CHAPTER ONE

THE GROWTH IMPERATIVE

Financial markets relentlessly pressure executives to grow and keep growing faster and faster. Is it possible to succeed with this mandate? Don't the innovations that can satisfy investors' demands for growth require taking risks that are unacceptable to those same investors? Is there a way out of this dilemma?

This is a book about how to create new growth in business. Growth is important because companies create shareholder value through profitable growth. Yet there is powerful evidence that once a company's core business has matured, the pursuit of new platforms for growth entails daunting risk. Roughly one company in ten is able to sustain the kind of growth that translates into an above-average increase in shareholder returns over more than a few years.[1] Too often the very attempt to grow causes the entire corporation to crash. Consequently, most executives are in a no-win situation: equity markets demand that they grow, but it's hard to know *how* to grow. Pursuing growth the wrong way can be worse than no growth at all.

Consider AT&T. In the wake of the government-mandated divestiture of its local telephony services in 1984, AT&T became primarily a long distance telecommunications services provider. The break-up agreement freed the company to invest in new businesses, so management almost immediately began seeking avenues for growth and the shareholder value that growth creates.

The first such attempt arose from a widely shared view that computer systems and telephone networks were going to converge. AT&T first tried to build its own computer division in order to position itself at that intersection, but was able to do no better than annual losses of $200 million. Rather than retreat from a business that had proved to be unassailable from the outside, the company decided in 1991 to bet bigger still, acquiring NCR, at the time the world's fifth-largest computer maker, for $7.4 billion. That proved only to be a down payment: AT&T lost another $2 billion trying to make the acquisition work. AT&T finally abandoned this growth vision in 1996, selling NCR for $3.4 billion, about a third of what it had invested in the opportunity.

But the company *had* to grow. So even as the NCR acquisition was failing, AT&T was seeking growth opportunities in technologies closer to its core. In light of the success of the wireless services that several of its spun-off local telephone companies had achieved, in 1994 the company bought McCaw Cellular, at the time the largest national wireless carrier in the United States, for $11.6 billion, eventually spending $15 billion in total on its own wireless business. When Wall Street analysts subsequently complained that they were unable to properly value the combined higher-growth wireless business within the lower-growth wireline company, AT&T decided to create a separately traded stock for the wireless business in 2000. This valued the business at $10.6 billion, about two-thirds of the investment AT&T had made in the venture.

But that move left the AT&T wireline stock right where it had started, and the company *had* to grow. So in 1998 it embarked upon a strategy to enter and reinvent the local telephony business with broadband technology. Acquiring TCI and MediaOne for a combined price of $112 billion made AT&T Broadband the largest cable operator in the United States. Then, more quickly than anyone could have foreseen, the difficulties in implementation and integration proved insurmountable. In 2000, AT&T agreed to sell its cable assets to Comcast for $72 billion.[2]

In the space of a little over ten years, AT&T had wasted about $50 billion and destroyed even more in shareholder value—all in the hope of *creating* shareholder value through growth.

The bad news is that AT&T is not a special case. Consider Cabot Corporation, the world's major producer of carbon black, a compound that imparts to products such as tires many of their most important properties. This business has long been very strong, but the core markets haven't grown rapidly. To create the growth that builds shareholder value, Cabot's executives in the early 1980s launched several aggressive growth initiatives in advanced materials, acquiring a set of promising specialty metals and high-tech ceramics businesses. These constituted operating platforms into which the company would infuse new process and materials technology that was emerging from its own research laboratories and work it had sponsored at MIT.

Wall Street greeted these investments to accelerate Cabot's growth trajectory with enthusiasm and drove the company's share price to triple the level at which it had languished prior to these initiatives. But as the losses created by Cabot's investments in these businesses began to drag the entire corporation's earnings down, Wall Street hammered the stock. While the overall market appreciated at a robust rate between 1988 and 1991, Cabot's shares dropped by more than half. In the early 1990s, feeling pressure to boost earnings, Cabot's board brought in new management whose mandate was to shut down the new businesses and refocus on the core. As Cabot's profitability rebounded, Wall Street enthusiastically doubled the company's share price. The problem, of course, was that this turnaround left the new management team no better off than their predecessors: desperately seeking growth opportunities for mature businesses with limited prospects.[3]

We could cite many cases of companies' similar attempts to create new-growth platforms after the core business had matured. They follow an all-too-similar pattern. When the core business approaches maturity and investors demand new growth, executives develop seemingly sensible strategies to generate it. Although they invest aggressively, their plans fail to create the needed growth fast enough; investors hammer the stock; management is sacked; and Wall Street rewards the new executive team for simply restoring the *status quo ante*: a profitable but low-growth core business.[4]

Even expanding firms face a variant of the growth imperative. No matter how fast the growth treadmill is going, it is not fast enough. The reason: Investors have a pesky tendency to discount into the *present* value of a company's stock price whatever rate of growth they *foresee* the company achieving. Thus, even if a company's core business is growing vigorously, the only way its managers can deliver a rate of return to shareholders in the future that exceeds the risk-adjusted market average is to grow *faster* than shareholders expect. Changes in stock prices are driven not by simply the *direction* of growth, but largely by *unexpected* changes in the *rate of change* in a company's earnings and cash flows. Hence, one company that is projected to grow at 5 percent and in fact keeps growing at 5 percent and another company that is projected to grow at 25 percent and delivers 25 percent growth will both produce for future investors a market-average risk-adjusted rate of return in the future.[5] A company must deliver the rate of growth that the market is projecting just to keep its stock price from falling. It must *exceed* the consensus forecast rate of growth in order to boost its share price. This is a heavy, omnipresent burden on every executive who is sensitive to enhancing shareholder value.[6]

It's actually even harder than this. That canny horde of investors not only discounts the expected rate of growth of a company's *existing* businesses into the present value of its stock price, but also discounts the growth from new, yet-to-be-established lines of business that they expect the management team to be able to create in the future. The magnitude of the market's bet on growth from unknown sources is, in general, based on the company's track record. If the market has been impressed with a company's historical ability to leverage its strengths to generate new lines of business, then the component of its stock price based on growth from unknown sources will be large. If a company's past efforts to create new-growth businesses have not borne fruit, then its market valuation will be dominated by the projected cash flow from known, established businesses.

Table 1-1 presents one consulting firm's analysis of the share prices of a select number of *Fortune* 500 companies, showing the proportion of each firm's share price on August 21, 2002, that was attributable to cash generated by existing assets, versus cash that investors expected to be generated by new investments.[7] Of this sample, the company that was on the hook at that time to generate the largest percentage of its total growth from future investments was Dell Computer. Only 22 percent of its share price of $28.05 was justified by cash thrown off by the company's present assets, whereas 78 percent of Dell's valuation reflected investors' confidence that the company would be able to invest in new assets that would generate whopping amounts of cash. Sixty-six percent of Johnson & Johnson's market valuation and 37 percent of Home Depot's valuation were grounded in expectations of growth from yet-to-be-made investments. These companies were on the hook for *big* numbers. On the other hand, only 5 percent of General Motors's stock price on that date was predicated on future investments. Although that's a chilling reflection of the track record of GM's former management in creating new-growth businesses, it means that if the present management team does a better job, the company's share price could respond handsomely.

Probably the most daunting challenge in delivering growth is that if you fail once to deliver it, the odds that you ever will be able to deliver in the future are very low. This is the conclusion of a remarkable study, *Stall Points,* that the Corporate Strategy Board published in 1998.[8] It examined the 172 companies that had spent time on *Fortune*'s list of the 50 largest companies between 1955 and 1995. Only 5 percent of these companies were able to sustain a real, inflation-adjusted growth rate of more than 6 percent across their entire tenure in this group. The other 95 percent reached a point at which their growth simply stalled, to rates at or below the rate of growth of the gross national product (GNP). Stalling is understandable, given our expectations that all growth markets become saturated and mature. What is scary is that of all these companies whose growth had stalled, only 4 percent were able to successfully reignite their growth even to a rate of 1 percent above GNP growth. Once growth had stalled, in other words, it proved nearly impossible to restart it.

The equity markets brutally punished those companies that allowed their growth to stall. Twenty-eight percent of them lost more than 75 percent of their market capitalization. Forty-one percent of the companies saw their market value drop by between 50 and 75 percent when they stalled, and 26 percent of the firms lost between 25

TABLE 1-1

Portion of Selected Firms' Market Value That Was Based on Expected Returns from New Investments on August 21, 2002

Fortune 500 rank	Company Name	Share Price	Percent of Valuation That Was Based on: New Investments	Existing Assets
53	Dell Computer	$28.05	78%	22%
47	Johnson & Johnson	$56.20	66%	34%
35	Procter & Gamble	$90.76	62%	38%
6	General Electric	$32.80	60%	40%
77	Lockheed Martin	$62.16	59%	41%
1	Wal-Mart Stores	$53.88	50%	50%
65	Intel	$19.15	49%	51%
49	Pfizer	$34.92	48%	52%
9	IBM	$81.93	46%	54%
24	Merck	$53.80	44%	56%
92	Cisco Systems	$15.00	42%	58%
18	Home Depot	$33.86	37%	63%
16	Boeing	$28.36	30%	70%
11	Verizon	$31.80	21%	79%
22	Kroger	$22.20	13%	87%
32	Sears Roebuck	$36.94	8%	92%
37	AOL Time Warner	$35.00	8%	92%
3	General Motors	$49.40	5%	95%
81	Phillips Petroleum	$35.00	3%	97%

Source: CSFB/HOLT; Deloitte Consuting analysis.

and 50 percent of their value. The remaining 5 percent lost less than 25 percent of their market capitalization. This, of course, increased pressure on management to regenerate growth, and to do so quickly—which made it all the more difficult to succeed. Managers cannot escape the mandate to grow.[9] Yet the odds of success, if history is any guide, are frighteningly low.

Is Innovation a Black Box?

Why is achieving and sustaining growth so hard? One popular answer is to blame managers for failing to generate new growth—implying that more capable and prescient people could have succeeded. The solve-the-problem-by-finding-a-better-manager approach might have credence if failures to restart growth were isolated events. Study after study, however, concludes that about 90 percent of all publicly traded companies have proved themselves unable to sustain for more than a few years a growth trajectory that creates above-average shareholder returns.[10] Unless we believe that the pool of management talent in established firms is like some perverse Lake Wobegon, where 90 percent of managers are below average, there has to be a more fundamental explanation for why the vast majority of good managers has not been able to crack the problem of sustaining growth.

A second common explanation for once-thriving companies' inability to sustain growth is that their managers become risk averse. But the facts refute this explanation, too. Corporate executives often bet the future of billion-dollar enterprises on an innovation. IBM bet its farm on the System 360 mainframe computer, and won. DuPont spent $400 million on a plant to make Kevlar tire cord, and lost. Corning put billions on the line to build its optical fiber business, and won big. More recently it sold off many of its other businesses in order to invest more in optical telecommunications, and has been bludgeoned. *Many* of the executives who have been unable to create sustained corporate growth have evidenced a strong stomach for risk.

There is a third, widely accepted explanation for why growth seems so hard to achieve repeatedly and well, which we also believe does not hold water: Creating new-growth businesses is simply unpredictable.

Many believe that the odds of success are just that—odds—and that they are low. Many of the most insightful management thinkers have accepted the assumption that creating growth is risky and unpredictable, and have therefore used their talents to help executives manage this unpredictability. Recommendations about letting a thousand flowers bloom, bringing Silicon Valley inside, failing fast, and accelerating selection pressures are all ways to deal with the allegedly irreducible unpredictability of successful innovation.[11] The structure of the venture capital industry is in fact a testament to the pervasive belief that we cannot predict which new-growth businesses will succeed. The industry maxim says that for every ten investments—all made in the belief they would succeed—two will fail outright, six will survive as the walking wounded, and two will hit the home runs on which the success of the entire portfolio turns. Because of this belief that the process of business creation is unfathomable, few have sought to pry open the black box to study the *process* by which new-growth businesses are created.

We do not accept that most companies' growth stalls because the odds of success for the next growth business they launch are impossibly low. The historical results may indeed seem random, but we believe it is because the process for creating new-growth businesses has not yet been well understood. In this book we intend to pry open the black box and study the processes that lead to success or failure in new-growth businesses.

To illustrate why it is important to understand the processes that create those results, consider these strings of numbers:

1, 2, 3, 4, 5, 6
75, 28, 41, 26, 38, 64

Which of these would you say is random, and which is predictable? The first string looks predictable: The next two numbers should be 7 and 8. But what if we told you that it was actually the winning numbers for a lottery, drawn from a drum of tumbling balls, whereas the second is the sequence of state and county roads one would follow on a scenic tour of the northern rim of Michigan's Upper Peninsula on the way from Sault Ste. Marie, Ontario to Saxon, Wisconsin?

Given the route implied by the first six roads, you can reliably predict the next two numbers—2 and 122—from a map. The lesson: You cannot say, just by looking at the result of the process, whether the process that created those results is capable of generating predictable output. You must understand the process itself.

The Forces That Shape Innovation

What can make the process of innovation more predictable? It does *not* entail learning to predict what *individuals* might do. Rather, it comes from understanding the *forces* that act upon the individuals involved in building businesses—forces that powerfully influence what managers choose and cannot choose to do.

Rarely does an idea for a new-growth business emerge fully formed from an innovative employee's head. No matter how well articulated a concept or insight might be, it must be shaped and modified, often significantly, as it gets fleshed out into a business plan that can win funding from the corporation. Along the way, it encounters a number of highly predictable forces. Managers as individuals might indeed be idiosyncratic and unpredictable, but they all face forces that are similar in their mechanism of action, their timing, and their impact on the character of the product and business plan that the company ultimately attempts to implement.[12] Understanding and managing these forces can make innovation more predictable.

The action and impact of these forces in shaping ideas into business plans is illustrated in a case study of the Big Idea Group (BIG), a company that identifies, develops, and markets ideas for new toys.[13] After quoting a senior executive of a multibillion-dollar toy company who complained that there have been no exciting new toy ideas for years, the case then chronicles how BIG attacks this problem—or rather, this opportunity.

BIG invites mothers, children, tinkerers, and retirees who have ideas for new toys to attend "Big Idea Hunts," which it convenes in locations across the country. These guests present their ideas to a panel of experts whose intuition BIG executives have come to trust. When the panel sees a good idea, BIG licenses it from the inventor and over the next several months shapes the idea into a business plan with a working prototype that they believe will sell. BIG then licenses the product to a toy company, which produces and markets it through its own channels. The company has been extraordinarily successful at finding, developing, and deploying into the market a sequence of truly exciting growth products.

How can there be such a flowering of high-potential new product opportunities in BIG's system, and such a dearth of opportunities in the large toy company? In discussing the case, students often suggest that the product developers in the toy company just aren't as creative, or that the executives of the major company are just too risk averse. If these diagnoses were true, the company would simply need to find more creative managers who could think outside the box. But a parade of people has cycled through the toy company, and none has been able to crack the apparent lack of exciting toy ideas. Why?

The answer lies in the process by which the ideas get shaped. Midlevel managers play a crucial role in *every* company's innovation process, as they shepherd partially formed ideas into fully fledged business plans in an effort to win funding from senior management. It is the middle managers who must decide which of the ideas that come bubbling in or up to them they will support and carry to upper management for approval, and which ideas they will simply allow to languish. This is a key reason why companies employ middle managers in the first place. Their job is to sift the good ideas from the bad and to make good ideas so much better that they readily secure funding from senior management.

How do they sift and shape? Middle managers typically hesitate to throw their weight behind new product concepts whose market is not assured. If a market fails to materialize, the company will have wasted millions of dollars. The system therefore mandates that midlevel managers support their proposals with credible data on the size and growth potential of the markets that each idea targets. Opinions and feedback from significant customers add immeasurably to the credibility of claims that an idea has potential. Where does this evidence come from, given that the product hasn't yet been fully developed? It typically comes from existing customers and markets for similar products that have been successful in the past.

Personal factors are at work in this shaping process, too. Managers who back ideas that flop often find their prospects for promotion effectively truncated. In fact, ambitious managers hesitate even to propose ideas that senior managers are not likely to approve. If they favor an idea that their superiors subsequently judge to be weak, their reputation for good judgment can be tarnished among the very executives they hope to impress. Furthermore, companies' management development programs rarely leave their most talented middle managers in a position for longer than a few years—they move them to new assignments to broaden their skills and experience. What this means, however, is that middle managers who want a reputation for delivering results will be inclined to promote only those new-growth ideas that will pay off within the time that they reside in that particular job.

The process of sorting through and packaging ideas into plans that can win funding, in other words, shapes those ideas to resemble the ideas that were approved and became successful in the past. The processes have in fact evolved to weed out business proposals that target markets where demand might be small. The problem for growth-seeking managers, of course, is that the exciting growth markets of tomorrow are small today.

This is why the senior managers at the major toy company and at BIG can live in the same world and yet see such different things. In every sizable company, not just in the toy business, the set of ideas that has been processed and packaged for top management approval is *very* different from the population of ideas that is bubbling at the bottom.

A dearth of good ideas is rarely the core problem in a company that struggles to launch exciting new-growth businesses. The problem is in the shaping process. Potentially innovative new ideas seem inexorably to be recast into attempts to make existing customers still happier. We believe that many of the ideas that emerge from this packaging and shaping process as me-too innovations could just as readily be shaped into business plans that create truly disruptive growth. Managers who understand these forces and learn to harness them in making key decisions will develop successful new-growth businesses much more consistently than historically has seemed possible.[14]

Where Predictability Comes From: Good Theory

The quest for predictability in an endeavor as complex as innovation is not quixotic. What brings predictability to any field is a body of well-researched *theory*—contingent statements of what causes what and why. Executives often discount the value of management theory because it is associated with the word *theoretical,* which connotes *impractical*. But theory is consummately practical. The law of gravity, for example, actually is a theory—and it is useful. It allows us to predict that if we step off a cliff, we will fall.[15]

Even though most managers don't think of themselves as being theory driven, they are in reality voracious consumers of theory. Every time managers make plans or take action, it is based on a mental model in the back of their heads that leads them to believe that the action being taken will lead to the desired result.[16] The problem is that managers are rarely aware of the theories they are using—and they often use the wrong theories for the situation they are in. It is the absence of conscious, trustworthy theories of cause and effect that makes success in building new businesses seem random.

To help executives to know whether and when they can trust the recommendations from management books or articles (including this one!) that they read for guidance as they build their businesses, we describe in the following sections a model of how good theories are built and used. We will repeatedly return to this model to illustrate how bad theory has caused growth builders to stumble in the past, and how the use of sound theory can remove many of the causes of failure.[17]

How Theories Are Built

The process of building solid theory has been researched in several disciplines, and scholars seem to agree that it proceeds in three stages. It begins by describing the phenomenon that we wish to understand. In physics, the phenomenon might be the behavior of high-energy particles. In the building of new businesses, the phenomena of interest are the things that innovators do in their efforts to succeed, and what the results of those actions are. Bad management theory results when researchers impatiently observe one or two success stories and then assume that they have seen enough.

After the phenomenon has been thoroughly characterized, researchers can then begin the second stage, which is to classify the phenomenon into categories. Juvenile-onset versus adult-onset diabetes is an example from medicine. Vertical and horizontal integration are categories of corporate diversification. Researchers need to categorize in order to highlight the most meaningful differences in the complex array of phenomena.

In the third stage, researchers articulate a theory that asserts what causes the phenomenon to occur, and why. The theory must also show whether and why the same causal mechanism might result in different outcomes, depending on the category or situation. The process of theory building is iterative, as researchers and managers keep cycling through these three steps, refining their ability to predict what actions will cause what results, under what circumstances.[18]

Getting the Categories Right

The middle stage in this cycle—getting the categories right—is the key to developing useful theory. To see why, imagine going to your medical doctor seeking treatment for a particular set of symptoms, and before you have a chance to describe what ails you, the physician hands you a prescription and tells you to "take two of these and call me in the morning."

"But how do you know this will help me?" you ask. "I haven't told you what's wrong."

"Why wouldn't it work?" comes the reply. "It cured my previous two patients just fine."

No sane patient would accept medicine like this. But academics, consultants, and managers *routinely* dispense and accept remedies to management problems in this manner. When something has worked for a few "excellent" companies, they readily advise all other companies that taking the same medicine will be good for them as well. One reason why the outcomes of innovation appear to be random is that many who write about strategy and management ignore categorization. They observe a few successful companies and then write a book recommending that other managers do the same things to be successful too—without regard for the possibility that there might be some circumstances in which their favorite solution is a bad idea.[19]

For example, thirty years ago many writers asserted that vertical integration was the key to IBM's extraordinary success. But in the late 1990s we read that *non*-integration explained the triumph of outsourcing titans such as Cisco and Dell. The authors of "best practices" gospels such as these are no better than the doctor we introduced previously. The critical question that these researchers need to resolve is, "What are the *circumstances* in which being integrated is competitively critical, and when is a strategy of partnering and outsourcing more likely to lead to success?"

Because theory-building scholars struggle to define the right and relevant categorization of circumstances, they rarely can define the circumstances immediately. Early studies almost always sort researchers' observations into categories defined by the *attributes* of the phenomena themselves. Their assertions about the actions or events that lead to the results at this point can only be statements about *correlation* between attributes and results, not about causality. This is the best they can do in early theory-building cycles.

Consider, for illustration, the history of man's attempts to fly. Early researchers observed strong correlations between being able to fly and having feathers and wings. Possessing these attributes had a high *correlation* with the ability to fly, but when humans attempted to follow the "best practices" of the most successful flyers by strapping feathered wings onto their arms, jumping off cliffs, and flapping hard, they were not successful—because as strong as the correlations were, the would-be aviators had not understood the fundamental causal mechanism that enabled certain animals to fly. It was not until Bernoulli's study of fluid mechanics helped him articulate the mechanism through which airfoils create lift that human flight began to be *possible*. But understanding the mechanism itself still wasn't enough to make the ability to fly perfectly *predictable*. Further research, entailing careful experimentation and measurement under various conditions, was needed to identify the *circumstances* in which that mechanism did and did not yield the desired result.

When the mechanism did not result in successful flight, researchers had to carefully decipher *why*—what it was about the circumstances in which the unexpected result occurred that led to failure. Once categories could be stated in terms of the different types of circumstances in which aviators might find themselves, then aviators could predict the conditions in which flight was and was not possible. They could develop technologies and techniques for successfully flying in those circumstances where flight was viable. And they could teach aviators how to recognize when the circumstances were changing, so that they could change their methods appropriately. Understanding the mechanism (what causes what, and why) made flight possible; understanding the categories of circumstances made flight predictable.[20]

How did aviation researchers know what the salient boundaries were between these categories of circumstance? As long as a change in conditions did not require change in the way the pilot flew the plane, the boundary between those conditions didn't matter. The circumstance boundaries that mattered were those that mandated a fundamental change in piloting techniques in order to keep the plane flying successfully.

Similar breakthroughs in management research increase the predictability of creating new-growth businesses. Getting beyond correlative assertions such as "Big companies are slow to innovate," or "In our sample of successful companies, each was run by a CEO who had been promoted from within," the breakthrough researcher first discovers the fundamental *causal* mechanism behind the phenomena of success. This allows those who are looking for "an answer" to get beyond the wings-and-feathers mind-set of copying the attributes of successful companies. The foundation for predictability only begins to be built when the researcher sees the same causal mechanism create a *different* outcome from what he or she expected—an anomaly. This prompts the researcher to define what it was about the circumstance or circumstances in which the anomaly occurred that caused the identical mechanism to result in a different outcome.

How can we tell what the right categorization is? As in aviation, a boundary between circumstances is salient only when executives need to use fundamentally different management techniques to succeed in the different circumstances defined by that boundary. If the same statement of cause and effect leads to the same outcome in two circumstances, then the distinction between those circumstances is not meaningful for the purposes of predictability.

To know for certain what circumstances they are in, managers also must know what circumstances they are *not* in. When collectively exhaustive and mutually exclusive *categories of circumstances* are defined, things get predictable: We can state what will cause what and why, and can predict how that statement of causality might vary by circumstance. Theories built on categories of circumstances become easy for companies to employ, because managers live and work in circumstances, not in attributes.[21]

When managers ask questions such as "Does this apply to my industry?" or "Does it apply to service businesses as well as product businesses?" they really are probing to understand the circumstances. In our studies, we have observed that industry-based or product/service-based categorization schemes almost never constitute a useful foundation for reliable theory. *The Innovator's Dilemma*, for example, described how the same mechanism that enabled entrant companies to up-end the leading established firms in disk drives and computers also toppled the leading companies in mechanical excavators, steel, retailing, motorcycles, accounting software, and motor controls.[22] The circumstances that mattered were not what industry you were in. Rather, there was a mechanism—the resource allocation process—that caused the established leaders to win the competitive fights when an innovation was financially attractive to their business model. The same mechanism disabled the established leaders when they were attacked by disruptive innovators—whose products, profit models, and customers were not attractive.

We can trust a theory only when its statement of what actions will lead to success describe how this will vary as a company's circumstances change.[23] This is a major reason why the outcomes of innovation efforts have seemed quite random: Shoddy categorization has led to one-size-fits-all recommendations that in turn have led to the wrong results in many circumstances.[24] It is the ability to begin thinking and acting in a circumstance-contingent way that brings predictability to our lives.

We often admire the intuition that successful entrepreneurs seem to have for building growth businesses. When they exercise their intuition about what actions will lead to the desired results, they really are employing theories that give them a sense of the right thing to do in various circumstances. These theories were not there at birth: They were learned through a set of experiences and mentors earlier in life.

If some people have learned the theories that we call intuition, then it is our hope that these theories also can be taught to others. This is our aspiration for this book. We hope to help managers who are trying to create new-growth businesses use the best research we have been able to assemble to learn how to match their actions to the circumstances in order to get the results they need. As our readers use these ways of thinking over and over, we hope that the thought processes inherent in these theories can become part of their intuition as well.

We have written this book from the perspective of senior managers in established companies who have been charged to maintain the health and vitality of their firms. We believe, however, that our ideas will be just as valuable to independent entrepreneurs, start-up companies, and venture capital investors. Simply for purposes of brevity, we will use the term *product* in this book when we describe what a company makes or provides. We mean, however, for this to encompass product *and* service businesses, because the concepts in the book apply just as readily to both.

The Outline of This Book

The Innovator's Dilemma summarized a theory that explains how, under certain circumstances, the mechanism of profit-maximizing resource allocation causes well-run companies to get killed. *The Innovator's Solution,* in contrast, summarizes a set of theories that can guide managers who need to grow new businesses with predictable success—to become the disruptors rather than the disruptees—and ultimately kill the well-run, established competitors. To succeed predictably, disruptors must be good theorists. As they shape their growth business to be disruptive, they must align every critical process and decision to fit the disruptive circumstance.

Because building successful growth businesses is such a vast topic, this book focuses on nine of the most important decisions that all managers must make in creating growth—decisions that represent key actions that drive success inside the black box of innovation. Each chapter offers a specific theory that managers can use to make one of these decisions in a way that greatly improves their probability of success. Some of this theory has emerged from our own studies, but we are indebted to many other scholars for much of what follows. Those whose work we draw upon have contributed to improving the predictability of business building because their assertions of causality have been built upon circumstance-based categories. It is because of their careful work that we believe that managers can begin using these theories explicitly as they make these decisions, trusting that their predictions will be applicable and reliable, given the circumstances that they are in.

The following list summarizes the questions we address.

- *Chapter 2:* How can we beat our most powerful competitors? What strategies will result in the competitors killing us, and what courses of action could actually give us the upper hand?
- *Chapter 3:* What products should we develop? Which improvements over previous products will customers enthusiastically reward with premium prices, and which will they greet with indifference?
- *Chapter 4:* Which initial customers will constitute the most viable foundation upon which to build a successful business?
- *Chapter 5:* Which activities required to design, produce, sell, and distribute our product should our company do internally, and which should we rely upon our partners and suppliers to provide?
- *Chapter 6:* How can we be sure that we maintain strong competitive advantages that yield attractive profits? How can we tell when commoditization is going to occur, and what can we do to keep earning attractive returns?

- *Chapter 7:* What is the best organizational structure for this venture? What organizational unit(s) and which managers should contribute to and be responsible for its success?
- *Chapter 8:* How do we get the details of a winning strategy right? When is flexibility important, and when will flexibility cause us to fail?
- *Chapter 9:* Whose investment capital will help us succeed, and whose capital might be the kiss of death? What sources of money will help us most at different stages of our development?
- *Chapter 10:* What role should the CEO play in sustaining the growth of the business? When should CEOs keep their hands off the new business, and when should they become involved?

The issues that we tackle in these chapters are critical, but they cannot constitute an exhaustive list of the questions that should be relevant to launching a new-growth business. We can simply hope that we have addressed the most important ones, so that although we cannot make the creation of new-growth businesses perfectly risk free, we *can* help managers take major steps in that direction.

Notes

1. Although we have not performed a true meta-analysis, there are four recently published studies that seem to converge on this estimate that roughly one company in ten succeeds at sustaining growth. Chris Zook and James Allen found in their 2001 study *Profit from the Core* (Boston: Harvard Business School Press) that only 13 percent of their sample of 1,854 companies were able to grow consistently over a ten-year period. Richard Foster and Sarah Kaplan published a study that same year, *Creative Destruction* (New York: Currency/Doubleday), in which they followed 1,008 companies from 1962 to 1998. They learned that only 160, or about 16 percent of these firms, were able merely to survive this time frame, and concluded that the perennially outperforming company is a chimera, something that has never existed at all. Jim Collins also published his *Good to Great* (New York: HarperBusiness) in 2001, in which he examined a universe of 1,435 companies over thirty years (1965–1995). Collins found only 126, or about 9 percent, that had managed to outperform equity market averages for a decade or more. The Corporate Strategy Board's findings in *Stall Points*

(Washington, DC: Corporate Strategy Board, 1988), which are summarized in detail in the text, show that 5 percent of companies in the *Fortune* 50 successfully maintained their growth, and another 4 percent were able to reignite some degree of growth after they had stalled. The studies all support our assertion that a 10 percent probability of succeeding in a quest for sustained growth is, if anything, a generous estimate.

2. Because all of these transactions included stock, "true" measures of the value of the different deals are ambiguous. Although when a deal actually closes, a definitive value can be fixed, the implied value of the transaction at the time a deal is announced can be useful: It signals what the relevant parties were willing to pay and accept at a point in time. Stock price changes subsequent to the deal's announcement are often a function of other, exogenous events having little to do with the deal itself. Where possible, we have used the value of the deals at announcement, rather than upon closing. Sources of data on these various transactions include the following:

NCR

"Fatal Attraction (AT&T's Failed Merger with NCR)," *The Economist,* 23 March 1996.

"NCR Spinoff Completes AT&T Restructure Plan," *Bloomberg Business News,* 1 January 1997.

McCaw and AT&T Wireless Sale

The Wall Street Journal, 21 September 1994.

"AT&T Splits Off AT&T Wireless," AT&T news release, 9 July 2001.

AT&T, TCI, and MediaOne

"AT&T Plans Mailing to Sell TCI Customers Phone, Web Services," The *Wall Street Journal,* 10 March 1999.

"The AT&T-Mediaone Deal: What the FCC Missed," *Business Week,* 19 June 2000.

"AT&T Broadband to Merge with Comcast Corporation in $72 Billion Transaction," AT&T news release, 19 December 2001.

"Consumer Groups Still Questioning Comcast-AT&T Cable Merger," Associated Press Newswires, 21 October 2002.

3. Cabot's stock price outperformed the market between 1991 and 1995 as it refocused on its core business, for two reasons. On one side of the equation, demand for carbon black increased in Asia and North America as car sales surged, thereby increasing the demand for tires. On the supply side, two other American-based producers of carbon black exited the industry because

they were unwilling to make the requisite investment in environmental controls, thereby increasing Cabot's pricing power. Increased demand and reduced supply translated into a tremendous increase in the profitability of Cabot's traditional carbon black operations, which was reflected in the company's stock price. Between 1996 and 2000, however, its stock price deteriorated again, reflecting the dearth of growth prospects.

4. An important study of companies' tendency to make investments that fail to create growth was done by Professor Michael C. Jensen: "The Modern Industrial Revolution, Exit, and the Failure of Internal Control Systems," *Journal of Finance* (July 1993): 831–880. Professor Jensen also delivered this paper as his presidential address to the American Finance Association. Interestingly, many of the firms that Jensen cites as having productively reaped growth from their investments were disruptive innovators—a key concept in this book.

   Our unit of analysis in this book, as in Jensen's work, is the individual firm, not the larger system of growth creation made manifest in a free market, capitalist economy. Works such as Joseph Schumpeter's *Theory of Economic Development* (Cambridge, MA: Harvard University Press, 1934) and *Capitalism, Socialism, and Democracy* (New York: London, Harper & Brothers, 1942) are seminal, landmark works that address the environment in which firms function. Our assertion here is that whatever the track record of free market economies in generating growth at the macro level, the track record of individual firms is quite poor. It is the performance of firms within a competitive market to which we hope to contribute.

5. This simple story is complicated somewhat by the market's apparent incorporation of an expected "fade" in any company's growth rate. Empirical analysis suggests that the market does not expect any company to grow, or even survive, forever. It therefore seems to incorporate into current prices a foreseen decline in growth rates from current levels and the eventual dissolution of the firm. This is the reason for the importance of terminal values in most valuation models. This fade period is estimated using regression analysis, and estimates vary widely. So, strictly speaking, if a company is expected to grow at 5 percent with a fade period of forty years, and five years into that forty-year period it is still growing at 5 percent, the stock price would rise at rates that generated economic returns for shareholders, because the forty-year fade period would start over. However, because this qualification applies to companies growing at 5 percent as well as those growing at 25 percent, it does not change the point we wish to make; that is, that the market is a harsh taskmaster, and merely meeting expectations does not generate meaningful reward.

6. On average over their long histories, of course, faster-growing firms yield higher returns. However, the faster-growing firm will have produced higher

returns than the slower-growing firm only for investors in the past. If markets discount efficiently, then the investors who reap above-average returns are those who were fortunate enough to have bought shares in the past when the future growth rate had not been fully discounted into the price of the stock. Those who bought when the future growth potential already had been discounted into the share price would not receive an above-market return. An excellent reference for this argument can be found in Alfred Rappaport and Michael J. Mauboussin, *Expectations Investing: Reading Stock Prices for Better Returns* (Boston: Harvard Business School Press, 2001). Rappaport and Mauboussin guide investors in methods to detect when a market's expectations for a company's growth might be incorrect.

7. These were the closing market prices for these companies' common shares on August 21, 2002. There is no significance to that particular date: It is simply the time when the analysis was done. HOLT Associates, a unit of Credit Suisse First Boston (CSFB), performed these calculations using proprietary methodology applied to publicly available financial data. The percent future is a measure of how much a company's current stock price can be attributed to current cash flows and how much is due to investors' expectations of future growth and performance. As CSFB/HOLT defines it,

> *The percent future is the percentage of the total market value that the market assigns to the company's expected future investment. Percent future begins with the total market value (debt plus equity) less that portion attributed to the present value of existing assets and investments and divides this by the total market value of debt and equity.*

CSFB/Holt calculates the present value of existing assets as the present value of the cash flows associated with the assets' wind down and the release of the associated nondepreciating working capital. The HOLT CFROI valuation methodology includes a forty-year fade of returns equal to the total market's average returns.

Percent Future = [Total Debt and Equity (market) – Present Value Existing Assets]/[Total Debt and Equity (market)]

The companies listed in table 1-1 are not a sequential ranking of *Fortune* 500 companies, because some of the data required to perform these calculations were not available for some companies. The companies listed in this table were chosen only for illustrative purposes, and were not chosen in any way to suggest that any company's share price is likely to increase or decline. For more information on the methodology that HOLT used, see <http://www.holtvalue.com>.

8. See *Stall Points* (Washington, DC: Corporate Strategy Board, 1998).
9. In the text we have focused only on the pressure that equity markets impose on companies to grow, but there are many other sources of intense pressure. We'll mention just a couple here. First, when a company is growing, there are increased opportunities for employees to be promoted into new management positions that are opening up above them. Hence, the potential for growth in managerial responsibility and capability is much greater in a growing firm than in a stagnant one. When growth slows, managers sense that their possibilities for advancement will be constrained not by their personal talent and performance, but rather by how many years must pass before the more senior managers above them will retire. When this happens, many of the most capable employees tend to leave the company, affecting the company's abilities to regenerate growth.

   Investment in new technologies also becomes difficult. When a growing firm runs out of capacity and must build a new plant or store, it is easy to employ the latest technology. When a company has stopped growing and has excess manufacturing capacity, proposals to invest in new technology typically do not fare well, since the full capital cost and the average manufacturing cost of producing with the new technology are compared against the marginal cost of producing in a fully depreciated plant. As a result, growing firms typically have a technology edge over slow-growth competitors. But that advantage is not rooted so much in the visionary wisdom of the managers as it is in the difference in the circumstances of growth versus no growth.
10. Detailed support for this estimate is provided in note 1.
11. For example, see James Brian Quinn, S*trategies for Change: Logical Incrementalism* (Homewood, IL: R.D. Irwin, 1980). Quinn suggests that the first step that corporate executives need to take in building new businesses is to "let a thousand flowers bloom," then tend the most promising and let the rest wither. In this view, the key to successful innovation lies in choosing the right flowers to tend—and that decision must rely on complex intuitive feelings, calibrated by experience.

    More recent work by Tom Peters (*Thriving on Chaos: Handbook for a Management Revolution* [New York: Knopf/Random House, 1987]) urges innovating managers to "fail fast"—to pursue new business ideas on a small scale and in a way that generates quick feedback about whether an idea is viable. Advocates of this approach urge corporate executives not to punish failures because it is only through repeated attempts that successful new businesses will emerge.

Others draw on analogies with biological evolution, where mutations arise in what appear to be random ways. Evolutionary theory posits that whether a mutant organism thrives or dies depends on its fit with the "selection environment"—the conditions within which it must compete against other organisms for the resources required to thrive. Hence, believing that good and bad innovations pop up randomly, these researchers advise corporate executives to focus on creating a "selection environment" in which viable new business ideas are culled from the bad as quickly as possible. Gary Hamel, for example, advocates creating "Silicon Valley inside"—an environment in which existing structures are constantly dismantled, recombined in novel ways, and tested, in order to stumble over something that actually works. (See Gary Hamel, *Leading the Revolution* [Boston: Harvard Business School Press, 2001].)

We are not critical of these books. They can be very helpful, given the present state of understanding, because if the processes that create innovations were indeed random, then a context within which managers could accelerate the creation and testing of ideas would indeed help. But if the process is *not* intrinsically random, as we assert, then addressing only the context is treating the symptom, not the source of the problem.

To see why, consider the studies of 3M's celebrated ability to create a stream of growth-generating innovations. A persistent highlight of these studies is 3M's "15 percent rule": At 3M, many employees are given 15 percent of their time to devote to developing their own ideas for new-growth businesses. This "slack" in how people spend their time is supported by a broadly dispersed capital budget that employees can tap in order to fund their would-be growth engines on a trial basis.

But what guidance does this policy give to a bench engineer at 3M? She is given 15 percent "slack" time to dedicate to creating new-growth businesses. She is also told that whatever she comes up with will be subject first to internal market selection pressures, then external market selection pressures. All this is helpful information. But none of it helps that engineer create a new idea, or decide which of the several ideas she might create are worth pursuing further. This plight generalizes to managers and executives at all levels in an organization. From bench engineer to middle manager to business unit head to CEO, it is not enough to occupy oneself only with creating a context for innovation that sorts the fruits of that context. Ultimately, every manager must create something of substance, and the success of that creation lies in the decisions managers must make.

All of these approaches create an "infinite regress." By bringing the market "inside," we have simply backed up the problem: How can managers decide which ideas will be developed to the point at which they can be subjected to the selection pressures of their internal market? Bringing the market still deeper inside simply creates the same conundrum. Ultimately, innovators must judge what they will work on and how they will do it —and what they should consider when making those decisions is what is in the black box. The acceptance of randomness in innovation, then, is not a stepping-stone on the way to greater understanding; it is a barrier.

Dr. Gary Hamel was one of the first scholars of this problem to raise with Professor Christensen the possibility that the management of innovation actually has the potential to yield predictable results. We express our thanks to him for his helpful thoughts.

12. The scholars who introduced us to these forces are Professor Joseph Bower of the Harvard Business School and Professor Robert Burgelman of the Stanford Business School. We owe a deep intellectual debt to them. See Joseph L. Bower, *Managing the Resource Allocation Process* (Homewood, IL: Richard D. Irwin, 1970); Robert Burgelman and Leonard Sayles, *Inside Corporate Innovation* (New York: Free Press, 1986); and Robert Burgelman, *Strategy Is Destiny* (New York: Free Press, 2002).
13. Clayton M. Christensen and Scott D. Anthony, "What's the BIG Idea?" Case 9-602-105 (Boston: Harvard Business School, 2001).
14. We have consciously chosen phrases such as "increase the probability of success" because business building is unlikely ever to become perfectly predictable, for at least three reasons. The first lies in the nature of competitive marketplaces. Companies whose actions were perfectly predictable would be relatively easy to defeat. Every company therefore has an interest in behaving in deeply unpredictable ways. A second reason is the computational challenge associated with any system with a large number of possible outcomes. Chess, for example, is a fully determined game: After White's first move, Black should always simply resign. But the number of possible games is so great, and the computational challenge so overwhelming, that the outcomes of games even between supercomputers remain unpredictable. A third reason is suggested by complexity theory, which holds that even fully determined systems that do not outstrip our computational abilities can still generate deeply random outcomes. Assessing the extent to which the outcomes of innovation can be predicted, and the significance of any residual uncertainty or unpredictability, remains a profound theoretical challenge with important practical implications.
15. The challenge of improving predictability has been addressed somewhat successfully in certain of the natural sciences. Many fields of science appear today to be cut and dried—predictable, governed by clear laws of cause and effect, for example. But it was not always so: Many happenings in the natural world seemed very random and unfathomably complex to the ancients and to early scientists. Research that adhered carefully to the scientific

method brought the predictability upon which so much progress has been built. Even when our most advanced theories have convinced scientists that the world is not deterministic, at least the phenomena are predictably random.

Infectious diseases, for example, at one point just seemed to strike at random. People didn't understand what caused them. Who survived and who did not seemed unpredictable. Although the outcome seemed random, however, the process that led to the results was not random—it just was not sufficiently understood. With many cancers today, as in the venture capitalists' world, patients' probabilities for survival can only be articulated in percentages. This is not because the outcomes are unpredictable, however. We just do not yet understand the process.

16. Peter Senge calls theories *mental models* (see Peter Senge, *The Fifth Discipline* [New York: Bantam Doubleday Dell, 1990]). We considered using the term *model* in this book, but opted instead to use the term *theory*. We have done this to be provocative, to inspire practitioners to value something that is indeed of value.
17. A full description of the process of theory building and of the ways in which business writers and academics ignore and violate the fundamental principles of this process is available in a paper that is presently under review, "The Process of Theory Building," by Clayton Christensen, Paul Carlile, and David Sundahl. Paper or electronic copies are available from Professor Christensen's office, cchristensen@hbs.edu. The scholars we have relied upon in synthesizing the model of theory building presented in this paper (and only very briefly summarized in this book) are, in alphabetical order, E. H. Carr, *What Is History?* (New York: Vintage Books, 1961); K. M. Eisenhardt, "Building Theories from Case Study Research," *Academy of Management Review* 14, no. 4 (1989): 532–550; B. Glaser and A. Straus, *The Discovery of Grounded Theory: Strategies of Qualitative Research* (London: Wiedenfeld and Nicholson, 1967); A. Kaplan, *The Conduct of Inquiry: Methodology for Behavioral Research* (Scranton, PA: Chandler, 1964); R. Kaplan, "The Role for Empirical Research in Management Accounting," *Accounting, Organizations and Society* 4, no. 5 (1986): 429–452; T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962); M. Poole and A. Van de Ven, "Using Paradox to Build Management and Organization Theories," *Academy of Management Review* 14, no. 4 (1989): 562–578; K. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959); F. Roethlisberger, *The Elusive Phenomena* (Boston: Harvard Business School Division of Research, 1977); Arthur Stinchcombe, "The Logic of Scientific Inference," chapter 2 in *Constructing Social Theories* (New York: Harcourt,

Brace & World, 1968); Andrew Van de Ven, "Professional Science for a Professional School," in *Breaking the Code of Change,* eds. Michael Beer and Nitin Nohria (Boston: Harvard Business School Press, 2000); Karl E. Weick, "Theory Construction as Disciplined Imagination," *Academy of Management Review* 14, no. 4, (1989): 516–531; and R. Yin, *Case Study Research* (Beverly Hills, CA: Sage Publications, 1984).

18. What we are saying is that the success of a theory should be measured by the accuracy with which it can predict outcomes across the entire range of situations in which managers find themselves. Consequently, we are not seeking "truth" in any absolute, Platonic sense; our standard is practicality and usefulness. If we enable managers to achieve the results they seek, then we will have been successful. Measuring the success of theories based on their usefulness is a respected tradition in the philosophy of science, articulated most fully in the school of logical positivism. For example, see R. Carnap, *Empiricism, Semantics and Ontology* (Chicago: University of Chicago Press, 1956); W. V. O. Quine, *Two Dogmas of Empiricism* (Cambridge, MA: Harvard University Press, 1961); and W. V. O. Quine, *Epistemology Naturalized.* (New York: Columbia University Press, 1969).
19. This is a serious deficiency of much management research. Econometricians call this practice "sampling on the dependent variable." Many writers, and many who think of themselves as serious academics, are so eager to prove the worth of their theories that they studiously avoid the discovery of anomalies. In case study research, this is done by carefully selecting examples that support the theory. In more formal academic research, it is done by calling points of data that don't fit the model "outliers" and finding a justification for excluding them from the statistical analysis. Both practices seriously limit the usefulness of what is written. It actually is the discovery of phenomena that the existing theory cannot explain that enables researchers to build better theory that is built upon a better classification scheme. We need to do *anomaly-seeking* research, not anomaly-avoiding research.

    We have urged doctoral students who are seeking potentially productive research questions for their thesis research to simply ask when a "fad" theory won't work—for example, "When is process reengineering a bad idea?" Or, "Might you ever want to outsource something that *is* your core competence, and do internally something that is *not* your core competence?" Asking questions like this almost always improves the validity of the original theory. This opportunity to improve our understanding often exists even for very well done, highly regarded pieces of research. For example, an important conclusion in Jim Collins's extraordinary book *From Good to Great* (New York: HarperBusiness, 2001) is that the executives of these successful companies weren't charismatic, flashy men and women. They were humble people who respected the opinions of others. A good opportunity to extend the validity of Collins's research is to ask a question such as, "Are there circumstances in which you actually *don't* want a humble, noncharismatic CEO?" We suspect that there are—and defining the different circumstances in which charisma and humility are virtues and vices could do a great service to boards of directors.

20. We thank Matthew Christensen of the Boston Consulting Group for suggesting this illustration from the world of aviation as a way of explaining how getting the categories right is the foundation for bringing predictability to an endeavor. Note how important it was for researchers to discover the circumstances in which the mechanisms of lift and stabilization did *not* result in successful flight. It was the very search for failures that made success consistently possible. Unfortunately, many of those engaged in management research seem anxious *not* to spotlight instances their theory did not accurately predict. They engage in anomaly-avoiding, rather than anomaly-seeking, research and as a result contribute to the perpetuation of unpredictability. Hence, we lay much responsibility for the perceived unpredictability of business building at the feet of the very people whose business it is to study and write about these problems. We may, on occasion, succumb to the same problem. We can state that in developing and refining the theories summarized in this book, we have truly sought to discover exceptions or anomalies that the theory would not have predicted; in so doing, we have improved the theories considerably. But anomalies remain. Where we are aware of these, we have tried to note them in the text or notes of this book. If any of our readers are familiar with anomalies that these theories cannot yet explain, we invite them to teach us about them, so that together we can work to improve the predictability of business building further.
21. In studies of how companies deal with technological change, for example, early researchers suggested attribute-based categories such as incremental versus radical change and product versus process change. Each categorization supported a theory, based on correlation, about how entrant and established companies were likely to be affected by the change, and each represented an improvement in predictive power over earlier categorization schemes. At this stage of the process there rarely is a best-by-consensus theory, because there are so many attributes of the phenomena. Scholars of this process have broadly observed that this confusion is an important but unavoidable stage in building theory. See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). Kuhn chronicles at length the energies expended by advocates of various competing theories at this stage, prior to the advent of a paradigm.

In addition, one of the most influential handbooks for management and social science research was written by Barney G. Glaser and Anselm L. Strauss (*The Discovery of Grounded Theory: Strategies of Qualitative Research* [London: Wiedenfeld and Nicholson, 1967]). Although they name their key concept "grounded theory," the book really is about categorization, because that process is so central to the building of valid theory. Their term "substantive theory" is similar to our term "attribute-based categories." They describe how a knowledge-building community of researchers ultimately succeeds in transforming their understanding into "formal theory," which we term "circumstance-based categories."

22. Clayton M. Christensen, T*he Innovator's Dilemma: When New Technologies Cause Great Firms to Fail* (Boston: Harvard Business School Press, 1997).
23. Managers need to know if a theory applies in their situation, if they are to trust it. A very useful book on this topic is Robert K. Yin's *Case Study Research: Design and Methods* (Beverly Hills, CA: Sage Publications, 1984). Building on Yin's concept, we would say that the breadth of applicability of a theory, which Yin calls its *external validity,* is established by the soundness of its categorization scheme. There is no other way to gauge where theory applies and where it does not. To see why, consider the disruptive innovation model that emerged from the study of the disk drive industry in the early chapters of *The Innovator's Dilemma*. The concern that readers of the disk drive study raised, of course, was whether the theory applied to other industries as well. *The Innovator's Dilemma* tried to address these concerns by showing how the same theory that explained who succeeded and failed in disk drives also explained what happened in mechanical excavators, steel, retailing, motorcycles, accounting software, motor controls, diabetes care, and computers. The variety was chosen to establish the breadth of the theory's applicability. But this didn't put concerns to rest. Readers continued to ask whether the theory applied to chemicals, to database software, and so on.

    Applying any theory to industry after industry cannot prove its applicability because it will always leave managers wondering if there is something different about their current circumstances that renders the theory untrustworthy. A theory can confidently be employed in prediction only when the categories that define its contingencies are clear. Some academic researchers, in a well-intentioned effort not to overstep the validity of what they can defensibly claim and not claim, go to great pains to articulate the "boundary conditions" within which their findings can be trusted. This is all well and good. But unless they concern themselves with defining what the other circumstances are that lie beyond the "boundary conditions" of their own study, they circumscribe what they can contribute to a body of useful theory.

24. An illustration of how important it is to get the categories right can be seen in the fascinating juxtaposition of two recent, solidly researched books by very smart students of management and competition that make compelling cases for diametrically opposite solutions to a problem. Each team of researchers addresses the same underlying problem—the challenge of delivering persistent, profitable growth. In *Creative Destruction* (New York: Currency/Doubleday, 2001), Richard Foster and Sarah Kaplan argue that if firms hope to create wealth sustainably and at a rate comparable to the broader market, they must be willing to explore radically new business models and visit upon themselves the tumult that characterizes the capital markets. At the same time, another well-executed study, *Profit from the Core* (Boston: Harvard Business School Press, 2001), by Bain consultants Chris Zook and James Allen, drew upon the same phenomenological evidence—that only a tiny minority of companies are able to sustain above-market returns for a significant time. But *their* book encourages companies to focus on and improve their established businesses rather than attempt to anticipate or even respond to the vagaries of equity investors by seeking to create new growth in less-related markets. Whereas Foster and Kaplan motivate their findings in terms of the historical suitability of incrementalism in a context of competitive continuity and argue for more radical change in light of today's exigencies, Zook and Allen hold that focus is timeless and remains the key to success. Their prescriptions are mutually exclusive. Whose advice should we follow? At present, managers grappling with their own growth problems have no choice but to pick a camp based on the reputations of the authors and the endorsements on the dust jacket. The answer is that there is a great opportunity for circumstance-focused researchers to build on the valuable groundwork that both sets of authors have established. The question that now needs answering is: What are the circumstances in which focusing on or near the core will yield sustained profit and growth, and what are the circumstances in which broader, Fosteresque creative destruction is the approach that will succeed?