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the innovation master plan

the CEO's guide to innovation

by langdon morris

this excerpt includes

chapter 3

Langdon Morris is recognized worldwide as one of the leading authors and consultants in the innovation field. A new chapter of his latest book, *The Innovation Master Plan*, will be presented exclusively at InnovationManagement.se every two weeks throughout the summer.
It will soon also be available at Amazon.com (but not quite yet).

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The Innovation Master Plan

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"I have missed more than 9,000 shots in my career. I have lost almost 300 games. On 26 occasions I have been entrusted to take the game winning shot ... and missed. And I have failed over and over and over again in my life. And that is why I succeed."[1]

Michael Jordan [in a TV commercial for Nike]

Innovation, like basketball, is inherently risky. You never know in advance who will win. Would-be NBA champions miss shots and lose games, while would-be innovators risk money and time, possibly a lot of both, to create, explore, and develop new ideas, hoping to turn them into innovations. But regardless of how good they are at the game of innovation, many of the resulting outputs may cost a fortune and never earn a dime.

The larger game of business in which innovation is pursued is inherently risky, too. You never know what your competitors are going to do, or what changes are coming to the market, or how soon your existing products and services will become obsolete.

From a manager's perspective this presents a daunting challenge and a critical dilemma that arises every day: in what aspects of my business should the innovation effort be focused? Should we apply ourselves to innovation in our products or our services, or our brand, or the organization itself, our leadership team, our technology, our capital structure, or any of the countless others among all the possible targets?

While any one among this broad range of possible innovation targets may be important, no target is guaranteed to be the right one to pursue, the one that will inevitably result in success. Consequently, it's far better to develop a portfolio of innovations-in-process that can be brought forward to create change, or to respond to change introduced by others. This is an approach that calls for taking modest and disciplined risks to accelerate learning, so that when it's time to take bigger risks they're informed by what we've already learned, and the likelihood of success will be much greater.

In this way innovation reduces the overall risk for the future of your organization by creating new future possibilities whose time *may* come.

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1 http://www.youtube.com/watch?v=m-EMOb3ATJ0

In this chapter we address the *what* of innovation. The central organizing concept is the carefully designed collection of new ideas-in-progress through which an organization prepares its own future while also preparing to meet future challenges introduced by competitors. This is the innovation portfolio.

In subsequent chapters we'll get to the details of the development process, progressing from strategy to ideas to innovations, and after that we'll examine the development of the right organizational culture to do this work, and then the necessary infrastructure to support it.

purpose and method

The innovation portfolio has two *purposes.* First, it's a learning system that will enhance our efforts to achieve our strategic intents through innovation, by helping us to deal with the problems of uncertainty and change. Second, and at the same time, it also reduces the inevitable risks inherent in the innovation process itself, and the larger risks related to being in business in the first place, by organizing the process of creating new products, services, and business models for the future.

The *concept* of a portfolio is that it provides a proven method for managing assets in the face of uncertainty. Just as investors in all types of assets create portfolios to help them attain optimal returns while choosing the level of risk that is most appropriate for them, the innovation portfolio gives us a tool to do the same for the innovation projects we're working on.

The *method* of innovation portfolio management is to transform the strategic initiatives that we have already reasoned through in Chapter One into a learning process that consists of a disciplined and thoughtful experiments that balance failures at the small scale on the way to ultimate successes at the large scale. We will explore the nature of this type of failure in a bit, but first…

The *content* of the innovation portfolio is, of course, innovation ideas and projects, of which there are four different types.

the four types of innovation

It's obvious that a fundamental breakthrough is entirely different from a minor incremental improvement, even though both may properly understood to be innovations. Toyota's hybrid Prius car, an example of a breakthrough technology, required years of intensive engineering innovation and billions of dollars of patient capital; the same company comes out every year with a new color palette for its cars, and changes the shapes of hoods, trunks, and headlights, which are of course entirely incremental changes.

These are the two obvious types of innovation, but there are also two other types that Toyota has used very successfully, as we see with its luxury Lexus and entry-level Scion brands. (If you are outside of the USA then you may not know about Scion, as it only exists in North America. It's a Toyota subsidiary that sells only less

expensive cars, which places it at the opposite end of the market from luxury-oriented Lexus.) Lexus and Scion cars do include some breakthrough technologies as well as a great many incremental innovations, but more importantly they are also "new venture innovations" because they are entirely new brands marketed by separate companies. The specific intent of new venture innovation is to extend the Toyota Corporation's life into the future for many decades.

According to the consulting firm Interbrand, which ought to know since brand valuation is what they do, the value of the Lexus brand, meaning the brand identity, name and logo as assets in and of themselves, as distinct from the revenue of the company, was about $3 billion in 2006, but then fell due to the global financial downturn and the massive impact that it had on the auto industry; it will most likely rise again.

Further, Lexus and Scion are also aspiring to be different *kinds* of car companies. They aspire to provide a different sort of experience to their customers, a better one of course, and in this respect can be thought of as "new business model innovations" because they are pursuing new ways of providing better experiences to earn higher profits.

This brief discussion of Toyota has shown how the company applies all four types of innovation to the development of its future value proposition.

Breakthrough innovations are the technical foundations of new companies and new industries. We pursue breakthroughs to attain major competitive advantage. Toyota, as world leader in hybrid engine technology, has gained an enormous advantage from its investment in this particular technology. As of September 2010 the company had sold 2 million Prius cars worldwide.

Incremental innovations are minor improvements to existing business structures, processes, and products. We invest in incremental innovation to maintain or improve our market share. Toyota, inventor of lean manufacturing, is also the world leader in incremental innovation, leadership that came about through a dedication to continuous improvement that has endured for decades, following on the pioneering spirit of its brilliant originator Taichi Ohno.

New venture innovations extend existing companies into new territories, either literally or figuratively. We create new ventures to expand into new markets for long term benefit.

And **new business models** are new ways of making money by leveraging new or different customer experiences. Like breakthroughs, new business models can also transform the structure of a market by changing customer expectations, attitudes, and buying patterns. Examples of new business model innovations include online retail, which is of course fundamentally different from brick and mortar retail. Each has its advantages, but as we have seen with the success of Amazon.com, online book selling dramatically reshaped the retail book business by forcing many local bookstores out of business entirely, and even the huge national Borders and Barnes & Noble chains are operating under severe duress because consumer buying patterns have changed so much.

It's often the case that a particular innovation doesn't fit into only one innovation category. Sometimes a breakthrough innovation is so different from the existing business that it becomes a new company, and hence a new venture innovation. Sometimes new business models are also new ventures, while in other situations it's an existing venture that must find a new business model.

As you prepare your innovation master plan and develop your innovation practice, all four types of innovation will become relevant and important because more than one type of innovation is typically required to sustain success in the market. Long term survival will likely require all four.

This is true even when a technology breakthrough is what you're developing and what you plan to sell. An example is Xerox. Chester Carlson's technological innovation was a compelling breakthrough, and a testimony to his insight and persistence. The story also highlights the difficulties of forecasting the market for genuinely new products, as many industrial giants of the day, including IBM, Kodak, and GE rejected Carlson's offer to license his technology because they could not envision its future success.

When he finally did find a partner, you may remember that it was tiny Haloid Company that took him on. Together they soon discovered that getting the technology to market entailed far more than simply building new machines, and the success of the company in its early years was due as much to its innovative approach to distribution – leasing the machines on a per-use basis instead of selling them outright – as it was to the technology. So it was Xerox's business model innovation that enabled the technology innovation to succeed in the market.

Today, Xerox is not the dominant company it once was, and a small but important aspect of the Xerox story is that in 1975 the company was so successful and so dominant that it was literally forced by federal government regulators to license its technology to competing companies. With this strange and quite short-sighted turn of events the regulators proved to have a poor grasp on the vibrant dynamics of technology competition, and sadly the company's downward slide began. Within four years, Xerox's share of the U.S. copier market dropped from nearly 100% to less than 14%. Again and again we see the inexorability of creative destruction and it's many agents, some of them lurking behind you, in this case, tragically, in the form of your own government.

Xerox is still struggling today. Its stock has declined from a high of about $60 per share at the peak of the internet boom to around $12, and it's threatened by creative competitors whose own innovations in distribution and technology have largely surpassed Xerox's. But ironically, the problem was definitely not that Xerox management, in those heady days when the company was the resounding market leader, had failed to recognize the importance of innovation. In fact, they funded innovation generously, and quite intelligently. Technical innovations that surpassed the efforts of most other companies were constantly being created at Xerox's legendary Palo Alto Research Center, PARC, from which an amazing string of enormous breakthroughs in many dimensions of technology emerged (as I mentioned above in discussing the turnaround of Apple).

At PARC the first really usable PC was developed, initially by the scientists there

for their own use, after which it was turned into a product called the Xerox Star. Apple co-founder Steve Jobs was given a tour of PARC during that period, and what he saw in the Star inspired many of the design features and innovations that became Apple's Macintosh. The Mac was a huge success, and so was Microsoft's subsequent Windows operating system, which is also built on many insights and breakthroughs that originated at PARC. But the Star was not a success; in fact it was a resounding failure, for reasons that we will explore in Chapter 6, because they have mostly to do with Xerox's corporate culture.

Ford is another example showing the importance of pursuing multiple types of innovations. The original Ford cars of the early 1900s were certainly innovative for their automotive engineering, but equally important to the company's success was the innovative production process (the first vertically integrated assembly line), manufacturing standardization, the distribution system (the dealer network), the salaries paid to workers (double the standard wage of the era), and the company's pricing model, affordability. All of these innovations together enabled Ford to dominate the market.

Even Ford's choice of black paint was an economic innovation, part of his relentless strategy of minimizing costs. Fords were originally painted brown, until a company engineer pointed out that black paint covered better, and would therefore be less expensive. The point for Mr. Ford was definitely not the particular color, but rather his highly principled obsession with of cost control. He understood that lowering the cost of manufacture was the key to developing his company and the broader market, and he was absolutely correct.

So from 1903 through World War I, cost control was the driver, but in the 1920s the market was maturing and Ford's success as a cost-cutting pioneer wasn't enough, as customers began to choose cars for comfort and style rather than just cost and efficiency. GM had copied and largely caught up with Ford's innovations, and began introducing some of its own. A minor GM innovation with major significance was the availability of cars in colors other than black.

Ford did not adapt quickly, and began to lose market share as the Depression set in and his own inability to grasp change held the company back. The company was saved from bankruptcy by the enormous demand for military vehicles during World War II, but after the war was over the company was again too slow to adapt, and was nearly bankrupt again by the late 1950s.

The Ford story illustrates how each industry has its own rhythm of innovation, including both technical and non-technical advances. Some innovations are advances in materials and manufacturing, some in design and methods, some in branding and communication, some in distribution.

While Ford struggled, GM seized the advantage and held it for 75 years. But Toyota and Honda are the leaders now, based on decades of emphasis on quality, reliability, and design, and most recently on new engine technologies like Toyota's hybrid. GM's share of the American market declined from 50% in 1980 to less than 35% by 2000, and then to less than 25% by 2008, and 19% through the first 9 months of 2010.

We don't know which companies will lead the market in 2020 or 2030, but no one would be surprised if it's a different one than any I've mentioned thus far,

because sustaining market leadership from one market structure to the next is very difficult and happens only rarely, and we can't predict when the market structure will change.

Sears is another example of a leader that became an also-ran. I noted in the previous chapter how Sears grew arrogant and complacent, and lost touch with the innovation mindset that had characterized its early years. Wal-Mart, meanwhile, developed its business in smaller rural markets that Sears had abandoned or ignored. Sam Walton recognized that change, in the form of interstate highways, enabled rural stores to lure customers from very large distances to regional shopping hubs with many acres of parking. (In France, the founders of the now-global retail chains Auchan and Carrefour had the same realization.) As Wal-Mart expanded, the company focused on cost control, and developed innovative approaches throughout its growing supply chain, thereby lowering its operating costs and making it strong enough to compete with Sears and K-Mart in urban markets. As Wal-Mart grew Sears became a second-tier retailer, while K-Mart was eventually forced into bankruptcy; in desperation, the two laggards then merged.

Sometimes entire product categories become obsolete, and the buggy whip is often mentioned as an example.[2] There was also, not so long ago, a thriving business in ice distribution throughout the US, but that went away when the electric refrigerator arrived in the 1920s and 30s. Business historians will soon be adding the demise of the music CD to the list of sad stories, as it's now being displaced by digital music distribution. Likewise the video rental business, epitomized by Blockbuster, is being made obsolete by video streaming à la Netflix, Apple TV, Google TV, and others.

The key point of these last few pages is that companies don't sustain success by focusing on only one type of innovation, and likewise they won't be successful in the future unless they balance their innovation efforts across all four types.

Each type must be managed as a separate portfolio of projects that address the specific needs and opportunities that each type of innovation epitomizes, and taken together all four will also constitute the fifth portfolio, consisting of all the projects.

Each of these five portfolios will be carefully designed to balance offensive innovations, those that we think could change the market to our advantage, and defensive ones, those that we prepare in case we need them. We sometimes refer to these defensive projects as "spare tires."

spare tires

Do you carry a spare tire in your car? And a first aid kit? And do you have fire extinguishers in your home and office? Of course you do, (or if you don't you know

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2 A buggy whip was a very long whip used in the days of horses and wagons (buggies, or carridges) by the wagon or buggy driver to help control the horses. It is often used as an example of an industry that declined rapidly, in this case as a result of the success of Ford's Model T auto. Buggy whip makers had perfected their product by the 1890s, but it was entirely obsolete by 1910.

you should). You do this even though you hope that you'll never have to use them.[3] Similarly, some of the projects in your innovation portfolio will never be used, but you'll still have good reason to develop them.

The purpose of your portfolio is to prepare for a wide variety of possible future conditions, some of which will never actually emerge into reality. So while it's inevitable that some projects in your innovation portfolio will never actually become relevant to the market, it's necessary to work on them anyway. These projects will therefore never return value in and of themselves, but still they're not failures, they're better understood as precautions.

the necessity of disciplined failure

Some projects will have a clear and immediate goal, when you can see direct application and a straight shot from concept through development to market impact. Other projects, though, may be important not for the direct results but for the learning that you will gain. Such projects often involve a high degree of failure, necessary failure. And concerning failure, it turns out that there's a lot to say.

For organizations to deliver on the promise of innovation it's essential that their employees have the skills to effectively generate ideas, insights, and innovations, to understand, diagnose, analyze, model, create, invent, solve, communicate, and implement. These are all skills that we might consider facets of "learning."

Naturally enough, any organization that thrives in a rapidly changing environment necessarily has developed the capability to learn and to apply that learning to keep up with the changing world. Hence we get Arie de Geus' insight that the company wins that learns fastest, and is then able to translate that learning into products, processes, and structures: "Learning faster than your competition is the only sustainable competitive advantage," he said.[4]

Learning faster must also lead to *doing* faster, and the capacity to systemically reduce cycle time is also a valuable skill. Toyota perfected cycle time reduction in its many-decades drive to reduce the time required to change production models on its assembly lines from days to hours, and then minutes; our interest is in reducing the time needed to solve problems and deal with complexity through innovation. At root what we're talking about, then, could also be called "the productivity of the innovation process," and a portfolio is certainly a productivity tool.

"Productivity," specifically, is the ratio of results obtained to investment required; in the auto industry it's measured as person-hours-per-car-manufactured; in a bank, it is cost per transaction; and in a software company it could be person-years required to produce a new program, or debug it.

While these examples explain productivity in "production" environments, we're even more interested in attaining improved outcomes in environments that are full of

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3 The invention of the "run flat" tire may make the metaphor of the spare tire obsolete. This would be a nice example of how innovation changes the game in the tire industry, and thereby affects how we think about an entire product category.

4 Arie de Geus. *The Living Company*. Harvard, 1997.

unknowns, where the central tasks are to create new knowledge through learning, and to apply this knowledge to solve problems and create business opportunities. This is the classic domain of innovation, and here productivity is much more challenging to measure.

Innovation as an approach to learning, particularly as it relates to potential breakthroughs and new business models, entails uncertainty, and hence the necessity to try things that may not work, in order to discover what actually does work.

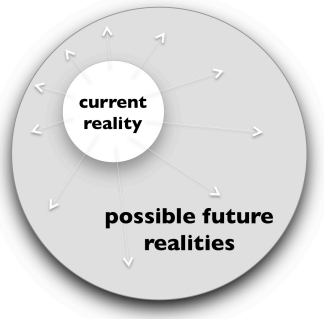

Figure 6
Possible Future Realities Outside of Current Reality

Further, preparing to meet and deal with the unpredictable future requires us to gain experience and knowledge through experimentation across a wide range of hypothetical situations. As they are hypothetical, many of them may never materialize, and as a consequence many of these experiments may appear to be "failures."

But if they've been conducted with suitable discipline and a learning outcome has been achieved, then they should actually be viewed as successes.

The degree of uncertainty that organizations face in the market will influence the breadth of failure that's necessary to pursue; the bigger the challenges and the faster the changes, the greater the breadth of failure you'll have to accept, or indeed, to embrace. So while you might think that you have to *endure* failure, the truth is that you need to *pursue* it.

The importance of these failures is so unmistakable and unavoidable that it's worth commenting about the mindset related to failure, and the prevailing cultural assumptions in our Western world.

Like you, perhaps, growing up in a success-oriented culture has indelibly planted in my mind the belief that failure is bad and to be avoided at all costs. If I fail it be may be a sign that I am a bad person, while success is culturally imperative, and a sign of goodness. We are obsessed with winners who succeed, and we scorn the losers who fail.

Causing failure is a further and deeper sign of inadequacy along any one of many possible dimensions of behavior, including poor preparation, lack of skill or talent, faulty reasoning, or bad judgment.[5]

Each of these deficiencies is inevitably uncomfortable and may be quite painful. Hence, when we fail in any of these ways we often feel angry at ourselves, and

5 Twyla Tharp. *The Creative Habit*. Simon & Schuster, 2003.

humiliated, and we may swear … "never to do that again. Ever!" Such is the stigma associated with failure, and the mountain of judgment that falls upon anyone who fails.

Now consider your own career. You aspire to be a top manager in a big company and you've succeeded at every level along your career path along the way, from schooling to the big milestone of the "first job," through the ranks and all the way up. You knew what to do, or you learned along the way, you planned it well and executed on it, and you succeeded.

This is not, however, the way it usually works with innovation. In the pursuit of innovation, failure may indeed result from all these sources and their insidious combinations, but it is still necessary. It is desirable.

Why?

Because some failures are entirely essential steps on the learning pathway that leads from a universe of uncertainty to clarity, capability, understanding, and thus to ultimate success. This reasoning lies at the roots of the comment from Michael Jordan that begins this chapter so gracefully (I'll repeat it here so you don't have to turn the pages back to find it).

> "I have missed more than 9,000 shots in my career. I have lost almost 300 games. On 26 occasions I have been entrusted to take the game winning shot … and missed. And I have failed over and over and over again in my life. And that is why I succeed."

He succeeds, that is, because he knows what causes failure and what causes success, because he has seen it from the inside, and has complete and operational knowledge.

And just as we play the basketball games and golf tournaments to discover who the winners will be, we go forward in life and in business day by day to meet the unknown. And if, in the face of the unknown, we have experimented, and tested, and failed at many possibilities until we arrived at ideas and innovations that make the most sense in a range of possible situations, then we are in a better position to succeed in the long run because we also have complete and operational knowledge.

But if we are intimidated by the fear or possibility of failure, and we don't test the limits of reality, then we're almost sure to fail in the long run because we won't be able to respond quickly enough to unexpected events as they unfold. Having failed to fail, that is, we are not properly prepared to succeed.

Innovation's apparent dead ends, like Jordan's missed shots and lost games, are inevitable, necessary, and in fact should be valued as learning opportunities. As a matter of innovation methodology we are therefore obliged to pursue failure in order to learn.

Executing on this principle requires disciplined management of the entire process such that we make good choices, set up clear experiments, probe fruitful opportunities, and indeed learn from the outcomes and ensure that there are no dead ends in the innovation process.

But that's not stating the situation clearly enough. Failure is not just something you tolerate. It's an absolute requirement. Because to succeed at innovation you *must*

fail.

Suppose you have a set of products and services from which you earn revenues and profits today. You know that at some future date, perhaps tomorrow or perhaps in years to come, those products and services will become obsolete, because your company or a competitor will introduce new products and services that deliver better value, that will cost less or perform better, or both.

The problem with this scenario is the uncertainty: you don't know exactly what those future products and services will be, or when they will be required.

So what do you do? You could guess wildly, using the dart-board method. But obviously it's better to make well-informed judgments, which you could possibly do by experimenting and testing a lot of ideas, developing from experience a reasoned hypothesis about where the market is likely to go based on a deep understanding of technology, consumer preferences, the economy, demographics, and competitive trends, as you hone in on the emerging future.

Accomplishing this requires that you try a lot of ideas in order to learn what works and doesn't work. This will happen in some sort of laboratory, whether it's a chemistry lab or a machine shop or a test kitchen, or even a spreadsheet. Some of the ideas will seem to succeed, while other will not. Some will need further elaboration and more detailed testing. You will learn from all of them; you may learn the most from the failures. As we have an athletic theme in this chapter, a comment by golfer Bobby Jones is very pertinent here: "I never learned anything from a match I won," he said. The winning, in other words, was the culmination of the learning process that was rich with non-winning.

Of course the end goal is to succeed spectacularly. The failures are the necessary enablers, the instances of trying and learning, learning and trying, that lead to the destination.

Innovation is therefore deeply and sometimes brutally ironic, and given this crazy cultural dynamic concerning success and failure, and the very heavy values and reactions that both of them evoke, it's no wonder that the mindset we discussed in the introduction is so challenging for managers to understand, and accept, and adopt.

As a matter of innovation culture, though, we must develop in our organization a rather different attitude about failure than considering it a life-defining stigma, the scarlet letter, "F"!

Notable aspects of that culture will include our attitudes towards uncertainty and ambiguity, as well as a highly disciplined portfolio management process, as we will explore below.

the degree of uncertainty

The type of innovation risk that's appropriate in any situation is linked to the market or markets in which you're competing, and the anticipated rewards you're pursuing. In rapidly changing markets, such as consumer electronics, innovation risk is inherently different than in a slower-changing industries such as, say, road construction.

The faster the rate of change, the bigger the strategic risks, and the broader the range of possible futures that you must prepare for. This will necessarily affect the composition of your innovation portfolio, so the ideal portfolio of each organization will be different: Apple, NASA, Genentech, Union Pacific, GE, and Starbucks are all innovative organizations, but when it comes to their innovation portfolios it's obvious that they cannot be the same in content or in style.

This concept is reflected in many industries as the percent of revenue that is typically allocated to R&D efforts. As these investments are understood to fund the future, the faster things are changing the more it may be necessary to invest.

The biggest investors tend to be high tech companies and pharmaceutical companies; in high tech, R&D investments are typically about 15% of revenue, while in pharmaceutical it typically ranges from about 15% to 20%. The third category of big spenders are the major auto companies, which invest 4% - 5%.[6]

As you transition to this new attitude about failure, it may be useful to compare your company's innovation portfolio with a venture capital portfolio. Venture capital investors know that it's impossible to precisely predict winning business ideas from among any set of well-conceived early stage companies, so they invest in 10 to 20 companies at a time with the expectation that a handful of them will eventually have a shot at greatness, and these successes will more than make up for the many failures.

To realize the potential of this analogy, many of the projects that constitute an innovation portfolio must mature and develop into robust investment opportunities, providing senior executives and board level directors with increasingly attractive new options. But not all of them will. And along the possibly long pathway to the failure or success of any particular idea, we will be obliged to live with a great deal of ambiguity.

return on ambiguity

The proper design and management of your innovation portfolio will enable you to fail intelligently, and with rigor, and to extract the maximum learning from these experiences, and so to ultimately create the most value.

But this requires you to inhabit a land of ambiguity, and to remain there for as long as it takes to conduct experiments, fail sufficiently, and gain definitive insight into the future as a result.

Consequently, one of the defining attributes of successful innovation managers is the high degree of tolerance for prolonged ambiguity that is required to succeed. It's the nature of the job, and those who are not comfortable with ambiguity usually aren't good at it.

By managing their portfolios over time, executives who can live with ambiguity will significantly improve their performance by not forcing false clarity to come too quickly. And as they practice the disciplines of innovation management and learning they'll get in better sync with the evolving market, and get better at selecting and supporting the projects that do have the greatest potential.

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6 Barry Jaruzelski and Kevin Dehoff. "The Global Innovation 1000." *strategy + business*. Winter 2010.

Still, many projects must fail. In fact, a rather high percentage of them should fail, because failure is an indication that you're pushing the limits of your current understanding hard enough to be confident that you're extracting every last bit of learning from every situation, and that you're examining a wide enough range of situations as a way to prepare for a range of unanticipated futures.

As Norbert Wiener puts, it, "Invention and discovery are not calculable risks. In the first place, the really inventive mind must take chances. A long-time record of no false starts probably does not mean infallibility on the part of the inventor, but merely that he has not been willing to push his ideas as far as they merit. The baseball player without a record of errors is the one who does not go after the balls that he might barely pull down, but allows them to damage the record of the other fellow."[7]

burn down

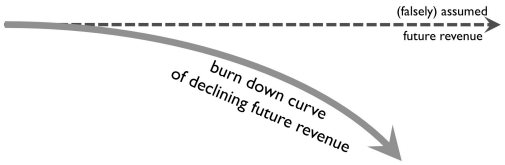
Suppose you're sitting in an innovation portfolio meeting and discussing potential innovation projects. You want to determine the likely ROI on a set of projects, so you assess the risks, and eventually you choose which projects to invest in. You compare the projected revenue stream for each proposed innovation with other uses for that same capital in existing business lines, and you decide that the current business can make better use of the money.

It's all completely rational and reasonable.

Except that you've likely just made a mistake, and perhaps a large one.

The mistake is that your current products and services are gradually (or abruptly) becoming obsolete, so future earnings from your current business will deteriorate. We call this the "burn-down rate," which reminds us that the house of current revenue is gradually burning down, and if you do no innovating then you'll eventually end up with nothing that people want to buy.[8]

Figure 7



Burn Down
The specific profiles of your curves will be different.

The burn down rate varies significantly by industry, so you have to study the specifics of your own business to make a reasonable assessment. Are you in telecom? Then you already know that many (now most) cell phones become obsolete within months. But Boeing is still selling its 747 more than 40 years after it was first introduced (in 1970).

That comment requires clarification, though, because the original 747 of 1970

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7 Norbert Wiener. *Invention: The Care and Feeding of Ideas*. The MIT Press, 1993. P. 145.

8 Eileen Rudden, Boston Consulting Group. She refers to it as the "discounted case flow trap." Clayton Christensen. *Innovation Killers*. Harvard Business Press, 2010.

and today's model don't even have the same outer shape. Though we still recognize it as a 747, just about everything has been changed, which is to say, innovated. The aircraft has been revised, system by system, from the inside out, with safer and more efficient technology throughout.

If Boeing had not made these changes, of course, then the 747 would have become entirely obsolete, so these investments were made in the interest of preserving the marketability of the product, and avoiding burn down. But if you don't factor in the burn-down, then your cash flow and ROI calculations are irrationally biased toward the existing business, and based on the obviously false assumption that existing revenue production will remain viable indefinitely.

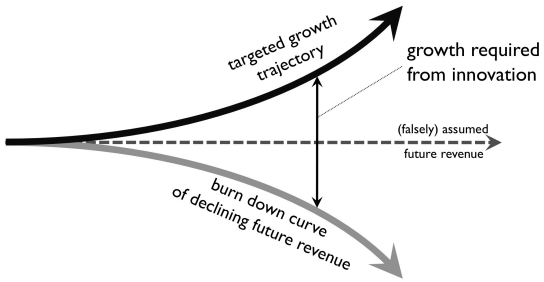

Figure 8
Burn Down & Targeted Growth
The vertical line represents the magnitude of the innovation challenge.

Hence, your portfolio management process has to account specifically for the rate of burn down when you calculate the revenue and profit growth that your innovation efforts must achieve. This may not be simple to calculate, but you need to understand when it is and is not fruitful to make incremental change to the existing business, to anticipate at which point it will be necessary to switch to something significantly different.

Here's a lovely example of someone thinking ahead and anticipating burn down. In describing the need to develop hybrid engine technology, Honda's head of engine development, Hideyo Miyano, commented that "We don't have much time." He estimated that within 20 years, gasoline engines would be pretty much obsolete, and he was consequently managing a large portfolio of projects to provide Honda with the necessary capabilities 20 years in the future.

What makes this particularly interested is when he said this. It was 1992.[9] Impressive, eh?!

manage portfolios *and* projects

OK, so as the future is unpredictable, and since one of the goals of the innovation portfolio is to explore broadly across the realms of possible future business, then as a

9 Karen Lowry Miller. "Honda Sets Its Sights on a Different Checkered Flag." *Business Week*. August 17, 1992. P. 45.

consequence of this very exploration, the innovation portfolio absolutely invites failure. The Chief Technology Officer of a major global corporation put it succinctly when he explained his approach to testing new technology. He said, provocatively, "Our purpose is to fail."

Because only by failing does his team learn which new technologies work and which don't, and how they can best be used or how they cannot or should not be used. Failure is an explicit learning strategy, and so they run innovation projects to push the limits of reasonableness, knowing full well that failure is lurking, the inevitable and welcome companion of learning. Individual projects fail so that the portfolio as a whole succeeds grandly.

And therefore it would be unfair, brutally and self-defeatingly unfair, to punish innovation project leaders and portfolio managers specifically for the failures. Instead, they should be measured on and rewarded for the performance of a portfolio as a whole, not the performance of specific individual projects.

"Whole" means that they have applied the lessons of last year's successes and failures to create this year's successes, and this year's failures are expected to contribute to next year's successes. This will reward them for engaging in a vibrant learning process, for assuring that insights gained in any particular project are made to benefit all the projects.

This was brought home to me some years ago when I visited a research facility at P&G, and during the tour it was mentioned that one of the underlying management principles they followed was a concept called "kill'em quick." By this they wanted everyone to understand that if you happened to be working on a project and you discovered that it was a dead end, an impending failure, that the lab managers invited and requested you to let them know *immediately* so the project could be halted and you could be reassigned to something more promising.

Underlying this policy was the realization that, yes, failure is inevitable, and we're running a learning process here, and the sooner we all see failure coming, the sooner we can adjust our plans.

Practiced rigorously, this will progressively steer every portfolio towards success.

non-correlated assets

Starting in the 1990s and into the following decade, many banks and investment houses earned magnificent profits from mortgage-backed securities, and as the numbers grew this business became as addictive as a drug. The upside possibilities seemed unlimited, while the risks seemed miniscule; consequently, many shifted ever larger portions of their efforts to this lucrative market.

When the market started to stumble in early 2008, some industry observers assumed that the holdings of these firms were diverse and that because they owned many different types of securities in many markets they would be protected from catastrophe because even if one segment of the financial markets did poorly, which indeed they saw happening, the market as a whole would remain healthy.

They were wrong on both assumptions. What these observers had not understood, but which we all were to learn soon thereafter, is that many firms had

concentrated too much of their portfolios and too much risk into this one type of asset. So when the mortgage backed security market failed, the plummet triggered a massively negative chain reaction.

One reason that it was possible for this to happen was that the market as a whole was largely unregulated. So in testimony before a congressional committee on October 23, 2008, former Federal Reserve chairman Alan Greenspan admitted that he had been "partially wrong" in his hands-off approach towards the banking industry. "I have found a flaw. It turned out to be much broader than anything I could have imagined. Those of us who have looked to the self-interest of lending institutions to protect shareholders' equity (myself especially) are in a state of shocked disbelief."[10]

The phrase, "much broader than I could have imagined" shows that Greenspan's understanding of portfolio theory may also have been lacking. This sort of catastrophic failure is precisely what he *should* have considered as possible, imagined that is. In practice, the "intellectual portfolio" of the uber regulator should by definition include an understanding of scenarios that are far broader than actual portfolio of behaviors that the regulatee, the market, can manifest.[11]

He told the House committee that he regretted his opposition to regulatory curbs on certain types of financial derivatives, those which left Wall Street banks facing billions of dollars worth of liabilities. Committee chairman Henry Waxman then pressed him, with a distinctly ideological bent. "You found that your view of the world, your ideology, was not working?" Greenspan had no choice but to agree, saying that, "That's precisely the reason I was shocked, because I'd been going for 40 years or so with considerable evidence that it was working exceptionally well."

The line of reasoning that Greenspan chose exposed another portfolio management error, because he didn't account for how new behaviors unfold: ideas always work until they don't, at which point being surprised that they no longer do is evidence of poor preparation. Greenspan continued, "I made a mistake in presuming that the self-interests of organizations, specifically banks and others, were such that they were best capable of protecting their own shareholders and their equity in the firms." It turned out that regulations were necessary to assure that short term greed did not make the banks excessively vulnerable to long term risks. In fact, banking regulations are designed precisely to address this question, and those regulations had been non-existent.

The final irony worth mentioning here is that during the 40 year period that Greenspan refers to, the government, largely at his own instigation, went through a process of progressively de-regulating the banks and investment houses, allowing them progressively more freedom to pursue whatever markets they wanted in whatever way they wanted. And it was precisely the cumulative impact of deregulation that enabled the system to become so intensively concentrated into one

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10 Edmund L. Andrews. "Greenspan Concedes Error on Regulation." *The New York Times*. October 23, 2008.

11 In cybernetic terms, we might technically consider this to be a problem of variety matching, in that the variety of the regulator has to exceed the possible variety of the regulatee, or in fact the regulator is doing a poor job. A good innovation portfolio must likewise dispose greater variety than the market can manifest, or it, too, is poorly devised.

type of soon-to-be-dysfunctional asset. So in his assessment of the possible behaviors of the market, Greenspan should have realized that deregulation was changing the very structure of the market, and therefore changing the nature of risk, and therefore requiring of the banks more self discipline if indeed they were also to be capable of self-regulating. (If he had looked, he would not likely have found much evidence of self-discipline.)

My point is not to belabor a debate over the correct ideological framework, but to use this story as a vivid example of how you have to think carefully about the principles of portfolio management, and study the contents of every portfolio in order for the portfolio to perform properly given a broad range of possible market behaviors. In the illustration below, the smaller circle represents the current behaviors of the market, which means that the design of the portfolio (whether it's an innovation portfolio or a regulation portfolio) has to encompass the possibilities in the bigger circle so that you are sure to examine and prepare for a wider range of possibilities since the exact future remains entirely unpredictable despite your persistent efforts to predict it.

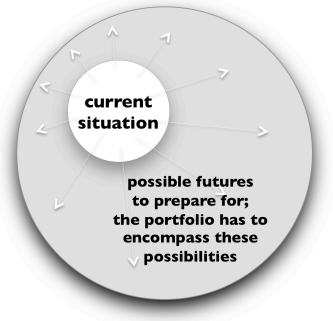

Figure 9
A Wide Range of Possibilities
(The same graphic, by the way, was used on page 84 to make a related point about possible futures.)

During the same period, Treasury secretary Paulson also realized he ought to have anticipated a meltdown in the US mortgage industry. "I could have seen the sub-prime crisis coming earlier," he told the *New York Times*, admitting thereby that his conceptual framework had been inadequate. And this is the core of the portfolio design issue; it is a matter of defining the right conceptual framework.

Some observers did indeed foresee the problems, including the once-infamous and now-famous Brookesley Born, who was forced to resign as a regulator when she called attention to the growing risks.

Author and investor Nassim Nicholas Taleb put it this way in his pre-crash book, *The Black Swan*: "Regulators in the banking business are prone to a severe expert problem and they tend to condone reckless (but hidden) risk taking."[12] [The "expert problem" is the situation of supposed experts knowing less than it appears they do.]

But they were in the minority. Greenspan and Paulson had a lot of company in mis-assesing the dynamics of the market. They and thousands of other bankers and

12 Nassim Nicholas Taleb. *The Black Swan.* Random House, 2007.

investment managers had forgotten one of the key lessons of Investment 101, which is that healthy investment portfolios consist of "non-correlated" assets. The principles of correlation and non-correlation were worked out in detail in the 1950s by the father of portfolio theory, Harry Markowitz, who subsequently was awarded the Nobel Prize in Economics.[13]

"Correlation" means that two (or more) investments tend to behave the same way in any given set of market conditions. Investment portfolios containing securities that are highly correlated are riskier than portfolios with non-correlated securities, because they tend to rise and fall together. The rising part is welcome, but the purpose of a portfolio is to reduce risk, so creating a portfolio in which everything has the potential to fall together is a very bad idea and increases risk, which is exactly the opposite of what a healthy portfolio should do.

Hence, a "non-correlated" portfolio consists of a variety of assets that are expected to perform differently under any given set of economic conditions. If the market goes up, some assets will do better than others; if the market goes down, different ones will be the right ones to own. So the prudent investor owns both types in a non-correlated, or diverse securities portfolio, containing a mix of stocks and bonds from different markets and regions.

The validity of the theory was unintentionally affirmed on the grand scale of the economy as a whole by the collapse of the mortgage market and the chain reaction that followed, as it became clear that the affected firms had concentrated far too much risk into mortgage-based assets such that when the mortgage market turned bad, it dragged entire firms down.

To compound the problem, and perhaps to heighten the irony, it was the aggressive behavior of the investment firms and banks that drove the mortgage market into crisis in the first place by selling millions of families into mortgages that they could not afford, and the scale of the resulting damage was so great that it affected the entire economy, and triggered the recession.

This is the classic phenomenon of a financial bubble, which is indeed also a problem of excessive correlation. To jump into the rising market, investors abandon the principle of portfolio diversity in order to reap the seductive profits looming in front of them, and pay the awful price when the collapse arrives.

In the case of your innovation portfolio, non-correlation requires your firm to be working on potential innovations that address a wide range of future market possibilities in order to assure – and here is the key point – that the available options will be useful under a wide variety of possible future conditions.

Knowing as we do about the unpredictability of change tells us that the innovation portfolio should contain a wider variety of projects-in-process than the variety of our current set of offers in the market. In other words, our innovation pipeline should be actively engaged in exploring possibilities that are outside of the current market, so that if the current market should suddenly shift, we are prepared to respond.

If you talk with innovation managers at Nokia and at Boeing, to name two companies, you'll find that they are in fact working on projects that they know will

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13 Harry Markowitz. *Portfolio Selection. Efficient Diversification of Investments*. Wiley, 1959.

never see the light of day; as of today, however, they just don't know which ones will eventually come to market, and which will not. Only time can tell them that, and prudence requires them to prepare. Nokia's set was, in hindsight, too narrow, and burn down became an inferno that also torched its stock price. Preparing a diverse innovation portfolio is exactly what Nokia did not do (or at least they left out the smart phone from it), and which caused the company's perceived value to fall so dramatically when Apple mastered the smart phone paradigm and Nokia had no immediate response.

This also explains another reason that pursuing only incremental innovation is not a success strategy, as incremental innovations on existing products and services are useless when the market shifts in an entirely different direction. To borrow a familiar cliché, it's not desirable to be the maker of the world's best buggy whips when all the customers have switched to cars.

Or, like Nokia, to be focused on making mobile phones cheaper when customers want them to be smarter.

Hence, as Apple has seen tremendous success with the iPhone, Nokia's stock value has plummeted even though the company is still the overall market leader by volume.

Or, like GM, which was focused on the annual styling cycle when customers decided they preferred improved design, higher quality and especially better gas mileage in the 1970s and again in the early 1990s, and yet again after 2005. (Here is a situation where a more robust appreciation for history would have served the company well.)

It does take a great deal of discipline to avoid falling into the trap of optimizing the current products and services for the current market, which is an example of dangerous correlation, and to recognize that non-correlated qualities and characteristics that may be different from what's needed and wanted today may become the romantic obsession of tomorrow's market. Mastering these new qualities and characteristics *in advance* is precisely the purpose of the innovation portfolio.

The need for non-correlation and therefore broad diversity in the portfolio also reinforces the need to explore and develop ideas across all four types of innovation. Hence, large companies will create and manage portfolios for each type of innovation, and hence there will also be a fifth portfolio that is an aggregate of the other four. The mechanics of creating these portfolios will be discussed in the next chapter.

Before we get there I will just briefly note that each different type of portfolio will be managed in a different process, by different people, who have different business goals, and who are measured and possibly rewarded differently.

Hence the thoughtful design of metrics and rewards is necessary to a well-managed portfolio, and your master plan also calls for you to the design the ideal metrics by which the performance of your portfolio should be measured. The topic of metrics will be discussed in detail in Chapter 6, as it pertains to both the innovation portfolio and the innovation process.

In Chapter 4, which follows below, I'll describe a detailed process for portfolio design, and then in Chapter 5 we'll look at a design for the broader innovation process.

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Please return to InnovationManagement.se for the subsequent chapters of *The Innovation Master Plan* by Langdon Morris.

You can learn more about his work and access additional writings and his blog at www.innovationlabs.com.

About this Book

This book is intended as a companion to my previous innovation book, *Permanent Innovation*.

During the four years since *Permanent Innovation* was completed, we've continued to refine our understanding of the innovation process through work with many organizations, and we've found that senior managers have a continuing interest in guidance in the design and management of their innovation initiatives. *The Innovation Master Plan* addresses many of those needs, and deals with aspects of the innovation process that *Permanent Innovation* didn't address.

In the course of preparing *The Innovation Master Plan*, I've also discovered some opportunities to improve *Permanent Innovation*, and as a result a revised edition is now available.

(You can download *Permanent Innovation* at
www.permanentinnovation.com)

About the Author

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Langdon Morris is a co-founder and partner of InnovationLabs LLC, one of the world's leading innovation consultancies. He works with organizations around the world to help them improve their proficiency in innovation.

He is Senior Fellow of the Economic Opportunities Program of the Aspen Institute, Editor of the Aerospace Technology Working Group Innovation Series, Associate Editor of the *International Journal of Innovation Science*, a Contributing Editor and Writer for InnovationManagement.se, and a member of the Scientific Committee of *Business Digest*, Paris. He is formerly Senior Practice Scholar of the Ackoff Center of the University of Pennsylvania and Contributing Editor of *Knowledge Management* magazine.

He is author, co-author, or editor of eight books on innovation and strategy, various of which have been translated into six languages, author of many articles and white papers, and a frequent speaker at workshops and conferences worldwide.

He has taught or lectured at universities in the US, France, Portugal, Taiwan, and Argentina, including Stanford University, the Ecole Nationale des Ponts et Chaussées and the Conservatoire National des Arts et Métiers, Paris, the University of Belgrano, Buenos Aires, and Chaoyang University of Technology, Taiwan.

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