Growing into new businesses: 
have faith in strategic planning basics

Andrew Campbell

This article was published, with some editing in Strategy and Leadership, Vol33, 2005.

When the core businesses start to slow down managers turn their attention to new businesses. However, despite investing considerable resources in the search for new growth most fail. Companies as successful and as different as Intel and McDonald’s have found it painfully difficult to develop new businesses. The oil companies in the 1970s, bulk chemical companies and cigarette companies in the 1980s and computer hardware companies in the 1990s have all expended billions of dollars on new businesses with few successes. It is therefore important for managers to have research-based guidance about how to address what is probably one of the toughest of challenges in management—how to find sources of new growth when the core business is maturing.

In much of the current literature, managers are encouraged to set up venturing units, to run idea generation workshops, to stimulate more innovation and entrepreneurial behavior, to develop “third horizon” projects, to learn how to nurture new businesses, to transform their cultures and to create a Silicon Valley within their companies where managers with ideas can market their projects to managers with money. In sum, managers are urged to do more: invest in more projects and take more risks.

In contrast, our research suggests that this advice does not work; at least it does not help companies develop new legs. The research was collected from shadowing managers responsible for developing new businesses, surveying corporate venturing units and assembling a database of more than 50 significant success stories (see box, “Research methods”)

Research Methods

The purpose of the research was to understand why managers find it difficult to enter significant new businesses, and then to understand why some companies succeed. We defined “significant new business” as a business unit with a business model different from that of the core businesses that is worth 20% of the company’s market capitalization or $1 billion, whichever is the smaller.
The field research involved three elements. First we shadowed managers responsible for finding and entering new businesses. We interviewed these managers every 3 or 4 months for an average of 2 years, to find out what issues they faced and how they thought about the challenge. Second, we surveyed, together with Julian Birkinshaw at London Business School, over 100 corporate venturing units and corporate incubators. We wanted to find some that had created new legs for their parent company and we wanted to understand why most are unsuccessful. Third, we assembled a database of over 50 stories of companies that had successfully developed or acquired a significant new leg. We interviewed managers in about half of these stories and we tested our emerging hypotheses against this database.

All three research avenues pointed to the same conclusion—managers need to assess opportunities more strategically and be less activity driven.

Our shadowing research included companies like Shell and McDonald’s. In every case, we concluded that managers were investing in too many projects, most of which had little chance of success. In the case of one UK company, 24 significant ideas were identified and examined and 11 were launched as new ventures. However, when put through a screening process developed by the research team (see below), only one venture passed the screen and two others were marginal. In the three years since, eight of the projects have been closed down, some with losses in the hundreds of millions of pounds sterling. The remaining three are either still small or still high risk.

The survey of corporate-venturing units also pointed to a need for better strategic selection. During the second half of the 1990s, most companies launched one or more corporate-venturing units or corporate incubators. Normal rules of corporate risk aversion were suspended. Corporate funds and third-party venture funds were available for all promising projects. Many of these units mimicked the processes and methods of the venture-capital industry. In other words, the companies were following current wisdom. Yet, out of a sample of more than 100 units, less than five significant new businesses were created for the parent company. Moreover, it was apparent that the total costs far exceeded even optimistic estimates of the value created. It turns out that corporate venturing does have a place, but not as the savior of mature companies.

Finally, our database of successes points to the value of strategic planning. Of the sample of successes, which was biased towards examples of diversifications rather than extensions, more than 70 percent originated from a process of strategic planning. Around 20 percent were opportunistic. None of the examples started life as a result of an internal program to create a “third horizon” or an effort of cultural change aimed at generating new businesses or a deliberate program of investing in a portfolio of promising new ideas. HP’s move into computers, IBM into business consulting and GE into financial services and media are examples.
Five rules for assessing growth opportunities

Most books and articles implore managers to devote more effort to innovation, experimentation and new business venturing. In contrast we believe that the vast majority of successful new businesses come from the more traditional route of strategic planning. Moreover most of the failures are failures in strategic selection rather than failures in entrepreneurial management, failures that are at least in part the result of excessive optimism and risk taking. We have, therefore, developed five rules that guide managers to be cautious and thoughtful as they search for growth opportunities.

1) Continue to invest in the core

The first rule is to caution managers to continue to invest in the core business. Writing in a business column for the *Independent on Sunday* titled seductively “My biggest mistake,” Sir Peter Walters, a former CEO of British Petroleum (now known as BP), described attempts by his company to enter new business areas. Driven by concerns about the future of the oil industry following the 1970s oil shock, BP entered nutrition, IT, minerals and other businesses either because of the similarity of the skills required (minerals) or because of their growth potential (IT software). After describing the failures, he concluded: “If we had put even half the effort into our core businesses that we put into new businesses, we would have come out ahead.”

This is the same story we have heard again and again from companies that have lost faith in their core business. A manager from Courtaulds, a British fibers company, described it as a “strategic round trip.” “You decide that the core is not growing fast enough,” John Steen explained. “You invest in new businesses. They under perform; and seven years later you are worse off than when you started having closed or sold the last of the new initiatives. I have seen my company do this three times.”

In *Profit from the Core*, Chris Zook has made much of this point. Yet most companies have still not got the message. Both Intel and McDonald’s suffered from devoting too much attention to new businesses. Throughout the 1990s, Intel managers talked about Job 1 and Job 2, to emphasize the twin challenges of continuing to focus on the core microprocessor business (Job 1), while actively seeking new businesses to enter (Job 2). Looking back, Intel would have been more successful if it had spent more time on Job 1 and less on Job 2.

Jim Cantalupo, former CEO of McDonald’s, drew the same conclusion when he was appointed to the top job. He argued that managers were distracted from running the core business properly because they were pursuing growth too energetically. This was not just a factor of new businesses: Cantalupo also cut back new store openings in the core hamburger business.

As former BP CEO Walters pointed out, the real cost of investing in new businesses may be distraction from the core, rather than money lost from a failed venture. Hence, in contrast to many authors, we counsel managers to give first priority to the core. New businesses should only be on the agenda...
when the challenges in the core do not demand the full attention of top management.

2) Don’t be seduced by sexy markets, but recognize rare games

The second rule is another word of caution - beware sexy markets. In one company, the head of the corporate development division was focusing on telecommunications and Internet opportunities because these were the only industries that were growing fast enough to allow for the creation of large amounts of value in a short time. The result: when the investments failed, the firm took more than $700 million in write offs.

In another company, the focus was on diversification into biodegradable products. Growth in this sector was expected to accelerate as waste disposal legislation tightened. A suitable acquisition was identified by the business-development team, but rejected by a cautious board. Subsequently, the target company was forced to close when too many competitors entered the market.

Most text books encourage managers to focus on growth markets. The problem is that these sectors attract the most competition. To create value, managers should be focusing on markets where they have an advantage, rather than markets that are growing. In fact it may be easier to create value in less popular areas than in businesses-of-the-future that attract every growth-hungry manager. What managers should look for are opportunities where their company can bring some special resource or competence to the game.

There are only two exceptions to this advice: “dog markets” and “rare games.” Dog markets, where most competitors currently earn less than their cost of capital or are likely to do so in the future, are characterized by intense competition. Airlines and steel are good examples as were semiconductors in the 1980s. Everyone agrees that dog markets should be avoided whether the company has advantages or not.

In contrast, rare games are markets where even average competitors are likely to do well. They normally occur because a new market suddenly opens up creating more demand than supply. Those companies who get in early have an advantage in terms of written-down assets or stronger brands. Internet service provision and telecommunication fiber were examples in the 1990s. Our observation is that managers and text books do not give sufficient attention to rare games.

Another reason for rare games is the behavior of competitors. Sometimes they price high, misguided by making room for new entrants. Sometimes they are so hampered by legacy processes, managerial overhead and outdated habits that unencumbered competitors have an advantage. These conditions have existed in financial services in many countries, allowing new entrants such as GE Capital and even supermarkets to enter the business and do well.

A company can enter a rare game without an advantage because it is a favourable environment. To be sustainable, however, the company needs to
create an advantage during the first few years. This normally comes from early mover benefits.

3) Look for advantage, don’t play the numbers game

The third rule directly contradicts much of the current literature. The advice from most authors on the topic is that managers need to have a lot of tries to be sure of getting one success. Or as Kirby Dyess, a former senior Intel executive put it: “In order to get a home run, we need to go to bat multiple times.”

In contrast, our third rule discourages managers from attempting many tries and from even considering this mindset. If the presumption is that many failures will produce a few successes, each new initiative will get back-of-the-hand support from the organization because it will be expected to fail. A numbers game mentality contributes to failure rather than success.

The alternative is to be selective: to invest in opportunities only when the company has a significant advantage. Sometimes this may mean no new initiatives. At other times it may mean two or three. But, for a particular level in the organization, it should never be 10 or 12 or, as in one company, 44.

Take the analogy of new product launches. Every marketing manager knows that the failure rate is high. Yet each new product launch has the full support of the organization. As a consequence, businesses rarely launch more than two or three new products a year. If you are going to give an initiative your full support, you cannot do it to more than two or three at a time. This forces managers to weed through their ideas for the very best, and then to try to make them work.

So, when does a company have a significant advantage? The answer is when it believes it can serve the market and earn 30 per cent better margins than competitors. When Rentokil, a company in pest control, considered entering the office plants business, its advantage was its business model. Managers believed they could charge a 30% premium by offering a higher quality service. All other things being equal this would translate to a 200% increase in profits.

When Kelloggs considered buying Keebler, the cookie company, managers knew that they could drive significant additional volume through Keebler’s distribution system: they could add Nutrigrain and other products to Keebler’s portfolio. In addition, they knew that Keebler’s strong direct sales and merchandising force would increase the sales of these products. All other things being equal, these changes could add 50 -100 % to Keebler’s profits.

However, for most new business projects, the 30 per cent challenge is too great. Either an existing competitor has many of the same advantages or it is possible to identify other likely competitors with similar advantages. When British Airways launched its low cost airline GO it had advantages. The parent company could give the new business some landing slots and help with operational support. But, compared to established competitors such as Ryan
Air and Easy Jet, these advantages would not have met the 30% hurdle. As a result British Airways struggled to make GO a success. After a spin out GO ended up being acquired by Easy Jet. Optimists can persuade themselves that they have a 10 to 20 per cent advantage. But a 30 per cent hurdle is high enough to require more than optimism.

4) Be humble about your skills

The fourth rule is another rule of caution: be humble about the current skill set, at least with respect to the new business. One of the reasons why the advantage hurdle needs to be set at 30 percent or greater is because managers are frequently overly optimistic about their skills. For example, the Rentokil pricing advantage only translates into a 200 percent increase in profits if all other things are equal.

Normally, however, all other things are not equal. Competitors may have advantages that are difficult to observe and copy and managers have much to learn in the new market. In unfamiliar situations, managers make mistakes; they invest too much in marketing, over design products, mismanage suppliers and commit other rookie errors. This is why the advantage needs to be at least 30 percent. Moreover, if the market is very different from existing markets, the expected advantage needs to be at least 50 percent to cover for the higher learning costs.

It is the learning costs that explain why companies find it so difficult to diversify. It is obvious in sport. We would expect a team skilled at soccer to perform pretty poorly playing American football, and we would not expect the Olympic table tennis champions to be able to qualify for the tennis event. Yet we often expect similar leaps of skill when entering a new business.

The emphasis we place on learning costs adds is much greater than that of other authors. It is normal to assess the degree of fit that a new business has with existing skills and to try to avoid businesses that demand wholly new skills. It is normal to put a contingency into the business plan. But we have not found examples of managers trying to estimate the likely learning costs and discounting their expected returns as a result.

Learning costs upset many plans, but they also suggest one type of attractive new business idea to search for—opportunities we call “saplings.” Saplings are operating units that already exist within the company for historic reasons or reasons connected with the core businesses, that have the potential to be developed into businesses. Their special feature is that they have, for whatever reason, especially strong managers with insights about how to grow the activity. Their other big selling point is that most of the learning has already been done.

Hewlett-Packard first started manufacturing processors for its instrument business because it could not get reliable supplies. In the 1980s, after many requests, the unit was given the freedom to sell computers to third parties. Now, HP is one of the world’s largest computer manufacturers.
GrandMet, the hotel company, acquired the IDV wines and spirits business in 1974, along with its bid for a brewer called Trumans. Management tried to sell IDV to pay down debt, but failed. Twenty years later, prior to its merger with Guinness, IDV had become the largest business in GrandMet. Understanding the impact of learning costs can help managers discover good prospects as well as guide them away from potential disasters.

5) Focus on people as much as potential

Our fifth rule is to encourage companies to search for entrepreneurial managers with ideas as much as opportunities in the market place. Most authors emphasize the search for opportunities, suggesting deep analysis of the industry, workshops to generate ideas and early experiments. We suggest that it is better to start with a search for managers who are capable of an willing to lead a new business.

The venture capital industry has a saying that there are only three things to think about when selecting projects to support – management, management and management. The same applies to new businesses inside larger companies.

Large companies are lulled into presuming that somewhere within the huge pool of talent managers can be found to lead almost any project. The challenge, they believe, is to find good projects for them to lead. While this way of making strategy works within an existing business, it does not work when the challenge is new businesses. Within an existing business, there are many managers who understand the products, the markets and the essence of the business model needed to make a profit. In new businesses all three of these areas of knowledge may be absent.

We therefore caution managers against exercises that generate large numbers of new ideas. They build enthusiasm for some new opportunities before the managers who can exploit the opportunity have been identified. This often leads to projects starting out with the wrong leadership. A company may have a huge advantage in technology or brand or customer relationships, but, if it does not have the managers with the talent, experience and will to exploit these advantages, no value will be created.

In all of our research sites management was an issue. In some, like Egg the internet bank created by Prudential, management was the key to success. Mike Harris, hired in to run the bank, was about the only manager in Britain who had had the experiences needed to make a success of the opportunity. He had previously run First Direct, Britain’s first major direct-to-consumer bank. He had also been involved in other technology driven start ups.

“We had great ideas, but we did not have managers with the right entrepreneurial skills to exploit them” was a common theme in our research sites. In one case, the company launched two new businesses and the head of business development became chief executive of both. A year later this manager was moved to run a larger division. In another company, the chief executive confided that his failure to succeed in mobile telephones was greatly affected by not having a “strong enough leader” for the business. He explained “It is not just a matter of technical talent. You need someone with the will to win, someone who can get the rest of the organization to support the project. These people are rare.”
It is the rarity of these people that drives the need to focus on people as much as potential. Any process to search for new business ideas should be complemented by a search for managers with the experience, talent and passion to lead. In fact, since these managers are likely to have their own ideas about what sorts of new businesses they want to create, it is often better to start with the search for talent rather than a search for opportunities.

6) Be realistic about ambitions

The sixth rule is to be realistic about ambitions. There is a macho attitude in management thinking that argues for setting high targets. In *Good to Great*, Jim Collins refers to BHAGs (Big Hairy Audacious Goals). Managers, he argues, achieve more dramatic advances when they set audacious goals. Leaders of companies, like Intel and McDonalds, who are facing a big growth challenge, are advised to set a big goal. “Create a billion dollar business within five years.” “Raise our growth rate from 10 percent to 15 percent in the next three years.”

Our view is that stretch goals cannot be used to drive the creation of new businesses. Stretch goals work in existing businesses because managers use them to get unstuck from ruts; they stimulate breakthrough ideas. Moreover, their knowledge of the existing businesses enables them to be wise about which breakthrough ideas are foolish and which are gems.

In the search for new businesses, managers are not stuck in ruts. So they do not need stretch goals to generate breakthrough ideas. Moreover, they are less able to be wise about which breakthrough ideas are practical. As a result they invest in too many losing projects. In our view, goals should be set only after the opportunities for new businesses have been screened, not before.

One director of corporate development was asked to generate $500 million of operating profits from new businesses in five years. He made the target more manageable by setting himself the objective of creating 10 new businesses each with $50 million of profit. But the target pushed him into risky investments that resulted in $500 million of write offs. While this was a particularly bad outcome, similar stories emerged from many of the other companies.

The need to avoid setting ambitious goals has major implications for the way that efforts to get into new businesses should be organized. If managers create a new businesses division or even a development unit, pressure will mount from bosses and subordinates to set targets and define objectives. This was an issue in most of the companies shadowed in the research. Where targets were set, they were more hindrance than help, interfering with objective assessment of the opportunities rather than stimulating additional ideas. The task should be to identify opportunities and see if any of them are worth supporting: to explore the potential rather than fill a growth gap. The task of exploring new businesses is, therefore, likely to be better done as a project team with a finite time frame rather than by setting up an organization unit.
In addition, managers need to be prepared to discover that there are no new businesses that the company should invest in at the moment: that the project team may come up with a blank. Fear of this outcome is one of the reasons why managers set up new businesses divisions. The expectation is that there will be a stream of new projects or acquisitions to invest in. Our research suggests that this is irrational. The evidence points in the opposite direction. Research from the Executive Board and academics such as Clayton Christensen indicate that 90 percent of companies that try to develop new businesses as a solution to a slow growing core fail. The traditional explanation is that these companies are bad at developing new businesses. However, our conclusion is that these companies fail because there are no opportunities for new businesses that fit their skills.

The alternative – giving money back to shareholders via earnings or buying back shares is no defeat. It is the right thing to do – at least until an opportunity that is likely to succeed emerges. Moreover, it is possible to produce acceptable performance for shareholders even with low revenue growth. For example, over the last ten years IBM has grown in line with the economy, but, by buying back more than 25 percent of the company’s stock, it has produced a total return to shareholders in the top quartile.

The strategic business case

We believe that managers can be much more strategic about investments in new businesses. At Ashridge Strategic Management Centre we have developed a screening tool – The New Businesses Traffic Lights. The tool can be applied to an idea before a business plan has been developed. It can also be applied alongside a business plan to assess the strategic logic for the proposal. It can even be applied to an existing investment that is failing to meet its short-term targets to see if the strategic logic is still sound.

The Traffic Lights tool is a distillation of our strategic thinking. There are four elements: the size of the profit pool, the size of the value advantage, the quality of the managers of the new business and their corporate sponsors, and the likely impact on the firm’s existing businesses (see exhibit). Each of the four elements is scored red, yellow, or green. Red indicates that the market segment has little available profit, or that the company’s value proposition offers no significant advantage, or that its managers and sponsors are inferior to those of existing incumbents, or that there is a major negative implication for its existing businesses. Any one red light is enough to stop the company from investing in the new business, even if some of the other lights are green.

Green indicates that the market segment is an easy one to make money in, or that the firm’s entry proposition has a compelling advantage, or that its managers and sponsors are clearly superior to those of the leading incumbents, or that there are big potential benefits for its existing businesses. Any one green light (without any red lights) suggests that the company should invest so long as a viable business plan can be developed.

Situations that have all yellow lights are marginal. Ideally managers should be asked to reformulate their idea or business plan so that at least one light can
be green. Alternatively, an experimental investment can be made in the hope that experience will demonstrate that one of the yellow judgments was too pessimistic.

Applying this screen to the portfolio of new business investments in most companies will result in red lights for many projects. Our conclusion is that companies are taking too many risks in their search for new growth businesses. Either they are driven by a commercially irrational concern about survival or they have been encouraged to make too many investments by their advisors. The potential for improvement is huge. Not only can significant money be saved from the “new businesses” budget, but extra resources can be focused on improving the core businesses, an activity that is often temporarily neglected and likely to yield good returns.

McDonald’s seems to have learned this lesson. Its core business growth is improving and its five remaining new businesses, including Chipotle the Mexican restaurant chain and Pret-a-Manger, the British sandwich business, have been placed in McDonald’s Ventures. The CEO has been asked to decide if any can become significant for McDonald’s without distracting from the core hamburger business. Those that cannot will be divested.

Intel, on the other hand, is still chasing new businesses. Its current focus is more limited in ambition than its previous attempts to become an Internet company. Nevertheless, Intel is committed to four or five significant new businesses, such as chips for flat-panel TVs and mobile phones. Some of these are likely to pass the Traffic Lights. Others probably will not. A little more basic strategic thinking and a little less risk-taking would be likely to result in fewer new businesses, releasing more cash for investing in the core or repurchasing shares.

Two companies with growing pains

Imagine you are a senior manager in McDonald’s or Intel. Both companies have had very successful histories. Intel was started in the 1970s as a producer of integrated circuits for memory products. In the 1980s, Intel focused on microprocessors and by 2000 the company was worth nearly $400 billion. McDonald’s was founded in the 1950s. Since then it has become the world’s best-known fast food restaurant, and in 2000 had grown to a market capitalization of over $60 billion.

Both companies today, however, face a tough challenge: the future does not look as good as the past. Unless these companies can find some new avenues for growth - some new businesses to enter – they can expect to become less exciting both for investors and employees.

Fortunately both companies are positioned in industries that are growing. The demand for semiconductors and good value restaurants is expected to continue to expand. But the segments in which Intel and McDonald’s are strongest - microprocessors and hamburgers - are weak. In fact recent performance has
been poor. In 2002 Intel’s net income was a third of its peak, and in 2003 McDonald’s published its first quarterly loss for 40 years. Furthermore, since the mid 1990’s, both companies have devoted significant resources to the quest for new growth businesses with little success. So what should they do now? Based on our research we suggest they follow six rules as they search for new businesses:

1) Continue to invest in the core business.
2) Don’t be seduced by sexy markets, but recognize rare games.
3) Look for advantage, but don’t play the numbers game.
4) Be humble about your skills.
5) Search for people as much as potential.
6) Be realistic about ambitions

2


For further information contact:
Ashridge Strategic Management Centre
3 Devonshire Street, London W1W 5DT
Tel: +44 (0)20 7 323 4422