Putting Rational Blinders Behind Us: Behavioural Understandings of Finance and Strategic Management

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Both financial economics and neoclassical economic approaches to strategy attempt to understand the operation of competitive markets assuming market equilibrium and optimising agents. These assumptions imply no strategies can exist that consistently outperform the market. However, behavioural finance results clearly demonstrate this implication is wrong. For strategic management, a theory that implies no rules exist to outperform the market cannot explain why some firms consistently perform better than others. This paper argues that a behavioural perspective of strategic management offers a coherent framework and set of assumptions that better inform the problems strategic management scholars attack.

Introduction

Strategic management and finance scholarship both address the performance of actors in competitive markets. Strategic management scholars generally view corporations as the participants competing in product or service markets. Finance scholars emphasise investors competing in capital markets.

Historically, the two fields differed in their disciplinary assumptions. Finance scholars generally assumed an efficient market model from economics. Strategic management scholars took a more behavioural view that implicitly assumed inefficient markets.

For half a century, finance has been dominated by efficient-market economic assumptions. Let us begin by clarifying what efficient means. As Shleifer notes¹ Fama defined an efficient financial market as one in which security prices always fully reflect the available information. The efficient markets hypothesis then states that real-world financial

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markets, such as the US bond or stock market, are actually efficient according to this definition... the EMH "rules out the possibility of trading systems based only on currently available information that have expected profits in excess of equilibrium expected profit or return" (Fama, 1970)."

Most efficient market models of capital markets assumed markets were either comprised of or dominated by rational traders who, by definition, valued securities based on the net present value of future earnings flows discounted by the investment’s risk characteristics. Rational in this usage (and throughout this paper) refers to an economics sense of rationality in which actors choose from all available alternatives the one that maximises expected value or utility. Finally, these models assumed markets were in equilibrium, meaning no trader could act individually to improve his expected returns above an expected market return.

Thus, the dominant finance models assumed rational investors competed in an efficient market where information transfers extremely quickly. For example, even the ‘weakest’ of efficiency assumptions assumed all market participants have full knowledge of publicly available information. These conditions lead to a central prediction that no rules could exist that would provide above average risk-adjusted returns. Within the capital asset pricing model, the assumptions lead to the prediction that risk, measured by beta, would provide the primary, if not only, explanation for systematic differences in stock returns.

In the finance world, these predictions fit empirical findings sufficiently to allow continued belief in these models (and while some finance scholars disagree now, many if not most remain committed to modelling rational decision-makers in competitive markets). Evidence consistent with the ‘efficient market hypothesis’ showed that stock market ‘experts’ do not consistently beat the market on average, and a variety of analyses could not find patterns in market movements. Contrary findings were dismissed as the use of an inappropriate market model rather than seen as valid refutations of the efficient market hypothesis. Finance scholars were quick to point out that any test of capital market efficiency constituted a joint test of market efficiency and the capital asset pricing model.

Stock market ‘experts’ do not consistently beat the market

In strategic management, on the other hand, scholars began by assuming some patterns of behaviour could positively influence performance. This approach may have reflected the teaching origins of the field—MBA students and corporate executives wanted to know how to make their companies successful. Furthermore, the founders of the field were not trained primarily in economics and so not fully imbued with the efficient market model. Nevertheless, this assumption fits with the reality that some firms consistently do better than others.

Oddly, in recent years behaviourists in the finance area have been chipping away at the efficient market assumption by identifying stock market patterns that are inconsistent with market efficiency. However, despite an impressive critique of traditional financial economics, behavioural finance scholars have been unable to offer a coherent alternative. Rather, they have generally drawn on theoretical ideas from psychology to explain individual investor behaviour, while continuing the tradition of equilibrium models of prices or other factors.

Meanwhile, economists have been introducing rationality and equilibrium assumptions into strategy. At the industry analysis level, the theories assume industry structure determines firm performance—if you are in a bad industry, you are done for. At the firm level, the Resource Based View suggests you will only get high returns if you have some special ‘resources’ that generate profits. Such resources must be scarce, complex and hard to imitate—otherwise competitors will copy the resources and compete away the returns. In short, for firms who have no special resources and operate in competitive markets, management can do little to improve performance.

This paper argues that these rationality and equilibrium assumptions are not helpful in strategic
management research. Rather, we argue that a behavioural perspective of strategic management, which focuses on disequilibrium conditions and rejects the profit maximisation model of decision-making in favour of a bounded rationality view, offers a more coherent framework and assumptions that better inform strategic management issues. This more realistic view suggests managers should identify market inefficiencies and utilise best practices that enable effective knowledge transfer, thereby enabling competitive advantage.

The behavioural challenge in finance
For many decades, efficient market logic dominated finance scholarship. Assumptions of rational investors in efficient markets resulted in the capital asset pricing model (which justifies the use of beta and related measures of the cost of capital). Scholars widely believed and taught that no one could consistently beat the stock market, and that stock price equalled the net present value of future dividends or earnings and constituted the true value of the firm. Jensen referred to capital market efficiency as the best-established empirical fact in economics.6

The empirical facts supporting this efficient market hypothesis take a curious path because the underlying economic theory implies that market returns should be random. When we normally test a theory, we take a null hypothesis (for example, that two variables are unrelated) and the hypothesis we want to test (the two variables have a positive association). The test then examines data to see if we can reject the null hypothesis. We only reject the null hypothesis when we are quite (95 per cent or 99 per cent) sure that the sample is inconsistent with the null. However, ‘tests’ of the efficient market hypothesis are actually tests of specific alternative models so that the null is the efficient market (random) model. That is, any ‘test’ of the efficient market hypothesis instead of requiring the hypothesis to be supported with more than 95 per cent probability, required that the inefficient alternatives have this high level of significance. Scholars interpreted an inability to find systematic patterns in stock prices as evidence for market efficiency. Such evidence of market efficiency was taken as demonstrating that stock prices represent the net present value of future earnings streams (even though direct study of this found it to be untrue).

Throughout, this view of capital markets had numerous unpleasant implications that scholars ignored. A world of fundamental traders should result in sporadic trading—if traders only trade on new information relevant to future earnings streams of companies or changes in risk parameters, we should not see continuous trading and price variation in capital markets. While finance theory assumed everyone traded on fundamentals and held diversified portfolios, many individuals and institutions clearly do not trade or act the way the models assume they do. Many investors held ‘inefficient’ portfolios. Large numbers of investors sold and traded on ‘technical’ analysis that assumed systematic patterns in stock prices. Others used the ‘value’ investment style, investing in companies with low price to earnings or price to book ratios. Far from being a few crazy investors who would be soon eliminated from the market (the normal economic excuse for ignoring non-optimising behaviour), many major funds committed themselves to investing on a variety of non-fundamental principles.

When individuals raised these kinds of questions, finance scholars quickly rejected them. They argued that rational traders would quickly exploit any traders who did not follow efficient rules. They argued that inefficient traders created the possibility of arbitrage by rational traders who would eliminate the impact of the inefficient traders. Part of this simply reflected the depth of scholars’ belief in the fundamental equilibrium and rationality assumptions of economics. As Krugman says: ‘The overwhelming thrust of conventional theory has been to say that agents are not only intelligent, they maximise—that is, they choose the best of all feasible alternatives. And when they interact, we assume that what they do is achieve an equilibrium, in which each individual is doing the best he can given what all the others are doing.’7 Of all possible markets, the capital markets seemed the most closely approaching the economist’s ideal—large numbers of traders, highly efficient information and transaction processing, etc.

However, in recent years, a much more behavioural view in finance has emerged:
Many studies showed that prices did not reflect true value. Modigliani and Cohn argued the stock market was substantially undervalued because analysts did not account correctly for inflation. Easton found stock prices were significantly above the net present value of future dividends. Other studies found stock prices varied too much given underlying variance in dividend streams.

Tests previously perceived to support the efficient market hypothesis lacked statistical power. Summers presents a model where prices reflect true value plus serially correlated noise. He shows that even if the immense majority of the market variance comes from this noise, statistical tests would lack sufficient power to detect it. Summers also noted that arbitragers face the same informational problems that statisticians do—the same difficulties that make it hard for econometrics to discover inefficiencies would also make it impossible for efficient traders to take advantage of these inefficiencies or inefficient traders.

Beta, the core of the capital asset pricing model was shown to not explain stock prices after controlling for firm size and market to book ratio. This, while a blow to that particular model, simply sparked a search for new risk measures so that the basic theory could remain intact (perhaps moving from one to three risk dimensions).

Some studies have shown ways to beat the market—there existed ‘rules for riches’. These studies showed a variety of ‘effects’—a January effect, day-of-the-week effect, a sunshine effect, a momentum effect, etc.

One of the seminal events in this change came from Black’s Presidential address to the American Finance Association after he had spent some time on Wall Street. He argued the world is much noisier than the conventional finance theory assumed. Instead of stock prices instantly reflecting true value, he said stock price is a noisy estimate of value. Noise causes markets to be inefficient and makes it difficult to test theories about the way financial or economic markets work.

In short, behavioural finance advocates find many of the core beliefs of traditional financial economics to be incorrect. As Shleifer notes, ‘market efficiency only emerges as an extreme special case, unlikely to hold under plausible circumstances’.

While amassing an impressive critique of traditional financial economics, the behaviourists have been unable to offer a coherent alternative. Empirical behavioural work has largely either simply looked for patterns inconsistent with market efficiency or taken phenomena concerning individual decision making from psychology and attempted to test them on financial data. Theoretical behavioural work usually takes a few simple assumptions about how investors behave and develops equilibrium models of prices or other factors.

Given these findings concerning what economists claim to be the most efficient market, we now turn to the implications for strategic management practice. The remainder of the paper considers measures imported from finance into strategic management, followed by an alternative behavioural perspective that offers a more realistic approach to addressing general strategic analysis issues that managers face.

Measurement imports from finance to strategy
Perhaps the strongest influence of finance on management has been through measurement tools. Many tools used to judge investments (and divisions) derive directly from efficient market finance theory. Let us consider these.

First, and foremost, net present value (NPV) and internal rate of return (IRR) measures derive from financial economic theory. Standard introductory finance texts criticise traditional (and popular) measures such as payback period and rate of return over some fixed horizon and instead advocate these discounted cash flow models.

While not bad, NPV and IRR have many problems. First, they depend on projects being free-standing or on the organisation being able to identify all the impacts of a given project. While fine for many projects (e.g., cost reduction in operations), for major projects we often see inter-
relations between a project and other strategic activities. Second, they assume projects are simply
gambles where the firm makes a decision today and later sees what happens. If firms make sequen-
tial decisions on a project, or if the project may lead to other opportunities later, then the one-
time decision model of NPV and IRR is wrong. Third, they assume the problem is selection.
Implicitly, these models assume the corporation faces a set of well-defined projects with good
forecasts and must select among them. In contrast, the critical problem for many companies is
developing good (i.e., profitable) projects and generating reliable forecasts. In addition, once a
company implements a particular technique to measure project performance, that technique will
influence the generation and justification of projects in many ways.

Stock price does not equal an estimate of the net present value of
future earnings streams

Part of the problem comes from scholars focusing on what they think they can handle well,
rather than the critical issues. Think about projects that lead to new product launches. Market-
gers generally assume the immense majority of new products fail—nine out of ten is the ratio often
mentioned. However, every one of those new products came to management with forecasts show-
ing positive profits. If the forecasts are very poor, it may not matter much which evaluation system
you use—the noise in the system is in the forecast, not the evaluation criterion. If we want to
improve such a system, we need to work on forecasts, not evaluation.

Let us now turn to two measures championed at the strategic level—NPV and economic value
added. Rappaport’s ‘shareholder value’ model says use NPV at the strategic level.15 Rappaport’s
shareholder value model argued for using net present value to evaluate investments and divisions.
While his success in profitably selling a technique taught to every MBA student is impressive, the
tool has all the problems noted above for NPV. Economic value added (EVA) approaches charge
divisions for the capital they use. The claim is that charging for the value of capital used generates
a measure closely associated with true economic value generated (as distinct from an accounting
value that reflects arbitrary accounting rules), and that the stock price reflects such economic value.

While these measures make sense, they generally deliver less than they promise. First, appli-
cations of these techniques seldom follow their underlying theories. A firm with multiple divisions
should use differing costs of capital (rates charged for the use of capital in the EVA approach)
since the divisions should have different betas. However, few firms will impose gross differences
in cost of capital on their divisions—one division could easily have a hurdle rate twice the rate
of another division. Managers reject the idea of rewarding a division with high EVA due to a low
cost of capital while punishing a division with identical cash flows but low EVA due to a high
cost of capital. Note that the cost of capital here is not some direct accounting charge for borrowing
but rather comes from estimates based on particular models of efficient capital markets.

Second, these techniques do not address the core issues. Just as noted with the use of NPV for
capital budgeting, these models present the strategy problem as choosing among known alternatives
with good forecasts, when in reality firms struggle to identify or generate strategic alternatives and
estimate their impacts.

While these critiques are well known, prior critiques generally assume that the underlying theory
is correct, while new research shows it is wrong. Advocates justify these models as tying internal
evaluation directly to what will increase shareholder value, but the evidence shows they do not.
NPV assumes shareholder value depends on the NPV of future earnings streams. Easton finds
stock prices in 1962 significantly exceeded the net present value of firms’ future dividends
streams.16 Modigliani and Cohn argued that the stock market was substantially undervalued by
approximately 50 per cent in the early 1970s because analysts did not account correctly for
inflation.17 Biddle, Bowen, and Wallace found that, ‘in no case does EVA significantly outperform
EBEI (earnings before extraordinary items) in tests of relative information content [ability to
explain capital market value]. On the contrary, in most cases the evidence suggests that earnings outperforms EVA.\textsuperscript{18}

To put it simply, advocates justify these measures by claiming they tie corporate decision making directly to things that influence the capital market’s valuation of the firm, but the empirical evidence shows this is not true. Stock price does not equal an unbiased estimate of the net present value of future earnings streams, and relates more closely to traditional measures such as earnings before extraordinary items than it does to EVA.

The techniques also handle risk in a curious way. Given their efficient capital market orientation, they treat risk as the association between a business’s value and the values of firms in general. Beta and related cost of capital measures derive from a theory that assumes widely diversified investors investing in efficient markets. Theoretically, such investors only care about the expected returns and their association with overall market movements (which constitutes risk in efficient markets finance).

Managers normally have very different ideas of what constitutes risk. Managerial conceptions of risk centre on the possibility and amount of potential loss. The potential for loss from a given project has nothing to do with its covariance with the stock market. Often, managers manipulate NPV and similar numbers in ad hoc ways trying to inject their risk concerns into techniques that assume their concerns are irrelevant.

Indeed, the real question is whether firms which use these techniques, make better decisions than firms which do not. The advocates have no such data. The scholars who teach these techniques have so much faith in their theory that such empirical validation seems trivial to them. Overall, it clearly makes sense to consider the capital used by a division or project in relation to the income it generates. However, most such measures give similar results. If we constrain cost of capital to be similar across divisions, we would expect return on assets (which implicitly assumes equal cost of capital across divisions) to correlate highly with net present value or EVA. If most projects have similar time profiles, payback period should provide similar choices and obviously provides valuable information to managers who worry about the uncertainty of the future.

In short, most measures based on financial economics depend on the assumption that stock prices equal the net present value of future earnings, a clearly suspect assumption. Furthermore, measures such as economic value added and net present value of earnings do not explain market returns better than supposedly flawed accounting measures. Overall, strategic management scholars have adopted and advocated many finance-justified measures for which there is either inconclusive or negative evidence.

For both scholars and practitioners, recognition of the theoretical and empirical limitations of EVA and NPV presents an important question: how do differing decision rules influence the quality (i.e., after the fact profitability) of investment decisions? Few if any firms systematically trace the connection between the analysis justifying investments and investment performance. Almost certainly, investment proposals reflect systematic errors. Proponents may over-estimate expected sales, or underestimate costs to gain approval. We suggest firms should examine the tie between proposal and outcome to identify systematic forecasting errors and to determine whether different investment rules actually make better investment decisions.

**Efficient market assumptions in strategy**

While much of the exchange between Finance and Strategic Management has been Strategic Management importing measures from Finance, a much more central issue comes from Strategic Management scholars adopting the economic-based logic associated with efficient markets.

Most models of efficient markets with rational participants predict no one obtains consistent, unusual levels of performance. In other words, no company makes economic profits in equilibrium. This has been exceedingly clear in the finance literature and some scholars in strategy accepted this logic. Barney, Mosakowski and Rumelt, Schendel and Teece all claim there should be no systematic practices that deliver above average performance—no ‘rules for riches’.\textsuperscript{19}
resource-based view is an attempt to solve this problem: Lippman and Rumelt and Barney define the strategic management scholar’s problem as explaining differences in performance in an efficient market in equilibrium.29 Barney ‘solves’ this problem by hypothesising ‘resources’ exist that can only be observed because they result in high profits.

In finance, this ‘no rules for riches’ actually constitutes a standard test for market efficiency. Many studies that test financial market efficiency essentially do so by looking for rules that would explain why some investors earn abnormal returns. Since efficient market theory predicts no systematic abnormal returns, it cannot explain such returns.

Economists in strategy faced the same problem. If markets are efficient and firms are rational, then why do we see such continued variation in firm performance and behaviour? In other words, a scholar who assumes rational decision-makers and markets in equilibrium, has great difficulty developing models that reflect the real business world as we see it: in a given market, firms differ widely in size and other characteristics and achieve widely varying but serially correlated levels of performance.

The Resource Based View tries to justify performance differences in equilibrium. Lippman and Rumelt and Barney assume some undefined firm-specific factors (resources) exist to cause these performance differences. This would be equivalent to the finance scholars explaining differential performance by traders as ‘some traders get better information than others and so make more money’ and inferring this not from clear evidence about trader information, but rather simply from differences in trader performance. Mosakowski attacks the problem more on its own ground by suggesting that managers cannot influence expected returns but can influence the distribution of returns. This has a clear parallel in finance theory where investors clearly can choose different variances in expected portfolio returns. However, Mosakowski’s solution requires that we believe managers cannot improve the expected performance of their firms by better decisions, a belief that runs contrary to many findings in strategic management.

Assuming largely efficient markets and rational competitors leads to two basic ways to have high profits, each of which underlies a stream of advice to managers:

1 Operate in less competitive markets. Within efficient market analyses, such markets only exist where firms have market power (ability to push around suppliers or customers) along with strong barriers to entry (as without such walls competitors will enter and increase competition). This is the basic rule offered by Porter’s Five Forces and Competitive Analysis.

2 Compete only where you have abilities (eg resources, capabilities, competences) that competitors do not have. With rationality assumptions, these resources must be exceedingly hard to transfer—Barney argues that managers cannot even understand them or other firms will get them by buying away managers.

Overall, these efficient market assumptions lead to an inactive and pessimistic perspective on management. Suppose I am in the restaurant business. I have little choice but to be in a competitive market. Likewise, what could I do with restaurants that competitors could not copy? If I have only one restaurant, I might have a superb chef, but with a chain, whatever recipes I have must be readily transferable. Competitors can observe management practices, décor, etc.. The RBV and industry analyses suggest I have little hope of success.

Yet, we know some restaurant chains do better than others. Likewise, in almost all industries, some firms do better than others. We need a behavioural perspective to make sense of these facts.

**Behavioural perspective of strategic decision-making**

Instead of trying to shoehorn strategic management into a set of assumptions that clearly do not fit, scholars in strategic management should take the world as they see it. Consider, for example, rationality and equilibrium.

Behaviourists have a long history of rejecting rationality and equilibrium on theoretical and
empirical grounds. Instead, many scholars take a bounded rationality view that sees individuals as trying to achieve their objectives but acting with severe limitations on the information they possess and their ability to process that information. A bounded rationality view implies (directly contrary to economic assumptions) we should never find a firm that does everything the best possible way (i.e., that had no way to improve its position).

Instead of the economists' 'rational' traders, a behavioural view sees the world as a set of bounded information processors trying to do well. Assuming firms act rationally rules out differences in quality of decisions—all firms make optimal decisions. This makes no sense given our understanding of human behaviour. In all human competitive activities, people differ and teams differ. For instance, assuming rationality in chess would mean all chess players were equally good—obviously ridiculous. It takes little awareness to see businesses differ in the quality of their management and operations.

The strategic management goal of offering advice to managers only makes sense if managers do not make optimal decisions. Indeed, the existence of business schools and the study of strategic management make little sense if firms already operate optimally—by definition, we cannot improve them.

Moreover, equilibrium analysis draws attention away from the more pressing issue of how firms actually achieve and sustain competitive advantage. A focus on disequilibrium conditions that reflects how firms develop capabilities would better inform managers about how to improve performance.  

While rejecting equilibrium and rationality should massively change strategic management research, it also directly influences how we should think about strategic management practice. Here are some suggestions for managers.

Look for rules for riches

Everyone does not know the best way to do things. Indeed, over time we have learned better ways to do many things. People who learn such practices first prosper.

In direct contrast to the 'no rules for riches' story of rational analyses, we really have a progression of tools that can help managers. For well-defined problems, operations research tools have proved exceedingly valuable. Scheduling, inventory, routing and many other practices can benefit enormously from operations research tools. Total quality management has shown benefits in many areas. Marketing research, manpower planning, and so forth have benefited firms.

We should not expect naively that just grabbing a tool would help. Tools must fit the firm—if marketing is unimportant, a large investment in marketing research may be misguided. In addition, the benefits from a tool depend on the behaviour of competitors. If the competitors all quickly learn to use the tool, the tool may not continue to provide high returns.

Instead of the rationalist, 'everyone will get everything that is public', managers should take a sensible view to learning. Suppose we have a new tool that has benefits. The first adopters of the tool should benefit. However, over time, we expect the benefits to the early adopters to decline, as eventually the tool becomes just a necessity for participation in the industry. For example, high quality gave immense competitive advantage in many industries in the past, but in some, everyone has reached a quality level such that it is simply a necessity to compete, not a differentiator. Likewise, while some of those who moved their production from the Europe or the US to cheaper Asian facilities benefited, now overseas production appears essential to compete in some industries.

Things that give advantage will disperse across an industry at different rates. In many financial industries, new products result in only the most fleeting of advantage—as soon as a product is offered, competitors can copy it. Being the first to offer a new kind of mutual fund or savings plan gives only fleeting advantage. In contrast, a firm may be able to retain an advantage from a better way to assess borrower risk or to process loans internally.

Better ways to do things exist—the trick is figuring out which ones really work and for how long they will offer advantage.
Look for market inefficiencies
Equilibrium assumptions imply we compete in well-defined markets and markets are efficient. This is often wrong.

Think about a market as a place where the price and quality of a given seller’s product influences the buying of another seller’s product. Some industries compete in relatively efficient markets where buyers can switch easily, and everyone knows (and cares) about what is in the market.

However, many markets are not efficient in this way. For instance, restaurants offering lunch largely compete with the other restaurants in walking distance. Retail banks compete at least partially with the retail banks physically close to their locations. Adhesives compete with other adhesives specifically targeted to a given application. Many technological firms compete only with the small set of firms offering similar products for the same customer group. Petrol stations compete with others nearby.

This diversity of ‘micro-markets’ and competitors constitutes part of the wonder of a free enterprise system. Consultants vary from big standardised companies to thousands of individuals with small angles (products and willing customers). Successful restaurants vary from major chains to hole-in-the-wall ethnic or local favorites.

Because niches often vary by product (e.g., a firm may hire an individual for market research but a major firm for accounting), this suggests bringing strategic analysis down to the product level. A petrol station may compete with a food store for some products and with a repair garage for others. In dental materials, conventional impression materials constitute a highly competitive market with few technological secrets whereas adhesives for attaching on-lays requires high-level technical skills, proprietary products and few competitors. Competition can vary radically across products.

While traditionally we have applied competitive analysis at the division or business-unit level, it makes sense to bring the analysis down to the product-market level. Buyers, suppliers, substitutes, barriers to entry, and rivalry all matter at the product level. In other words, we want to look for opportunities to gain market power or advantage at the lowest level of competition—products sold to given customers, etc.

We also need to consider how information and diffusion of information influence the definition of a market and competition within a market. If markets mean places where the price and quality of one firm’s offerings influence buyer behaviour about another firm’s products, then customer information, search, and purchasing patterns underlie the definition and redefinition of markets. As customer information and willingness to switch among suppliers change, then the structure of the market changes. Thus, firms may be able to influence market definitions by changing customer information, easing or making harder customer search, and altering the costs and difficulty of customer purchases.

Manage knowledge transfer
Finally, a behavioural perspective emphasises the difficulty of knowledge transfer instead of the efficient market’s assumption of quick diffusion of knowledge. This analysis can be done from two symmetric perspectives—if you have a good tool or product, what determines how long you can keep it to yourself, or alternatively, if a competitor has such a tool or product, how long until you can get it?

Suppose a firm finds a good thing—a profitable niche, a better technique, or whatever. What has to happen before competitors catch on?

First, competitors have to know the good thing exists. Sometimes, this is obvious, however, sometimes it is not. Unless management trumpets them, external observers may be unaware of improvements in internal processes. Furthermore, competitors are likely to observe changes in standard areas of attention for a given product while missing advantages that come from less standard areas. For example, before the quality fanfare, a firm’s quality efforts might have benefited its customers while competitors paid little attention. Competitors track certain things and ignore others.
Consider, for example, Starbucks. Why didn’t a major restaurant chain steal the Starbucks idea and take it global? Employees of major restaurant chains certainly knew of Starbucks but it did not fall neatly into their categories (e.g. it is not ‘fast food’, ‘casual dining’ etc.). Thus, the systems that large restaurant chains use to monitor rivals and identify restaurants with new business models may have excluded Starbucks—it may not have been on their radar screens. As Starbucks did not fall into the standard competitive categories, major competitors did not invest in mimicking its business model. Starbucks has no secret technology—a major firm that mimicked Starbucks while Starbucks was small could have dominated that segment of the US coffee market.

Second, the competitors have to want to copy the good thing. Managers face a continuous stream of tools that claim to provide benefits. We would expect which ones they pursue reflects many factors—expected benefits, uncertainty about the benefits, industry norms and fashions, internal politics, company norms and practices, time available etc. Your competitors might suspect you do something well, but may choose to put their scarce development efforts into other things.

Third, the competitors must be able to copy it. Some good things can be protected with copyrights, patents and so forth. Others may simply be too hard to copy. Many companies try to copy things, but often do not keep at it long—the parade of managerial fashions so common in many companies. We know how hard it is to transfer practices and technologies successfully within a company; transfer is much harder between companies when one is actively trying to prevent such a transfer.22

Depending on whether you are in the firm with the good thing or in a firm trying to find a good thing, these points lead to a variety of actions regarding scanning, selecting and implementing advantageous activities.

Systematic scanning is limited in that it looks where you know to look

If we want to find what others do well, we need to scan. In some industries, firms share surprising amounts about managerial techniques and technology through industry associations. Other insights can come from systematic scanning—routinely checking competitor offerings, pricing etc. However, systematic scanning is limited in that it looks where you know to look. Real advantages often come from innovations in places people did not know to look. Breakthrough innovations often come from firms outside an industry which current industry occupants may not see as competitors. New ideas may simply fall through the cracks. For such ideas, we may want to create a workforce that openly seeks ideas and investigates related businesses, along with systems that help funnel their findings to decision makers.

If you generate a stream of potential innovations to consider, choosing which innovations to pursue is difficult. We often have poor information and ability to forecast the outcomes of adopting innovations. Depending on incentives and individual differences, forecasts can be wildly optimistic or exceedingly conservative. Differing innovations offer differing and incomparable benefits and costs. Simple decision rules (for example, net present value) appear of limited benefit.

An options perspective, as a perspective not a decision rule, may help. Real options occur when an investment today creates a different investment choice later. Generally, in such sequential investment processes, the decision maker can avoid many of the negative outcomes by ceasing to invest when the project appears to be going badly. An options analysis results in a general suggestion to look for projects with high potential rather than simply high average expected outcome. The mean expected outcome may combine very high and low potential outcomes, but in an options world managers may be able to pull the plug and avoid realising the negative outcomes. Options thinking emphasises thinking about where a given decision positions you in the future—how good are the alternatives it creates for future decisions.

Uncertainty also suggests thinking about the organisational issues of implementing a given inno-
vation rather than taking such things for granted. You may have greater success with a modest innovation that excites and stimulates organisational efforts rather than a potentially better innovation that lacks champions and creates no energy.

Finally, uncertainty may suggest taking a portfolio approach to trying out innovations. You often do not really know how an innovation works until you try it. Thus, trying out several innovations may increase the chances of finding a good one. On the other hand, trying an excessive number of things may result in insufficient effort directed at each one and employees deciding management simply plays with fads. Sometimes it helps to try things on a small but committed basis.

On the other side, if you have something that gives advantage, you want to keep that advantage. You may hide it by mixing it with other factors. When a division contains several businesses, line of business accounting offers little information on the cost structure for any of the businesses. Instead of advertising the way a capability is created, advertise just the value the system produces. Use designs that make operations difficult for outsiders to understand. Instead of a transparent decision rule, a proprietary programme may make the outcomes less obvious. Instead of doing all the activities in one place, distributing them may make it harder for anyone to know how it all works.

**Conclusion**

Just as some Finance scholars appear ready to discard rationality and equilibrium assumptions for more realistic views of investors, some Strategic Management scholars want to adopt rationality and equilibrium assumptions for Strategic Management. However, if empirical tests can reject rationality in financial markets, claimed to be the most efficient of markets, it makes little sense to adopt this assumption in the much less perfect markets where most corporate strategy plays out.

The most direct implication of behavioural finance scholars’ rejection of efficient capital markets for strategic management researchers is the need to reconsider our approaches to strategic measurement. The basic rules underlying analysis of divisional performance, and strategic investments need to be reconsidered. Indeed, this is an area for practical research—given the set of approved and implemented projects, can we identify better ways to create analyses and better decision rules.

Given the aforementioned limitations of importing assumptions from finance into strategic management research, we should reject prescriptions based on rationality and equilibrium analysis. Alternatively, recognising how cognitively limited actors make decisions and actually interact will better inform how firms achieve and sustain competitive advantage. While some progress has been made on developing prescriptions from this perspective, much remains to be done. A full-fledged behavioural approach might help both finance and strategic management fields achieve consistent, productive scholarship that offers realistic prescriptions for managers facing inefficient markets in disequilibrium and challenges to effective knowledge transfer. Instead of taking the extreme position of equilibrium market conditions, strategy scholars should build on technology transfer literature to understand when and what will transfer. Recent work on strategic alliances explores the dynamics of co-operation between firms in competitive markets and offers a potential foundation for more thorough behavioural work on the extent to which firms learn.23 Research evidence shows that firms with a clear intent to learn do, indeed, accomplish this goal.

**References**

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**Biographies**

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