NPV methods have been complemented with questionnaires and systems that attempt to rank the relative attractiveness of individual projects or with scoring methods that weigh in one way or another the risk involved in the project with the potential benefits[12]. Often these methods consist of long organization-specific lists of questions about the market potential, the technology gap, the strength of the team, the competitive position, the ability to protect the result of the project, the spill-over effects, etc. Based on the results of these questionnaires projects can be ranked according to a number of weighted decision criteria. Outcomes of these questionnaires may be a relative positioning of the project or an absolute ranking. A rough approach consists then in the comparison of the outcome of this exercise with the capacity of the organisation and to fill up the capacity with the top ranked projects. This approach has the drawback that it has difficulty capturing the real risks involved (both upside and a downside risk), and it very often cannot take into account the positive externalities of the projects.

These risk lists do have value because they help an organization to reduce the unforeseeable uncertainty into foreseeable uncertainty. They also can help as a tool to get different functions or roles of the organisation to exchange information about the projects. In this way they can be very valuable in the evaluation of projects. But most scoring methods fail the test of rigour and relevance when it comes to selecting projects.

Complementary to NPV methods and rankings it can be helpful to consider the selection activity a process through which the organisation attempts to analyse to what extent the conditions are favourable to carry out the project within the organization. Projects may be intrinsically interesting but the organisation may not have the capabilities to bring them to a successful end. We have found it useful to evaluate projects on five questions, organised in a decision tree (figure 3)[13] and based to some extent on the early work of Teece[14].

In analysing the decision tree one needs to take into account that there are two strong simplifications in this decision tree. The first one is that answers to the five questions are binary, i.e. yes or no, weak or strong, etc. In reality this is not the case and answers often are more complex and conditional. The second simplification is that the answers to the questions are fixed. In practice it is precisely the managerial action that enables the organisation to change the answer and thus eventually improve the attractiveness of a project.

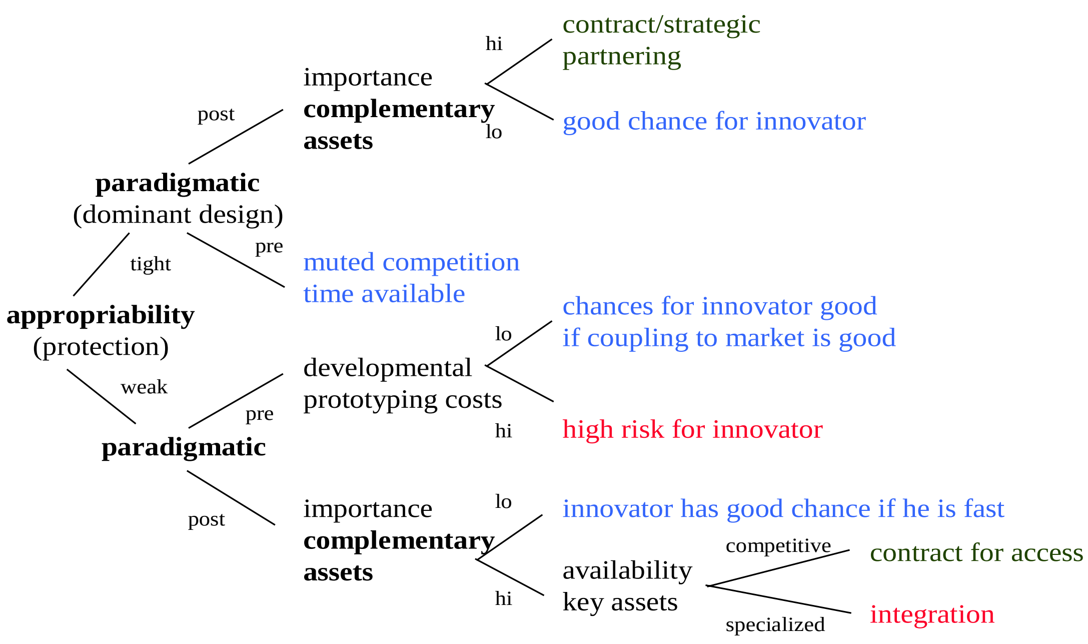


Figure 3: Decision tree to evaluate the potential of a technology project

The five questions are as follows:

a. How easily can the organisation protect the know-how developed in the projects and thus appropriate the benefits derived from the project in the form of rents. Such a protection can of course take many forms. Patents can play a role, but since we know the limitations of patents in the protection of intellectual property rights[15] we also need to take into account other forms of protection such as brands, trade secrets, copyrights, a monopoly on critical resources, speed in development, market dominance, etc. [16]
b. Is there already a dominant design (or market paradigm) for the product or the system in the way it is defined by Utterback and Abernathy[17] (and confirmed by many empirical follow up studies). The unit of analysis in their model is a new technology, or new combination of existing technologies. The model argues that you can distinguish four stages in the development of the new technology. In the first, fluid, phase, there will be a high degree of activity in product innovations, which are offered to the market. There are several reasons for this, but the two main ones are the low barriers to entry, and the

difficulty to carry out market research in emerging markets and thus the need to experiment. This first phase usually leads to the emergence of what has become commonly known as a dominant design. It has lots of scientific descriptions, but in brief it is a sort of milestone or quasi-standard in an industry. In a sense the product that becomes a dominant design embodies the requirements of many classes of users, even though it may not perfectly match the requirements of one particular group of users. The emergence of the dominant design changes the nature of the competition completely. From competition based on the functionality of the product, one moves to a competition based on cost and quality. The challenge is not any more to define your product, but to offer a product similar to the one from the competition at a lower price. That requires usually heavy investments in automation, business reengineering and a much leaner organization. This is a period of intensive process innovation. Finally, there is a fourth phase in the technological life cycle, when innovation, both in process and product, becomes less relevant to the survival in the competitive arena, and where the context in which, and the amenities that come with the product, are an essential element of the competition. For the purpose of our analysis it is at this stage sufficient to understand whether for the project at hand the dominant design has emerged. It will be clear that the market relation is a very different one before and after the breakthrough of the dominant design. Before that breakthrough one needs to be in close contacts with customers and/or users in order to keep the finger on their pulse and to observe the sometimes quite dramatic changes in customer preferences. After the breakthrough of the dominant design standard techniques of market research will be sufficient to measure the smaller changes in customer preferences.

This model is related to the concept of disruptive technologies[18]: the emergence of a disruptive technology creates the conditions for the start of a fluid phase and the redefinition of a dominant design.

c. What is the speed with which a prototype can be developed? Speed of development has been at the core of a lot of studies in the eighties and nineties of previous century.[19] Iansiti[20] showed convincingly that the performance of technology development and the competitive position of the firm are influenced significantly by the speed with which prototypes can be turned around. And more recently the work of Thomke on experimentation provides interesting insights in the competitive influence of rapid experimentation and the role of computer aided tools therein.[21 22]

d. How important are the complementary assets in the realization of the benefits provided by the project[23]. The importance of overcoming network externalities in the success of a project have been widely documented and partners can play an important role in building up the network of products and processes that enable the realisation of the full benefit of the project. The success of a project will depend to a large extent on the importance of these partners: the more important they are the more one is dependent on the availability of these complementary assets. The success of a project will thus often depend on the balance of power with these partners.
e. The availability of the complementary assets is an important issue. Therefore a fifth question is how these complementary assets will be accessible. Are these complementary assets available on a competitive basis and can the organisation put the suppliers of these assets in competition with each other, or are the providers specialised (and thus onlyu offered by a monopolist or through an oligopoly.

The decision tree suggests clearly that there is a particular sequence to be followed in answering the questions. A few examples will help us to understand how this can be used to evaluate the potential of a project and at the same time how one gets some insights in the implementation challenges.

Assume that protection is relatively easy in the industry concerned (think for example of the pharmaceutical industry) and the project will lead to patentable know how. In this case the appropriability of the rents is tight. Assume also that the dominant design or the market paradigm is not yet known. The role of complementary assets in this case cannot yet be important (otherwise the dominant design would be determined by the complementary assets). One is in a situation where competition is hampered by the protection, but where the innovator will need the time to shape the dominant design. The problem for the innovator is in this case mostly bridging the cash gap between the investment in the project and the cash flow derived from sales. These are projects that can be very promising on condition the organisation has sufficient cash.

Success will also depend on a good connection with the market in order to make emerge the dominant design.

If on the contrary the dominant design is known, then one has to focus on the complementary assets. If those are not important the project has the potential of being a success: it is easy to protect the IP, the market needs are known and the organisation is not dependent on third parties for its success. If they happen to be important, the organisation will need to contract or develop a partnership for access to these complementary assets. But it does so from a position of strength.

The more difficult cases happen at the lower side of the decision tree, i.e. when the know how generated by the project is difficult to protect. Assume this case and assume also that the dominant design is not known. In this case the project will require constant adjustment to be in tune with the changing needs of the users and customers, but this from a rather uncomfortable position of weakness with respect to IP protection. Critical to the success of the project is the speed with which these adjustments can be performed, i.e. it will depend on the speed of experimentation and turning around a prototype. If that speed is high there are still some good chances for the project to succeed, on condition that one can stay informed of changes in market conditions and customer preferences through a very close coupling to the customers. If on the other hand the speed of prototype turnaround is low (and even worse if it is combined with high costs of prototype development), the project has very little chance of succeeding. The only consolation may be that while the downside risks are high for such a project, few other organisations may venture in this field, and the margins may be high if success is achieved.

Following the path of weak protection, but with an existing dominant design, we have, as in the upper branches of the decision tree, to consider the importance of the complementary assets. If they are not important, speed and quality of management will have to replace the weak position in terms of protection. A good knowledge and interaction with customers is also important to success, but chances are fair. If on the other hand the complementary assets are important, one needs to consider how they are available. If they are delivered on a competitive basis, and access is easy, one needs to ensure that the right contracts are in place. The negotiation position is less favourable, but not impossible. If these assets are in the hands of a monopoly or an oligopoly, the success of the project is not really yours, but can only be realised through a close integration with those who control the complementary assets. Such integration can take many different forms: selling the IP, creating an integrated organisation, etc.

It is worth coming back to an earlier comment: this is not a deterministic model because good managerial action can change the answers to some of the five questions. For example the answer to the first question about protection is perhaps more about what one can do to improve the protection so that one feels comfortable enough to answer that one is on the upper branches of the decision tree. The value of the decision tree is more in the reflection that one can have in the organisation on how to get the most benefits out of a given project.

5. Selecting the portfolio

Once the organization has selected the candidates for the projects for technology development, it needs to figure out how attractive the collection of projects is and how that collection or portfolio supports the overall business strategy. Three broad categories of solutions have been proposed over the years. A first stream of ideas comes out of the operations research literature and proposes optimisation methods for portfolio selection, mainly based on mathematical programming. A second, more recent stream of literature sees technology projects as options and applies option theory to the project portfolio. A third, far more qualitative approach, suggests visualizing the project portfolio in series of matrices that help the managers in qualitative decision making on what the most appropriate portfolio is.

Mathematical programming models have long been proposed[24][25]. They have the attraction that they lead to an optimal portfolio, can easily take into account the interactions between the different projects, and allow for sensitivity analysis. Though this body of knowledge has provided a great number of applications in different industries and for different type of portfolios, and can rely on the rich literature on mathematical programming, it never has really caught on with practitioners.[26] Often the reasons cited for the limited application is the limited capability of these models to incorporate risks, as well as the need for extensive and reliable information. Often the data collection needed to make the model practical was considered to be too heavy to make it a valuable exercise. Another reason may well be that the academic world has emphasized too strongly the sophistication of the model and has in the process forgotten that these models need to be understood by managers[27] in order to be trusted. Too often the models have been perceived to be black boxes that did not allow managers to gain managerial insight. This does not mean that there were no successful applications. Loch et al [28] describe an interesting example of such an adoption. The mathematical model used is a fairly simple mixed integer linear programme but the emphasis of the exercise is more on the use of standard methods proposed by the product innovation literature on how to transfer knowledge, e.g. gatekeepers, weak ties, overcoming stickiness of information, etc[29] in order to improve the utilisation of the model and its diffusion throughout the organisation.

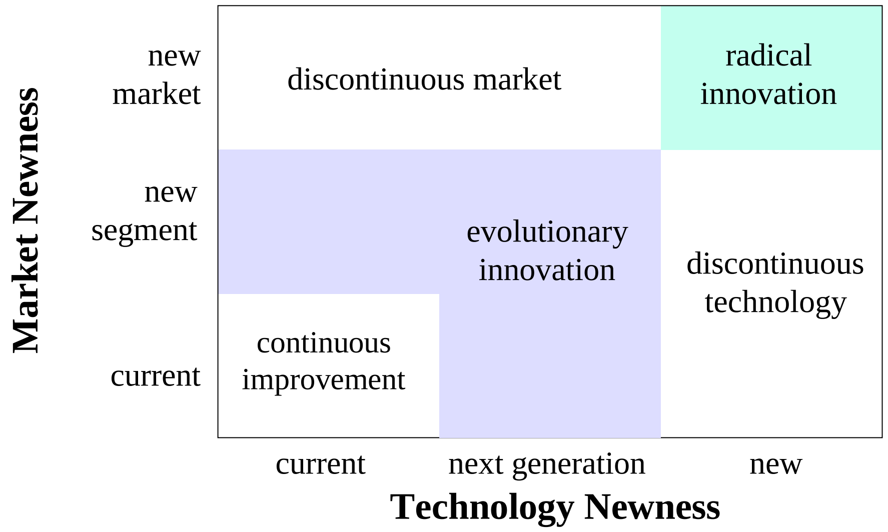
A second approach proposed in the literature, but not yet widely practised by managers is to use real options to evaluate the project portfolio. Real options' thinking has been proposed for strategy development beyond technology strategy. [30][31] Applying real options is really arguing that an investment in a technological project is buying a ticket, e.g. for access to a profitable market in the case of an R&D project at some time in the future. In this way investing in a technology project is like holding an option analogous to a financial call option. With the discovery of new information and the resulting reduction of uncertainty, one can adjust the initial technological strategy. As with financial options this provides a flexibility to adapt to new information. It thus improves the value of the investment in the project because it enhances the upside potential, while limiting the downside losses relative to the initial expectations about the project. This real option approach goes contrary to what NPV approaches do to technology projects: NPV treatment understates the value of an investment in technological development, real options enhances its value.

This approach has brought some early interesting insights. For example Huchzermacher and Loch[32] argue that in R&D one faces more diverse forms of uncertainty than in traditional financial applications. Apart from the uncertainty in pay-off there are also higher uncertainty market payoffs, project budgets, product performance, market requirements and project schedules. They find the interesting and unexpected result that if uncertainty is resolved or cost and revenues occur after all decisions have been made, more variability may smear out contingencies and thus reduce the value of flexibility. In addition variability may reduce the probability of flexibility ever being exercised, which also reduces its value.

But there is also quite some criticism on the real options approach to strategy. In a recent debate Adner and Levinthal[33] argued strongly that real options cannot be applied to strategy (and by extension to technology strategy) because one of the major assumptions of real options is that abandonment can be done efficiently. They express strong doubts that organisations can do this: ‘ the greater the role of an organisation in molding the possible course of an initiative after an initial investment, the greater the organisational challenges and the strategic trade-offs associated with applying a real option. As a result the less helpful the logic is for guiding strategy’.[34] This is clearly a debate that needs further research.

A third, more qualitative approach attempts to present the portfolio of projects in a coherent and usually visually attractive way, such that managers can discuss the merits and weaknesses of the portfolio. This has been the result of the reflections of quite a few consulting organisations, and virtually all of the big consulting firms have developed their own set of matrices[35]. In figure 4 there are a few examples if such matrices. The purpose is usually to map the different projects (often represented by circles or squares that given an indication of relative investment size) in the portfolio in matrices with dimensions like:

- the risks involved in the projects versus the expected financial return
- the competitive technological position of the organisation vs. the maturity of the technologies used in the project
- the expected cash flows over time
- the market position of the organisation in the targeted market segment versus the market attractiveness
- the newness of the expected output in compared to similar products or processes in the market versus its technological newness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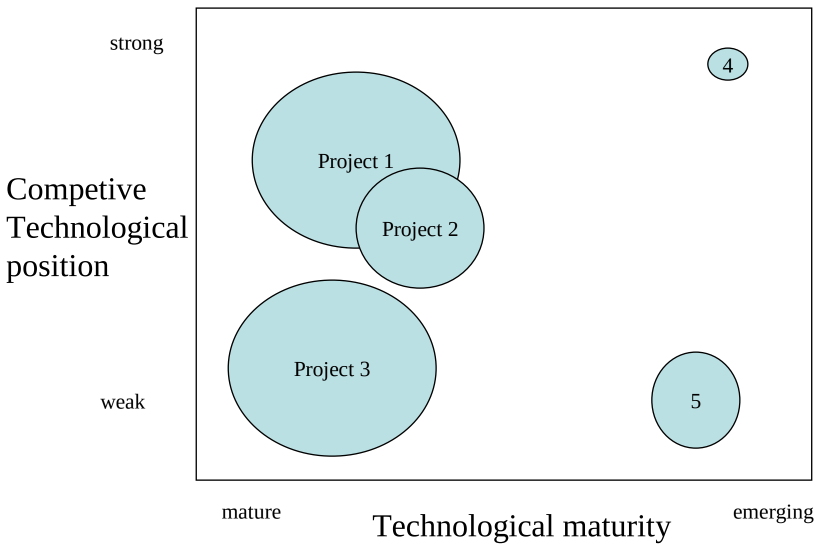
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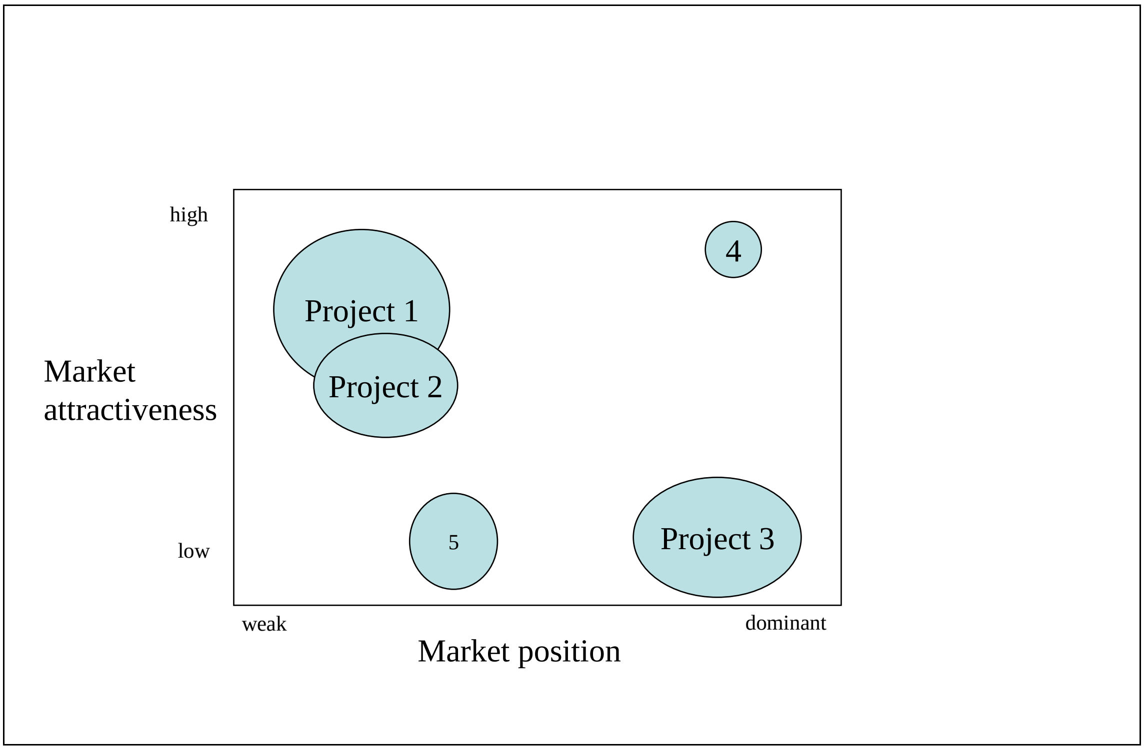

Figure 4: examples of technology strategy maps

The advantage of these matrices is that they are relatively easy to construct and understand, and that they can form a basis for discussion. But the disadvantage is that most of them remain very qualitative, become unwieldy when there are many projects and provide little guidance of what a good portfolio is, let alone providing an optimal portfolio. There are some guideline one can apply to these matrices, e.g. that an organisation needs to have a balanced portfolio (neither too risky nor too conservative), it should not postpone all the positive cash flow towards the end of the portfolio life, etc. But the only real managerial advice that one can give for these matrices is that the management of the organisation should feel comfortable with it and that the portfolio should be in line with the risk level the organisation is willing to take.

6. Matching with the capacity of the technological organisation.

The chosen portfolio must be implemented. This may appear to be an operational issue, not belonging to a discussion on technology strategy. But far too often technology strategies do not get implemented because the capacity of the organisation to carry out the projects is overestimated. Anybody will understand that a simple calculation whereby the capacity of the organisation to carry out technology projects is C and the average load of a project is L that the capacity of the organisation is less than C/L because of variability or delays in execution. But the reduction of available capacity due to these factors of variation is higher than usually expected and it can be shown with fairly reasonable assumptions that the capacity of an organisation is often only 80% or lower of its theoretical capacity. Therefore one can try to increase the available capacity by effective process management, i.e. eliminating unnecessary variations in workload and work processes in order to eliminate distractions and delays, and effective bottleneck management[36]. But from a more strategic perspective the message is that it may pay off to take on fewer projects, because they will be processed faster.

The second consideration in order to avoid congestion is to realise that the capacity of the groups involved in technology development often also needs to have the capacity to cope with product adaptations, maintenance and needs a buffer to cope with unforeseen uncertainty.

7. Evaluation

While it may be a short afterthought to the issue of technology strategies, it is important to mention that the loop needs to be closed. A technology strategy will be effective when it is regularly reviewed and when the results of the technological projects that are the expression of the technology strategy are compared to the overall goals of the organisation. At the same time such a review by a technology steering committee will offer the opportunity to check to what extent the learning through the technology development leads to new opportunities for the strategy of the organisation. It is in this evaluation process that the two purposes of a technology strategy, mentioned at the start of this chapter, will come to full fruition.

8. Conclusion

While there is more to be said about technology strategy we want to finish with a few summarising statements.

The operational expression of a technology strategy is the set of projects that an organisation wants to implement. Determining a strategy is selecting the projects and the portfolio of projects. In this chapter we did argue that this selection is both a decision and a process. It is a decision because the organisation needs to make resource commitments, but it is also a process of constant evaluating whether the projects fit the strategy of the organisation and whether the organisation has the capability of bringing the projects to a successful end.

Tools and techniques exits to support the management team in the decision making process, but the acceptance of these tools and techniques driven as much by the quality of the tools as by the quality of the technology transfer process that makes these tools palatable to the managers.

The project portfolio cannot be disconnected from its context. Strategic context and leadership, an environment that stimulates creativity, an acute awareness of the capacity of the organisation and a commitment to avoid congestion, a clear understanding of the complementary assets and their availability are a few examples of how the context influences the shaping of a technology strategy.

And finally there is no technology strategy without risk. Taking risks requires people to commit themselves. Technology strategies without technological leaders who are willing to take risks are just documents.

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