Why Is It So Hard To Find New Growth Platforms?

By **Professor Andrew Campbell**

Companies as different as McDonald's, Intel and Shell have all invested significant sums, in the last 10 years, looking for new sources of growth. All have failed and at some cost to their managers, shareholders and society at large. Moreover, these companies are no exception. Research suggests that 90% or more of companies fail to find new sources of growth as their core businesses mature. If we could better understand why it is so difficult to find new growth platforms, we would be better armed to deal with the problem.

Much of the current literature argues that the high failure rate, when looking for new sources of growth, is due to poor processes and skills. As a result, the traditional advice is for companies to take more risks, become more entrepreneurial, copy some of the approaches used in the venture capital industry and build a pipeline of new businesses.

Our research suggests a different explanation. We have observed that established companies have established managerial habits, rules of thumb and mindsets. For example, Intel is fanatical about safety and first-pass yields. Shell has a habit of investing in research when it faces a problem. McDonald's has had a commitment to decentralisation and to growing faster than its rivals.

We have observed that companies are successful in new growth areas when the critical success factors in the new business fit their existing mindsets. Failure is often the result of trying to do things that do not fit. In other words, a company's mindset is normally well tuned to the needs of the *existing* businesses, but often get in the way when the company tries to enter new areas. The problem this creates for many companies, such as Intel or Shell, is that very few *new* growth areas both fit with their existing mindsets and are significant in size.

This shortage of opportunities theory is an alternative explanation of the low success rate. It suggests a different way of approaching the problem. It suggests that efforts to generate additional ideas or to experiment with a portfolio of new ventures are likely to be fruitless. It suggests

CASE STUDY: INTEL

Intel has been trying to find new growth around its core microprocessor business for 20 years. Some initiatives were intended to help extend the core into new areas, while others concerned growth opportunities outside the core. Intel managers even labelled these two types of initiatives as Job 1 (extending the core) and Job 2 (beyond the core).

In 1993 Frank Gill took charge of Intel Products Group (IPG), the unit with responsibility for most of Job 2. It included new businesses such as motherboards for PCs, LAN adapter cards, fax modems, PCMCIA cards, video conferencing and massively parallel supercomputers. One of Gill's subordinates Jim Johnson explained "Andy Grove [the CEO] wants a second product line to balance microprocessors".

By 1999, however, little progress had been made. "Despite the emergence of many new opportunities across the computing industry, and the emergence of many new ideas within the company, Intel had had difficulty turning these opportunities into successful stand-alone businesses", concluded Professor Robert Burgelman in a book devoted to Intel's new growth initiatives. Andy Grove was less diplomatic "I tried like hell to develop new business opportunities, but they almost all turned into c**p."

Since then Craig Barrett, who became CEO in 1998, also tried to create new businesses focusing on the Internet. The language changed. The core was labeled the 'blue' business and new businesses were called 'green'. A New Businesses Group was created at the highest level. Barrett freed up his own time to work on new businesses. But, the success rate did not improve. Intel lost US$1billion on web-hosting and generated little profit from its other initiatives.

Today the company is still trying to find new growth. Now the focus is on chips for different applications, like mobile phones and flat panel displays, but without a major success so far.

that less risk aversion and more tolerance of entrepreneurial initiative is also likely to fail. The solution is either to change the corporate mindsets or to search carefully for and invest only in opportunities that fit closely enough to have a chance of succeeding.

Our research suggests that changing corporate mindsets is unlikely to be the solution for most companies. Dramatic changes to mindsets are rare. Short of changing many senior managers, the company's ways of working and rules of thumb continue to influence behaviour. Moreover, making big changes to the way managers work is risky: the habits and mindsets are usually needed to extract top performance from existing businesses.

Our conclusion, therefore, is that the solution for most companies is to be more realistic and more selective about their growth ambitions.

As part of our research, we shadowed managers in companies, like McDonald's and Shell, during periods when they were trying to develop new businesses. In every case, we concluded that these managers were investing in too many projects, most of which had little chance of success. In one case, 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 **Figure 1**), only one small venture passed the screen; two others were marginal. In the three years since, 8 of the projects have been closed down, some with hundreds of millions of losses. The remaining three are either small or still high risk, and, in aggregate, do not make a profit. In other cases there were many more new initiatives (one manager referred to 361 proposals his department had processed), but only a few passed our screen.

Our second research avenue involved assembling a database of successes. Most successes appear to be the result of careful strategic choice rather than multiple experiments or portfolios of initiatives. Of the sample of more than 50 successes, which was biased towards examples of diversification rather than extension, more than 70% originated from a process of strategic

OUT OF A SAMPLE OF MORE THAN 100 UNITS, THE NUMBER OF SIGNIFICANT NEW BUSINESSES THAT WERE CREATED FOR THE PARENT COMPANY WAS LESS THAN 5

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planning: thinking through the options and choosing one or a few new businesses to invest in.

Centrica, a British retailer of gas, which had previously been part of a nationalised industry, is a good example. Faced with new competition in a previously protected market, Centrica forecast tighter margins and reduced volumes. This spurred managers into a major review of what the company should do. The review was divided into an upstream project focused on gas supply and storage and a downstream project focused on retailing and customer relationships. Many new business opportunities were considered, such as call centre outsourcing building on the company's experience servicing its 17 million customers.

The review concluded that the company should expand its gas asset interests, enter the business of retailing electricity, explore retailing telecoms and look for acquisitions of companies with business models that involved a mobile workforce and large call centres. The latter thought led the company to acquire AA, the UK motor rescue and financial services business. While some of these strategies have not progressed as well as hoped, the overall result turned a company with declining prospects into a top quartile performer.

HP's move into computers, IBM's success in business consulting and GE's development of financial services are also examples of decisions that were driven by careful strategic choice. In all three cases, the company already had some activity in these new areas that could be built on: in house sourcing of CPUs at HP, an unloved consulting and services activity at IBM and customer financing at GE. However, these seeds were not sown deliberately. They were resources these companies could build on that, at a certain point in time, presented a significant opportunity and a fit with the company's managerial approach.

None of the examples in the database started out as part of a corporate incubator, as a result of an internal programme to create a 'third horizon', as an effort of cultural change aimed at generating new businesses or as a deliberate programme of investing in a portfolio of promising new ideas. About 20% of the examples were more opportunistic than planned. For example, Royal Bank of Scotland's decision to invest in Direct Line was clearly opportunistic. Peter Wood was looking for a company to back his idea. He happened to know the manager who had become Finance Director of Royal Bank of Scotland. One day, when he was 'playing golf' with this manager, he suggested that Royal Bank support the project. Twenty years later Direct Line is one of Royal Bank's most successful businesses.

Our third research avenue involved surveying, together with Professor Julian Birkinshaw of the London Business School, the successes and failures of corporate venturing units. These units operated under different rules from their parent companies. In fact, they were often set up to avoid the mindset constraint of the existing businesses. 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, they were following current advice. They were investing in more projects, taking more risks and trying to create the seeds that would lead to significant new businesses.

Yet, out of a sample of more than 100 units, the number of significant new businesses that were created for the parent company was less than 5. Moreover, it was apparent that the total costs far exceeded even optimistic estimates of the value created. Our conclusion was that these units were not an appropriate way of trying to tackle the growth challenge. It turns out that corporate venturing does have a place, but not as a process for developing new growth businesses.

In contrast to the advice that fills most of the pages of most books and articles, the shortage of significant opportunities that fit appears to be the biggest challenge for managers seeking new growth. The implication is that managers need:

- a better way of identifying opportunities that fit (see **Figure 1.**). This will reduce losses from failed initiatives and give real focus to any programme of new growth
- more patience. Instead of trying harder by launching more projects, managers need to do less, and often for a number of years

FIG 1: THE NEW BUSINESSES TRAFFIC LIGHTS

The researchers developed a screening process for new businesses that could be applied to projects prior to the development of a business case. Since the process of developing a business case can take up significant resources, and often builds momentum behind a project that becomes hard to kill off, the team wanted to find a screen that could reject projects earlier in the process. The purpose of the screen was to identify whether managers were investing their time in suitable projects. The screen involved four Traffic Lights:

1. Does the company have a significant value advantage (green), a small or uncertain advantage (yellow) or a disadvantage (red) when compared to likely competitors in this new business? This involves judgements about the special contribution the company can make, about the percentage of the contribution that can be turned into value without the risks of entering the new business and about the likely costs of learning the new business.

2. Is the profit pool for this new business average (yellow), 'a rare game' (green) or 'a dog' (red)? Judgements about variables, such as Porter's 5-forces, were critical to this assessment.

3. Does the company have leaders of this new business (and sponsoring managers in the parent company) who are especially insightful or skilled (green), average (yellow) or less skilled than likely competitors (red)? Judgements about the status, drive, business acumen and knowledge of the market, technology or business model of the likely leaders were critical to this assessment.

4. Is the impact of this new business on the existing businesses likely to be significantly positive (green), small or uncertain (yellow) or significantly negative (red)? Judgements about the likely distraction effects and the synergy effects were needed to make this assessment.

Any green light, with no reds, signalled a good project. Any red light signalled rejection. All yellow lights signalled a maybe. As we shadowed managers responsible for developing new businesses, we found that few, if any, of the ideas they were investing in were good projects.

Our research suggests that it can take 5, 10 or more years for a particular company to develop a significant new growth opportunity that fits

- a willingness to reject all projects, give excess money back to shareholders and focus on optimising the core business. This will require a shift in thinking about growth and shareholder returns. Instead of seeing growth as a do-or-die imperative, managers need to understand that they can generate excellent returns for shareholders with a profitable mature business that distributes its excess cash

We are often accused of being defeatist or anti-growth. As one manager put it, "you are asking us to commit premature suicide". However, managers who face up to the reality of 'few significant opportunities that fit' will find they are better armed to help their companies succeed than those who believe that more risk taking and more new ventures are the answer. In fact, investing unwisely in new businesses is the most common form of corporate suicide. By keeping their powder dry, realistic managers will be ready to invest when a good opportunity arises and they will still have the trust of their shareholders and partners to help them exploit it to the full.

Andrew is Director of the Ashridge Strategic Management Centre and author of many books and articles. This article is based on 'The Growth Gamble: When Leaders Should Bet Big On New Businesses And How To Avoid Expensive Failures', April 2005 co-authored with Robert Park. He can be contacted at andrew.campbell@ashridge.org.uk.

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