Preface

This book is about business and management, success and failure, science and storytelling. It's written to help managers think for themselves, rather than listen to the parade of management experts and consultants and celebrity CEOs, each claiming to have the next new thing. Think of it as a guide for the reflective manager, a way to separate the nuggets from the nonsense.

Of course, for those who want a book that promises to reveal the secret of success, or the formula to dominate their market, or the six steps to greatness, there are plenty to choose from. Every year, dozens of new books claim to reveal the secrets of leading companies, from General Electric and Toyota to Starbucks and Google. *Learn their secrets and apply them to your company!* Other books profile hugely successful business leaders like Michael Dell or Jack Welch or Steve Jobs or Richard Branson. *Find out what makes them great, then go do likewise!* Others tell you how to become an innovation powerhouse, or craft a failsafe strategy, or devise a boundaryless organization, or make the competition irrelevant. *Here's the way to beat your rivals!*

In fact, for all the secrets and formulas, for all the self-proclaimed thought leadership, success in business is as elusive as ever. It's probably *more* elusive than ever, with increasingly global competition and technological change moving at faster and faster rates—which might explain why we're tempted by promises of breakthroughs and secrets and quick fixes in the first place. Desperate circumstances push us to look for miracle cures.

What's going on here isn't some vast right-wing conspiracy, or left-wing conspiracy or Wall Street conspiracy or Ivy League conspiracy, for that matter. In part it's a marriage of convenience. Managers are busy people, under enormous pressure to deliver higher revenues, greater profits, and ever larger returns for shareholders. They naturally search for ready-made answers, for tidy plug-and-play solutions that might give them a leg up on their rivals. And the people who write business books—consultants and business school professors and strategy gurus—are happy to oblige. Demand stimulates supply, and supply finds a ready demand. Around and around we go.

But there's more going on than just laziness or greed. Many thoughtful people work very hard to pinpoint the reasons for company success. If they have trouble finding definitive answers, we ought to ask why. *Why* is it so hard to determine the factors that lead to high performance? *Why* is it that even clever minds that earnestly want to uncover the secrets of success don't find solid answers—even when they gather huge amounts of data about hundreds of companies over many years? Is there something about the way we ask the question, or the way we go about trying to find answers, that keeps us from getting it right?

The central idea in this book is that our thinking about business is shaped by a number of delusions. There are good precedents for investigating delusions in business and economics. Charles Mackay's 1841 classic, *Extraordinary Popular Delusions and the Madness of Crowds,* chronicled the follies of public judgment, from Dutch tulip mania to speculative bubbles and more. More recently, cognitive psychologists have identified biases that affect the way individuals make decisions under uncertainty. This book is about a different set of delusions, the ones that distort our understanding of company performance, that make it difficult to know why one company succeeds and another fails. These errors of thinking pervade much that we read about business, whether in leading magazines or scholarly journals or management bestsellers. They cloud our ability to think clearly and critically about the nature of success in business.

Is delusion too strong a word? I don't think so. A longtime friend of mine, Dick Stull, explains the difference between illusion and delusion this way. When Michael Jordan appears to hang motionless in midair for a split second while on his way to a slam-dunk, that's an *illusion.* Your eyes are playing tricks on you. But if you think *you* can lace up a pair of Nikes, grab a basketball, and be like Mike, well, that's a *delusion.* You're kidding yourself. It ain't gonna happen. The delusions I describe in this book are a bit like that—they're promises that you can achieve great success if you just do one thing or another, but they're fundamentally flawed. In fact, some of the biggest business blockbusters of recent years contain not one or two, but several delusions. For all their claims of scientific rigor, for all their lengthy descriptions of apparently solid and careful research, they operate mainly at the level of storytelling. They offer tales of inspiration that we find comforting and satisfying, but they're based on shaky thinking. They're deluded.

Mark Twain once said: "Always do right. This will gratify some people and astonish the rest." My purpose is a bit different. Rather than gratify and astonish, I hope this book will stimulate discussion and raise the level of business thinking. The point isn't to make managers smarter. The business world is full of people who are plenty smart—clever, quick of mind, and conversant in current management concepts. In short supply are managers who are wise—by which I mean discerning, reflective, and able to judge what's correct and what's wrong. I'd like this book to help managers become wiser: more discerning, more appropriately skeptical, and less vulnerable to simplistic formulas and quick-fix remedies. Why is this a worthwhile goal? I've lived in and around the business world for more than twenty-five years, first as a manager for a leading U.S. company, then as a professor at Harvard Business School, and for these past ten years as a professor at IMD in Lausanne, Switzerland. I work on a daily basis with executives from a wide variety of industries. What I've observed, over and over, is a tendency by managers and professors alike to embrace simple answers, some of them patently simpleminded and wrongheaded, and to latch on to quick solutions rather than to question and think for themselves.

But rather than tell you *what* to think, I'd rather have you think critically for yourself. You may find some parts of this book to be a bit provocative. If so, that's fine. I want you to challenge what I write rather than accept it. One of my role models here is the late Herbert Simon, father of artificial intelligence, Nobel Prize winner in economics for his work on decision making, and professor at Carnegie Mellon University from the late 1940s until his death in 2001. In his memoirs, *Models of My Life,* Simon described how his service on several foreign fact-finding missions in the 1960s, often time-consuming and very costly, led him to formulate his Travel Theorem, which goes like this:

> Anything that can be learned by a normal American adult on a trip to a foreign country (of less than one year's duration) can be learned more quickly, cheaply, and easily by visiting the San Diego Public Library.

The response? Simon wrote: "People react almost violently to my Travel Theorem. I try to explain that it has nothing to do with the pleasures of travel, but only with the efficiency of travel for learning. They don't seem to hear my explanation; they remain outraged. They point out that I seem to be traveling all the time. Why shouldn't other people travel too? After they simmer down enough to understand the theorem, they still attack it. It takes a long time to calm their passion with reason—and usually it isn't extinguished, but temporarily subdued. Why, they think, argue with a madman?"

Well, I think the Travel Theorem is wonderful—not because I agree with it, but because it makes me think. It forces me to ask: *What is the real purpose of this trip?* Is it for enjoyment or for learning? If the latter, exactly what am I trying to learn, and what's the best way to learn it? Could my time and money be better spent searching available sources rather than running off to the ends of the earth? Disagree with Simon's Travel Theorem if you wish, but that's not the point. The point is to force us to ask under what circumstances it's correct and when it's false–and that sort of critical thinking is always useful.

Most management books ask the first-order question: *What leads to high performance?* This book sets out to answer a different question: *Why is it so hard to understand high performance?* My aim is to pull back the curtains and ask the questions we don't often raise, to point out some of the delusions that keep us from seeing clearly. Much of this book, chapters 2 through 8, shows why the experts–gurus, consultants, professors, and journalists–are so often wrong. It exposes delusions that are all around us–in the business press, in academic research, and in recent bestsellers. But that takes us only so far. Once we've cleared away the delusions that permeate so much popular thinking about business, what then? The second thing a wise manager must do is focus on the elements that drive company performance while recognizing the fundamental uncertainty at the heart of the business world. The remainder of the book, chapters 9 and 10, takes up these questions, suggesting how managers might replace delusions with a more discerning way of understanding company performance, one that respects probabilities. Fortunately, there are managers on the scene today who provide good role models, and the final chapter offers a few brief portraits that can serve as examples for the rest of us.

Is there a pot of gold at the end of this rainbow? Not in the usual sense of the term. You won't find any promises of guaranteed results anywhere in these pages. There's no assurance that success follows predictably if you adopt these four rules, or live by that five-point plan, or commit yourself to those six steps. Yet I'm convinced that a clear-eyed and thoughtful approach *is* a better way to think about management–better, anyway, than the kind of casual thinking that characterizes so much of what's on business bookshelves today.

Another of the wise men whose voice appears in these pages, the physicist Richard Feynman, once remarked that many fields have a tendency for pomposity, to make things seem deep and profound. It's as if the less we know, the more we try to dress things up with complicated-sounding terms. We do this in countless fields, from sociology to philosophy to history to economics—and it's definitely the case in business. I suspect that the dreariness in so much business writing often stems from wanting to sound as though we have all the answers, and from a corresponding unwillingness to recognize the limits of what we know. Regarding a particularly self-important philosopher, Feynman observed:

> It isn't the philosophy that gets me, it's the pomposity. If they'd just *laugh* at themselves! If they'd just say, "I think it's like this, but von Leipzig thought it was like that, and he had a good shot at it, too." If they'd explain that this is their best guess.

Well, this is *my* best guess. This is the way I see it.

the center of events. When times are good, we lavish praise and create heroes. When things go bad, we lay blame and create villains. These stories offer a means of establishing right and wrong, a way of attributing moral responsibility. Of the dozens of articles about ABB, only a few tried to resist this tendency and retain a sense of perspective. In their 2002 *Fortune* article, Richard Tomlinson and Paola Hjelt wrote: "Barnevik was never as good as the rave reviews he received in the 1990s, nor was he half as bad as the recent damning press coverage might suggest. What's been missing since open season was declared on Barnevik is a sense of proportion about how much of the blame he should shoulder." A wise view, but all too rare among the scores of articles and case studies about ABB. Most writers went with the simpler story. Once widely revered, Percy Barnevik was now an exemplar of arrogance, of greed, of bad leadership. A final postscript to the saga of ABB came in late 2005, when the Office of the Prosecutor in Zurich dropped its case against Barnevik and Goran Lindahl in connection with their pension deal. Finding no fault with either manager, the prosecutor noted that the deal had been agreed in 1998, when ABB had been a very profitable company, and had been entirely proper given rules of disclosure. Yet by then the damage had been done, the perception of villainy set in stone. It was, as Barnevik said, a hell of a thing.

CHAPTER FOUR

Halos All Around Us

> The difference between a lady and a flower girl is not how she behaves, but how she's treated.
>
> George Bernard Shaw
> *Pygmalion,* 1916

During World War I, an American psychologist named Edward Thorndike was conducting research into the ways that superiors rate their subordinates. In one study, he asked army officers to rate their soldiers on a variety of features: intelligence, physique, leadership, character, and so on. He was struck by the results. Some men were thought to be "superior soldiers" and were rated highly at just about everything, while others were thought to be subpar across the board. It was as if officers figured that a soldier who was handsome and had good posture should also be able to shoot straight, polish his shoes well, and play the harmonica, too. Thorndike called it the Halo Effect.

There are a few kinds of Halo Effect. One refers to what Thorndike observed, a tendency to make inferences about specific traits on the basis of a general impression. It's difficult for most people to independently measure separate features; there's a common tendency to blend them together. The Halo Effect is a way for the mind to create and maintain a coherent and consistent picture, to reduce cognitive dissonance. Here's a recent example: In the autumn of 2001, after the September 11 attacks, George W. Bush's overall approval rating rose sharply. No surprise there, as the American public closed ranks behind its president. But the number of Americans who approved of President Bush's *handling of the economy* also rose—from 47 percent to 60 percent. Now, whether or not you like Bush's economic policies, there's no reason to believe that his handling of the economy was suddenly better in the weeks after September 11. But it's hard to keep these things separate: General approval of the president carried over to approval of a specific policy. The American public conferred a Halo on its president and made favorable attributions across the board. After all, it's uncomfortable for many people to believe that their president might be good on issues of national security but ineffective on the economy—it's far easier to think he's about the same for both. And what goes up can also come down. By October 2005, with public support for the Iraq War fading and in the wake of Hurricane Katrina's devastation, President Bush's overall approval rating sank to 37 percent, down from 41 percent in August 2005. Interestingly, Americans also gave the president lower marks on every specific question in the poll: For his economic policies, Bush had a 32 percent approval rating in October compared with 37 percent in August; regarding Iraq, 32 percent down from 38 percent; and fighting terrorism, 46 percent versus 54 percent. Asked whether President Bush had strong qualities of leadership, 45 percent of Americans said yes, compared with 54 percent in August. Each of these individual indicators moved in parallel, suggesting they were not independent but rather based on a single, overall assessment—a Halo.

This sort of Halo Effect shows up in many places. One of the companies I work with gets thousands of calls every day to its customer support center. Sometimes the problems can be solved right away, but often the service representative has to look into the matter and call back later. When the company subsequently surveyed its customers to see how satisfied they had been with the support center, customers whose problem had been solved right away rated the service representative as more knowledgeable than did customers whose problem had not been solved. That's not surprising, since it's reasonable to infer that a quick solution came from a well-informed rep. But here's what's more intriguing: 58 percent of customers whose problem had been solved right away remembered that their call had been answered "immediately" or "very quickly," while only 4 percent remembered having been kept waiting "too long." Meanwhile, of those customers whose problem had *not* been solved right away, only 36 percent remembered their call had been answered "immediately" or "very quickly," while 18 percent recalled they had waited "too long." In fact, the company had an automated answering system and there was no difference in waiting time between the two groups. Rather, an overall impression about customer service created a powerful Halo Effect that shaped perceptions about waiting time.

But the Halo Effect is not just a way to reduce cognitive dissonance. It's also a heuristic, a sort of rule of thumb that people use to make guesses about things that are hard to assess directly. We tend to grasp information that is relevant, tangible, and appears to be objective, and then make attributions about other features that are more vague or ambiguous. For example, we may not know if a new product is good, but if it comes from a well-known company with an excellent reputation, we might reasonably infer it should be of good quality. That's what brand building is about: creating Halos so that consumers are more likely to think favorably of a product or service. Or take a well-documented setting for the Halo Effect–the job interview. What's the most relevant and tangible information we first have about job candidates? Probably the school where they earned their degree, their grade-point average, and what honors they received. With this information clearly in mind–relevant, tangible, and seemingly objective–interviewers tend to shade their evaluations about other things that are less tangible, such as a candidate's personal manner or the quality of answers to general questions. A strong record from an excellent school? The job candidate often appears to be a little brighter, with smarter answers and greater potential for success. A modest record from an unheralded local school? The very same answers may sound a little less intelligent, the same appearance a bit less impressive. Which is exactly what Thorndike found in his study about army officers and their soldiers all those years ago.

Now consider companies. What's the most relevant and tangible information we often have about a company? Financial performance, of course. Whether the company is profitable. Whether sales are growing. Whether the price of its stock is on the rise. Financial performance looks to be accurate and objective. Numbers don't lie, we like to say—which is why Enron, Tyco, and a handful of other recent scandals shake our confidence so deeply. We routinely trust financial performance figures. And it's natural that on the basis of this performance data, people make attributions about other things that are less tangible and objective. All of which helps explain what we saw at Cisco and ABB. As long as Cisco was growing and profitable and setting records for its share price, managers and journalists and professors inferred that it had a wonderful ability to listen to its customers, a cohesive corporate culture, and a brilliant strategy. And when the bubble burst, observers were quick to make the opposite attribution. It all made sense. It told a coherent story. Same for ABB, where rising sales and profits led to favorable evaluations of its organization structure, its risk-taking culture, and most clearly the man at the top—and then to unfavorable evaluations when performance fell. Journalistic hyperbole? To some extent, sure. But more importantly, a natural human tendency to make attributions based on cues that we think are reliable.

Halos in the Business World

Financial information is far from the only data on which people make attributions. Barry Staw, then at the University of Illinois and later at the University of California, conducted an experiment in which groups of participants were asked to estimate a company's future sales and earnings per share based on a set of financial data. Afterward, he told some of the groups they had performed well, making accurate estimates of sales and earnings per share, and told other groups they had performed poorly—but Staw did so completely *at random.* In fact, the "high-performing groups" and the "low-performing groups" had done equally well in their financial calculations; the only difference was what Staw *told* them about their performance. Then he asked the participants to rate how well their groups had done on a range of issues. The results? When told they had performed well, people described their groups as having been highly cohesive, with better communication, more openness to change, and superior motivation. When told they had performed poorly, they recalled a lack of cohesion, poor communication, and low motivation. Staw concluded that people attribute one set of characteristics to groups they believe are effective, and a very different set of characteristics to groups they believe are ineffective. That's the Halo Effect in action.

Of course, these findings do not mean that group cohesiveness and effective communication are unimportant in group performance. It only means that you can't hope to measure cohesiveness or communication or motivation by asking people to rate themselves when they already know something about the outcome. Once people—whether outside observers or participants—believe the outcome is good, they tend to make positive attributions about the decision process; and when they believe the outcome is poor, they tend to make negative attributions. Why? Because it's hard to know in objective terms exactly what constitutes good communication or optimal cohesion or appropriate role clarity, so people tend to make attributions based on other data that they believe are reliable. Performance is a cue by which people attribute characteristics to groups and to organizations.

Some people questioned Staw's findings. They doubted whether an experiment that put strangers together for just thirty minutes could accurately capture the perceptions of work groups. A team led by H. Kirk Downey at the University of Oklahoma therefore replicated Staw's study, using the exact same set of financial problems, but with groups of people who had a prior history of working together, and giving them considerably more time to make their calculations. Again, groups were told–*at random*–that they had performed well or poorly. The results were virtually the same as in Staw's experiment. Once again, "high-performing teams" reported that their groups had been more cohesive, that teammates were of high ability and had enjoyed working together, that communication had been of a high quality, that they had been open to new ideas, and that overall they had been satisfied with the group process. All because of the randomly assigned description of performance–nothing more. Like Staw, Downey and his colleagues found a strong tendency to make attributions on the basis of performance.

Surprising? It probably shouldn't be. Picture a group where people express their views vigorously and passionately, even arguing with one another. If the group performs well, participants might reasonably look back and say that open and forthright expressions of opinion were a key reason for success. They'll say: *We were honest, we didn't hold back–and that's why we did so well! We had a good process!* But what if the group's performance turned out to be poor? Now people might recall things differently. *We argued and fought. We were dysfunctional. Next time we should follow a respectful and disciplined process.* But now imagine a group where people are calm, polite, and respectful of one another. They speak quietly and in turn. If the group does well, participants might look back and credit their courteous and cooperative nature. *We respected one another. We didn't fight. We had a good process!* But if the same group's performance was poor, people might say: *We were too polite. We censored ourselves. Next time, we should be more direct and open, not so concerned about one another's feelings.* The fact is, a wide variety of behaviors can lead to good decisions. There's no precise way to engineer an "optimal" discussion process. We may try to avoid extremes, sure, but between those extremes is a wide range of behavior that might be conducive to success. And because we really don't know what makes an optimal decision process, we tend to make attributions based on other things that are relevant and seemingly objective–namely, what we're told about performance outcomes.

Halos on the People and for the People

The Halo Effect shapes many things, including the attributions we make about an organization's people. It's widely believed that companies that manage their human resources well will outperform those that don't. That was, after all, the idea behind O'Reilly and Pfeffer's book, *Hidden Value: How Great Companies Achieve Extraordinary Results with Ordinary People.* It makes good sense. A company that does an effective job of attracting people, provides them with an environment where they can be productive and creative, and motivates them to work hard for the common good, ought to do well. How could it be otherwise? But watch out for the Halo Effect. If we're not careful, any successful company can attribute its good results to its people.

Here's a memorable example. In 1983, *Fortune* published its first survey of *America's Most Admired Companies.* The winner was IBM. The following year, in 1984, IBM topped the list again. When asked to describe IBM's strengths, CEO John Opel gave credit to his company's people: "The fundamental thing is that the people who work in the company make it a good company. That's really the secret: the people. It's our good fortune to have superior people who work hard and support each other. They have adapted to our basic set of beliefs–the standard we expect of one another–and follow those standards in dealing with one another and with people outside the company. I know it sounds corny, but it's true, and there's no point in trying to analyze it much more than that." And what sorts of people did IBM look for? Opel explained: "We're a positive bunch of people, the kind who like to do creative things. I believe that like begets like. You look for people with the same qualities as other people who are building the company." Not only were IBM's people said to be great, they also guarded against feelings of self-satisfaction. Opel concluded: "If any of us in our company behave in any way that reinforces the idea of smugness of power or arrogance, then our image could be severely tarnished. The hero of today can become the bum of tomorrow."

That's the way it looked in 1984, and of course it seemed reasonable. Every day, John Opel came to work and found himself surrounded by smart, creative, hardworking people. It was only natural to think that IBM's great people were responsible for its success. But during those same years, IBM failed to see the growing commoditization of its main business lines–mainframe computer systems and minicomputers. By the end of the 1980s, IBM was slipping badly; and by 1992, it was awash in red ink. Opel's successor, John Akers, was replaced. How did observers explain IBM's poor performance? By pointing the finger at its people and company culture, of course. In *Big Blues: The Unmaking of IBM, Wall Street Journal* reporter Paul Carroll criticized the company's "button-down culture," its "rigid bureaucracy," and its "complacent executives." The same people who were praised in 1984 now were blamed for the decline of a great industrial enterprise. Had they suddenly changed their ways? Probably not. Had the CEO been blind about his people–had they been *complacent* and *rigid* all along? I don't think so. John Opel was probably entirely honest when he sensed that he was surrounded by hardworking, excellent people. And they *were* well-suited for IBM of the 1960s and 1970s. But when the industry changed and IBM missed the turn in the road, its people were on the receiving end of a very different attribution. Our evaluations depend on whether we think we're seeing a lady or a flower girl.

Halos on our Leaders

Perhaps nothing lends itself to the Halo Effect more than leadership. Good leaders are often said to have a handful of important qualities: clear vision, effective communication skills, self-confidence, personal charm, and more. Most people would agree these are elements of good leadership. But defining them is a different matter altogether, since several of these qualities tend to be in the eye of the beholder–which is affected by company performance. It's exactly what we saw at ABB. While his company was successful, people said that Percy Barnevik had a clear vision, excellent communication skills, impressive self-confidence, and great charm; and when ABB's fortunes turned, the very same man was demonized as arrogant, too controlling, and abrasive. Of course, it is possible that as ABB's fortunes fell, Barnevik became increasingly stressed and anxious, in which case causality runs in the opposite direction–from company performance to individual behavior. Yet that argument, plausible as it may be, was not advanced; no one said that Barnevik had changed.

Bill George, former CEO of Medtronic, advanced a similar list about leadership in his 2003 book, *Authentic Leadership: Rediscovering the Secrets to Creating Lasting Value.* George wrote that outstanding leaders share a handful of qualities, including steadfast courage, clear vision, personal integrity, and outstanding character. They are *authentic leaders.* Not surprisingly, all the examples came from successful companies. George also mentioned a handful of failed companies, and their leaders were always *inauthentic.* Well, you can *always* find good things to say about leaders at successful companies, and you can always find reasons to criticize leaders of failing firms. A critical reader ought to ask if any successful companies have *inauthentic leaders,* and if any unsuccessful companies are run by *authentic leaders,* because if not, it's quite possible we're just throwing around Halos. And very predictably–at least for a book written in 2003–listed among the *inauthentic leaders* was none other than Percy Barnevik. George recounted the secret pension payoff to Barnevik and Lindahl, described the resulting public outcry, and then observed: "Currently, ABB is operating at a loss, bleeding cash, and its $40 billion market [capitalization] has collapsed to $4 billion." The inference was clear: Barnevik was *inauthentic*–for which the secret pension deal was the smoking gun, the definitive proof–all of which helped explain why ABB had performed poorly. But of course, no one had suggested Barnevik was inauthentic while ABB was doing well.

George further explained that a quality of *authentic leaders* is "a burning passion for their missions" and "a laserlike focus on overcoming barriers." A prominent example? Microsoft's Bill Gates, who "believed so passionately in Microsoft's mission of unifying computing with an integrated set of software that he was willing to fight the U.S. Government with all his might to keep from being broken up." It was easy to applaud Gates's persistence in 2003, when it was clear that Microsoft would not be split apart. But just two years earlier, in 2001, things had looked very different. Microsoft had been found guilty of predatory behavior–hardly something that one normally associates with *authentic leadership*–and ordered to be broken up by Judge Thomas Penfield Jackson. Gates had been roundly criticized for stubbornly leading his company into an unnecessary and destructive confrontation with the U.S. government, something that could have been avoided with a bit of foresight and diplomacy. David Yoffie of Harvard Business School, writing in 2001, contrasted Gates's leadership style with that of Intel chief executive Andy Grove, whose company had also been the focus of a U.S. Department of Justice investigation but had taken a very different approach. Grove had carefully navigated Intel's position, admitting no wrongdoing but showing more cooperation with the Department of Justice and avoiding a bloody trial. Gates, meanwhile, had not given an inch, and the result was a mess. Yoffie wrote: "For years now, Microsoft has been mired in court, facing charges of predatory behavior by the U.S. Department of Justice and the attorneys general of more than a dozen states. It has seen its name and business practices dragged through the mud, its senior executives distracted and embarrassed, and its very future as a single company thrown into doubt. No matter how the litigation is ultimately decided, Microsoft will have suffered significant damage to its business and its reputation." Inviting conflict with the government is not good leadership, Yoffie observed: "Even if a company wins the verdict, it can still suffer large penalties in the form of wasted resources, distracted management, and a tarnished image. Just ask Bill Gates." And that wasn't all. A few months after Bill George applauded Gates's behavior, in early 2004, testimony in a new class-action lawsuit against Microsoft showed the company to be "combative and rude," using bullying and other "unfair tactics to compete in markets where its technology was inferior."

So which was Mr. Gates, *authentic* or foolhardy? I've been a Gates watcher for a long time now (I wrote my first case study about Bill Gates and Microsoft back in 1991, spending a week at the Redmond campus interviewing Gates, Steve Ballmer, and a dozen other Microsoft executives), and aside from a major philanthropic commitment to improving world health, he seems to have changed relatively little over the years. As Microsoft's chief executive, Gates was a highly ambitious, tough, uncompromising, and unapologetic competitor. Did that make him worthy of praise as a brilliant, visionary, and *authentic* leader? When Microsoft was doing well, that sort of description seemed justified. Was Bill Gates inflexible and obstinate, sometimes petulant, and occasionally exposing his company to needless risks? When times were tough, that sort of criticism seemed reasonable, too. The attributions we made depended on the company's fortunes.

None of this should be very surprising. A serious scholar of leadership, the late James Meindl at SUNY Buffalo concluded after a series of insightful studies that we have no satisfactory theory of effective leadership that is independent of performance. We think we know what good leadership is all about–clarity of vision, communication skills, good judgment, and more–but in fact a wide range of behaviors can be said to fit these criteria. Show me a company that delivers high performance, and I can always find something positive to say about the person in charge–about the clarity of his or her vision, about good communication skills, sound judgment, and integrity. Show me a company that has fallen on hard times, and I can always find some reason to explain why the leader failed. All of which brings to mind a 1964 Supreme Court case about free speech and pornography, in which Justice Potter Stewart memorably wrote that while he could not provide a good definition of hard-core pornography, "I know it when I see it." Since good leadership is usually difficult to identify in the absence of data about performance, it seems that leadership is even *more* difficult to recognize than is hardcore pornography—which at least Justice Stewart knew when he saw it. For all the books written about leadership, most people don't recognize good leadership when they see it unless they also have clues about company performance from other things that can be assessed more clearly—namely, financial performance. And once they have evidence that a company is performing well, they confidently make attributions about a company's leadership, as well as its culture, its customer focus, and the quality of its people.

Halos in our Surveys

The Halo Effect shapes how individuals think about decision processes, an organization's people, and leadership—and it doesn't go away when we conduct large-scale surveys, either. Quite the contrary. If we're not careful, surveys might be little more than large collections of Halos, much as we saw regarding the assessments of President Bush. Consider *Fortune* magazine's annual ranking of the *World's Most Admired Companies,* the one mentioned earlier that named IBM as *Most Admired* in 1983 and 1984. Every year, *Fortune* asks thousands of business executives and industry analysts to evaluate hundreds of companies in eight categories: quality of management, quality of products and services, value as a long-term investment, innovativeness, soundness of financial position, ability to attract, develop, and retain talented people, responsibility to the community and environment, and wise use of corporate assets. Mix the answers together and you get the *World's Most Admired Companies* in each of these categories—as well as the overall winner. It's an impressive effort, and it produces an eye-catching cover story every year. Over the years, *Fortune* has named not just IBM, but luminaries like General Electric, Wal-Mart, and Dell—a very impressive bunch.

But when some researchers took a closer look, they found that *Fortune*'s *Most Admired* ratings were heavily influenced by a Halo Effect. The scores on the eight different factors for a given company turn out to be highly correlated–much more than should be the case given variance within each category. Furthermore, many of the scores were very much driven by the company's financial performance, just what we would expect given the salient and tangible nature of financial results. Two different studies showed that a company's financial performance explained between 42 percent and 53 percent of the variance of the overall rating. In other words, when a company posts high profits and its stock price is moving upward, the people who fill out *Fortune*'s survey tend to infer that its products and services are of a high quality, that it is innovative and well managed, that it is good at retaining people, and so forth. Cisco offers a case in point. In 1997, the same year Cisco leapt onto the cover of leading business magazines, it made its first appearance on *Fortune*'s *Most Admired* list, entering the charts at number fourteen. Then it rocketed upward, reaching number four in 1999 before topping out at number three in 2000. It's no surprise that Cisco rated high for investment value–its stock value was, after all, going stratospheric. But Cisco was rated high for lots of other things, too: quality of management, innovativeness, quality of people, and more. When the tech bubble burst and Cisco's stock fell, in 2001, Cisco's rating as an investment value quite naturally fell. But with the Halo of financial performance tarnished, *its ratings fell across the boards.* Cisco was now *less admired* for innovativeness, for people, the whole works. Its overall rating dropped to number fifteen in 2001, then twenty-two in 2002 and twenty-eight in 2003. *Fortune*'s survey isn't the only one to be undermined by the Halo Effect. Remember the *Financial Times*'s survey of *Most Respected Companies*? In 1996, when ABB was at its peak, it was rated high across the boards, for business performance, corporate strategy, and maximizing employee potential, and its leader was applauded for his strategic vision and focus. Again, the pattern is entirely consistent with the Halo Effect.

And there's more. In 1984, an organization called the Great

Places to Work Institute made a big splash with a book called *The 100 Best Companies to Work for in America.* Every year since then, it has compiled the *Best Companies to Work For* index. Based on these findings, the *International Herald Tribune* claimed that being a *Great Place to Work* leads to high performance, noting that the companies on the 1998 list had a total market return (share price plus reinvested dividends) over the next five years of 9.56 percent, compared with a return of 3.81 percent for all the companies on the S&P 500. The inference was clear: Companies that care about creating a great place to work will attract good people and help them be more productive, leading to superior performance. It all makes good sense. But how did the institute determine what's a great place to work? Simple, they asked employees. Employees were asked to rate their companies on two attributes: trust and culture. The trust index had five elements: credibility, respect, fairness, pride, and camaraderie. Credibility, in turn, was measured by responses to statements like this: *Management keeps me informed about important issues and changes. People around here are given a lot of responsibility.* High agreement meant high credibility, which meant a *Great Place to Work.* Respect was measured by asking for responses to questions like this: *Management involves people in decisions that affect their jobs or work environment. I am offered training and development to further myself professionally.* Again, high agreement meant respect, which was associated with being a *Great Place to Work.* The website also gathered comments like this one, said to be from an employee in a sample company: "There is a high level of trust & empowerment here. We are not bound by any rules & we can do whatever we want at work. We receive encouragement & motivation from our team leaders. We have company events & wellness programs which allow us to balance our personal & professional lives."

At first glance, this all looks plausible, but it's undermined by the Halo Effect. Companies that are profitable, prosperous, and growing fast will often be perceived as desirable places to work. Again, look at Cisco. It debuted on the charts in 1998 at number twenty-five, then climbed to twenty-third place in 1999. In 2000, when Cisco was briefly the most valuable company in the world, it shot up to third place, where it stayed for two years. Once the layoffs hit and the stock price tanked, how was Cisco rated as a *Great Place to Work*? It fell to fifteen in 2002, then to twenty-four, and finally twenty-eight in 2004—not exactly tracking performance, but pretty close. Did Cisco become a *worse* place to work after 2000? Yes, if we think in terms of employee morale and the chance to get rich. But that's a *reflection* of performance, not a *cause* of it. If you *don't* believe the *Fortune* and *Best Places* lists are shaded by the Halo Effect, you have to believe that the people who filled out the surveys are *not* affected by the same tendency found in participants of Barry Staw's experiment or by journalists at *Business Week, Fortune,* and other news publications, which would seem doubtful.

Delusion One: *The Halo Effect*

In chapter 1, we asked why we know so little about company performance. For all the attention devoted to the question, why is it so hard to understand why some companies succeed and others fail? In fact, our thinking about business is shaped by a number of delusions, the first of which is the Halo Effect. So many of the things that we—managers, journalists, professors, and consultants—commonly think *contribute* to company performance are often attributions *based on* performance. And even when we try to gather data in large-scale samples, like the *Fortune* survey or the *Great Place to Work* study, we often do little more than multiply the Halo Effect.

The Halo Effect isn't the only delusion that distorts our thinking about business. In the following chapters, we'll come across several more. But in many ways the Halo Effect is the most basic delusion of them all. It is a flaw—sometimes compounded by other errors—that turns up again and again, weakening the quality of our data and often diminishing our ability to think clearly about the factors that shape company performance.

CHAPTER NINE

The Mother of All Business Questions, *Take Two*

> The job of supposed intellectuals is to combat oversimplification or reductionism and to say, well, actually, it's more complicated than that. At least that's part of the job. However, you must have noticed how often certain complexities are introduced as a means of obfuscation. Here it becomes necessary to ply with glee the celebrated razor of old Occam, dispose of unnecessary assumptions, and proclaim that, actually, things are *less* complicated than they appear.
>
> Christopher Hitchens
> *Letters to a Young Contrarian,* 2001

Exposing delusions that cloud our thinking about company performance is essential for discerning managers, but it's not entirely satisfying, because it doesn't answer the mother of all business questions: *What leads to high performance?* In fact, it may seem as if we've been going backward. We may seem to know even less than we did at the start of this book. Does *employee satisfaction* lead to high performance? Probably, but it's hard to say how much, and it turns out the reverse effect is stronger: Company performance is a more important determinant of employee satisfaction. Well then, a strong *corporate culture* leads to high performance, right? Managers should strive to build strong values that are shared by all, shouldn't they? Perhaps, but just how much culture affects performance is hard to say, and once again the reverse effect may be greater, as successful companies are usually said to have strong cultures. What about *customer focus?* Isn't it vital for companies to be close to their customers? Yes, but if we're not careful, just about any high-performing company can be said to have good customer focus, and any company with sluggish sales or falling margins can be said to have lost its way with customers. *Leadership* isn't a more satisfying explanation, either, because we can always claim that successful companies have effective leaders, who seem to be endowed with clear vision and good communication skills; and we can always say that the leader of a failing company somehow lost the plot.

But all of this begs a deeper question: If so many of the things we observe are not drivers of performance but attributions based on performance, then what brought about high performance in the first place? We may agree with George Bernard Shaw that the difference between a lady and a flower girl isn't how she acts but how she's treated, but that doesn't explain how she came to be a lady to begin with. To say she was born into a wealthy family only pushes the question back a generation—how, we should want to know, did her family become wealthy while another did not? So we have to return to the question that started it all: *What leads to high performance?*

If we believe management gurus and consultants and many business school professors, high performance can be achieved with enough care and attention to a precise set of elements—these four factors or those six steps or these eight principles. Do those things, and success is just around the corner. But to paraphrase Christopher Hitchens, all the emphasis on steps and formulas may obscure a more simple truth. It may further the fiction that a specific set of steps will lead, predictably, to success. And if you never achieve greatness, well, the problem isn't with our formula—which was, after all, the product of rigorous research, of extensive data exhaustively analyzed—but with you and your failure to follow the formula. But in fact, the truth may be considerably simpler than these formulas suggest. They may divert our attention from a more powerful insight—that while we can do many things to improve our chances of success, at its core business performance retains a large measure of uncertainty. Business performance may actually be simpler than it is often made out to be, but may also be less certain and less amenable to engineering with predictable outcomes.

Here's the way I like to think about company performance. According to Michael Porter of Harvard Business School, company performance is driven by two things: *strategy* and *execution.* Strategy is about performing different activities from those of rival companies, or performing similar activities in different ways. A strategy is not a goal or an objective or a target. It's not a vision or mission or a statement of purpose. It's about being different from rivals in some important way. In turn, *execution* is all about carrying out those choices. It refers to the way that people, working together in an organizational setting, mobilize resources to deliver on the strategy. Building high-quality products, providing customer service, managing working capital, developing and deploying talent–these usually aren't matters of strategy because almost every company wants to do these things well. Rather, these things are the stuff of day-to-day management. They're all about effective operations. Explaining high performance in terms of just two things–strategy and execution–may at first raise our hopes. Just two items rather than some lengthy list! Surely managers ought to be able to get two things right! But a closer look shows that both are fraught with uncertainty, and makes plain why all the talk about blueprints and guarantees and immutable laws is a delusion.

The Risky Business of Strategic Choice . . .

All companies face a handful of basic strategic choices. In what products and markets shall we compete? What activities shall we perform–and what shall we decide to leave to suppliers or partners? How shall we position ourselves against our rivals–shall we take a premium position, or shall we be known for low cost? These are choices in the sense that a company can't hope to be all things to all customers at all times, but has to choose to compete in *this* product line and not in *that* one; to enter *this* market and stay out of *that* one; to perform *these* activities but not *those;* and to position itself relative to competitors in *this* way but not *that* one. These sorts of choices aren't bland statements of aspiration, but fundamental decisions that set a company apart from its rivals. And choosing to be different implies risk.

You wouldn't appreciate the risky nature of strategic choice if you read most business books. For example, the Evergreen Project advised companies to "devise and maintain a clearly stated, focused strategy." The exact nature of that strategy wasn't important. If a company desires to grow, the authors explained, "it doesn't matter how you achieve this growth. You can do it by organic expansion, mergers and acquisitions, or a combination of both." They went on: "Whatever your strategy, whether it is low prices or innovative products, it will work if it is sharply defined, clearly communicated, and well understood by employees, customers, partners, and investors." Which is, of course, sheer nonsense. It may be true that if we pick a group of highly successful companies, we can find some that grew by organic expansion and others by acquisition, some that offered low prices and others that emphasized innovation, but it doesn't follow that one strategy is just as good as another provided it's well-defined and clearly communicated. All we've done is grab the wrong end of the stick. In a given market setting, some choices may be foolish, even suicidal. Try, for example, pursuing capacity expansion in an industry that already has overcapacity–that's rarely a wise move, and even the best communication and sharpest definition won't help. Likewise, *Good to Great* underplayed the risky nature of strategic choice. At the outset, Collins wrote: "We expected that good-to-great leaders would begin by setting a new vision and strategy." Instead, his team found that successful companies first assembled a team of great people, and a successful strategy followed. *Great* companies got "the right people on the bus, the wrong people off the bus, and the right people in the right seats–and then figured out where to drive it." That's the extent of attention paid to strategic choice in *Good to Great.* There's nothing about competitors or positioning or risk. Strategy isn't even a topic that's listed in the index.

Neither of these books recognized a central fact: Strategy always involves risk because we don't know for sure how our choices will turn out. There are several reasons why strategic decisions are so uncertain. A first reason has to do with customers. Will customers embrace or reject a new product or service? How much will they be willing to pay? It's hard to tell for sure. Market research is often useful, and as we said with regard to Harrah's in chapter 1, some businesses lend themselves to scientific experimentation—they offer natural laboratories where variations can be tested in carefully controlled settings. But many major initiatives, like the launch of a new product or a new business model, don't easily lend themselves to experiments. There are, in fact, legendary examples where mountains of market research didn't help at all. Sam Phillips, the legendary Sun Records producer, once cautioned: "Any time you think you know what the public's going to want, that's when you know you're looking at a damn fool when you're looking in the mirror." Market reaction is always uncertain, and smart strategists know it.

A second source of risk has to do with competitors. Even if we can accurately predict what customers will do, we still have to contend with rivals, some of whom have made equally good predictions about customers and may pursue a similar set of choices—or may leapfrog us entirely with a revolutionary product or service. Predicting a rival's moves is hardly an exact science—especially when that rival is also trying to predict our behavior. An entire branch of economics, game theory, has grown up around a simple form of competitive intelligence that involves just two players, called the Prisoner's Dilemma. Expand the game to include multiple players, each with somewhat different resources and capabilities and risk preferences, and the complexity of the game grows in exponential leaps.

A third source of risk comes from technological change. Some industries are relatively stable, with products that don't change much and customer demand that remains steady for long periods of time. If you're Kellogg's and you sell cornflakes, you might be able to crank out a steady profit year after year. People still need to eat breakfast, no one has invented a much better cornflake, and you have a well-known brand, all of which may translate into steady revenues and profits (at least until generic cornflakes and private labels erode market share and margins or until large retailers squeeze our profits–nothing is forever, as Schumpeter would tell us). But in other industries, technology changes rapidly and strategic choices can come one after another with life-and-death consequences. In his groundbreaking research, Clayton Christensen at Harvard Business School showed that in a wide range of industries, from earth-moving equipment to disk drives to steel, successful companies were repeatedly dislodged by new technologies. They didn't fail because they were badly managed–the problem was more insidious than that. Rather, it was because they kept doing everything *right*–they focused on the needs of their customers and invested in new products that had a high likelihood of success–that they became vulnerable to new technologies. These so-called disruptive technologies at first didn't look attractive to established players–they didn't meet the needs of existing customers and didn't promise substantial sales–and therefore tended to be ignored, yet they improved over time and eventually displaced the existing technology, spelling disaster for market leaders. It is, after all, very hard to know which new technologies will lead nowhere and can be safely ignored, and which will transform the industry and pose a mortal threat.

Add together these three factors–uncertain customer demand, unpredictable competitors, and changing technology–and it becomes clear why strategic choice is inherently risky. And nowhere have the risks been higher than in high-technology industries. Remember in chapter 7 when Jim Collins expressed surprise that the eleven *Great* companies came from ordinary, unspectacular industries like consumer retailing, consumer products, financial services, and steel? He offered a powerful implication: *You don't have to be a glossy high-tech or biotech company to become Great. If these run-of-the-mill companies can become Great, well, then so can you!* But I suspect a different interpretation is more accurate. These industries can be described as *dowdy,* but a better word might be *stable.* They were less subject to radical changes in technology, were less susceptible to shifts in customer demand, and may have had less intense competition. All of which meant that companies in these industries had a better chance of racking up consistent performance, year after year, than did companies in more turbulent industries. High tech companies, by contrast, were much less likely to string together fifteen years of high performance. Among the thirty-five darlings of corporate America featured in *In Search of Excellence* were several high-tech companies: computer makers Amdahl, Data General, Digital Equipment, IBM, Hewlett-Packard, and Wang Labs, plus semiconductor firms like Intel, National Semiconductor, and Texas Instruments. In the ten years after the study ended, not one of them kept pace with the overall market, as shown on table 1A in the appendix. *Not one.* It makes a wonderful story to claim that anyone can become *Great,* but the only companies that met Collins's criteria of fifteen years above the market average happened to be active in consumer products like cigarettes and razor blades and toilet paper, or consumer retailing like drugstores, or financial services like retail banks and mortgages. Collins remarked that companies can become *Great* even in unlikely places, but a fresh look at the evidence suggests that he got it backward. If the criterion is fifteen consecutive years of high performance, it may be more accurate to say that *only* companies in stable industries are likely to achieve *Greatness.*

A final source of risk comes not from all the things outside the company–customers, rivals, and technologies–but from uncertainties surrounding internal capabilities. The fact is, managers can't tell exactly how their company–with its particular people and skills and experiences–will respond to a new course of action. Strategy professors describe this with the phrase "causal ambiguity," meaning that the many and subtle interrelationships inside a company make it hard to know exactly what will be the outcome of a given set of actions. Take all of these together, and the inherent riskiness of strategic choice is clear.

What should a manager do in the face of all this uncertainty? Is it better to take a Foxlike view of the world and ceaselessly factor in a wide range of information, making adjustments and shifting plans accordingly? Or is better to pursue Hedgehoglike simplicity and focus? The latter is certainly easier to explain to employees and easier for them to follow with confidence. And, according to some, it may also be more effective. Peters and Waterman spoke about the value of *sticking to the knitting;* the Evergreen Project emphasized the importance of a clearly stated, sharply focused strategy; and Collins's eleven *Great* companies were all said to have a Hedgehog-like focus. And they're not alone. In a study that I mentioned briefly in chapter 1, Chris Zook of Bain & Co looked at 1,854 companies over ten years and found that of those companies that had achieved high performance–defined as *sustained, profitable growth*–fully 78 percent had focused on one core business. The implication: Companies that focus on their core outperform those that do not. But let's be careful. It may be true that 78 percent of high-performing companies had a single core business, but it doesn't follow that having a single core improves your chances of success, because we don't know the proportion of companies in the total population that had one core versus those that had more. We need to make sure we grab the right end of the stick. The key question is not how many successful companies have a focused profile; rather, it's whether companies with a focused profile are more likely to be successful. A change in strategy might not be the *cause* of bad performance as much as the *result,* since companies normally stick with a winning formula. A more interesting question, which so far hasn't been answered, goes like this: What should a company do when its core comes under pressure? Will it improve its chances of success by becoming more Hedgehoglike, by redoubling its focus on a narrow core? Or will it benefit from Foxlike improvisation and adaptation? That's a tougher question, but it's one that managers face in their daily jobs. It's the question that Nokia faced in chapter 1 when its handsets came under pressure from new rivals, and it's also the question Lego faced as demand for traditional toys faltered. So far, there's little conclusive research on the subject, perhaps because the question cannot be answered by taking a long sweep of time and looking at overall patterns. The question has to be studied in a different way, isolating specific moments of decision and comparing the fortunes of companies that followed different paths. As of now, we have little in the way of persuasive answers to this question.

In the meantime, we're left with the brutal fact that strategic choice is hugely consequential for a company's performance yet also inherently risky. We may look at successful companies and applaud them for what seem, in retrospect, to have been brilliant decisions, but we forget that at the time those decisions were made, they were controversial and risky. McDonald's bet on franchising looks smart today, but in the 1950s it was a leap in the dark. Dell's strategy of selling direct now seems brilliant but was attempted only after multiple failures with conventional channels. Or, recalling companies we discussed in earlier chapters, remember Cisco's decision to assemble a full range of product offerings through acquisitions or ABB's bet on leading rationalization of the European power industry through consolidation and cost cutting. The managers who took those choices appraised a wide variety of factors and decided to be different from their rivals. We remember all of these decisions because they turned out well, but success was not inevitable. As James March of Stanford and Zur Shapira of New York University explained, "Post hoc reconstruction permits history to be told in such a way that 'chance,' either in the sense of genuinely probabilistic phenomena or in the sense of unexplained variation, is minimized as an explanation." But chance *does* play a role, and the difference between a brilliant visionary and a foolish gambler is usually inferred after the fact, an attribution based on outcomes. The fact is, strategic choices always involve risk. The task of strategic leadership is to gather appropriate information and evaluate it thoughtfully, then make choices that, while risky, provide the best chances for success in a competitive industry setting.

. . . And the Uncertainties of Execution

In recent years, increasing attention has been paid to the second pillar of company performance, execution. A few prominent business leaders have trumpeted its importance. In the view of Larry

Bossidy, former head of Honeywell and before that a senior executive at General Electric, execution isn't merely one of the important elements of company performance, but ranks right at the top. "Execution," he wrote, "is the great unaddressed issue in the business world today. Its absence is the single biggest obstacle to success and the cause of the disappointments that are mistakenly attributed to other causes. No strategy can deliver results unless it's converted into specific actions–and those specific actions are the stuff of execution." That might seem like good news, because if execution is not only very important, but also involves less uncertainty than strategic choice, then perhaps managers can make predictable improvements to company performance after all. And it is true that execution isn't as risky as strategic choice, and for an obvious reason. Strategic choice depends on customer preferences and the actions of competitors and the prospects of some new technology–all of which are in the messy world outside. By contrast, execution takes place entirely within the company. It happens on our premises, with our people, working together to achieve an agreed-upon strategy that we formulated. There are fewer unknowns.

Yet execution still involves a number of uncertainties. After all, an organization isn't a system of mechanical parts, interchangeable and replaceable. It's better understood as a *sociotechnical system,* a combination of men and machines, of people and things, of hardware and software, but also of ideas and attitudes. Some technical elements can often be copied and applied with predictable results. For example, manufacturing methods, production formulas, inventory management, and computer systems can often be shared across business units with similar effect. But when we begin to examine how those technical systems interact with social systems, with people and values and attitudes and expectations, the results are harder to predict. Take, as an example, human resources management practices. Mark Huselid and Brian Becker, authors of a study we read about in chapter 5, found that human resource management systems have an important impact on company performance and concluded that managers would be well-advised to identify leading HR practices and apply them to their company. But they warned that what works at one organization, with its people and norms and traditions, may not lead to the same result in another. The way human resource policies affect performance reflects an "idiosyncratic contingency." Effective execution remains uncertain.

"Idiosyncratic contingency" is a bit like "causal ambiguity"–it's how a PhD says *"I don't know."* But it's the truth. Despite our best efforts, the ways that people and processes work together in complex organizations are very hard to untangle and even harder to transplant elsewhere with the same results. Even if we set out with the best of intentions to improve operational effectiveness, we can never predict exactly how a given set of practices will shape a company's performance. Which helps explain why the explanatory power of the study by Nick Bloom and Stephen Dorgan, reviewed in the previous chapter, was rather modest. They found that adopting certain business practices could explain about 10 percent of the variance in company performance. Why not more than 10 percent? Because those same practices will lead to somewhat different results depending on a whole host of factors: an organization's people, their skills, their expectations, and the organizational context in which those practices are used. None of this suggests that some practices aren't, on balance, better than others, nor does it mean that they won't be generally useful for most companies much of the time. It means only that execution, like strategy, doesn't lend itself to predictable cause-and-effect relationships. Our best efforts to isolate and understand the inner workings of organizations will be moderately successful at best.

Given its importance as a contributor to company performance, it's good that execution has received a growing amount of attention. Countless books and articles now talk about the need to get things done. A phrase we hear over and over is "flawless execution." One of the four elements in *What Really Works* used exactly this term: Successful companies were said to "develop and maintain flawless operational execution." A recent book titled *Flawless Execution* claimed to offer companies a way to achieve peak performance and "win their battles of the business world." *Business Week* used the same phrase to describe the challenges facing Nissan in its battle with Toyota: "Nissan's execution must be flawless." All of this ought to be a good thing–not just to recognize the importance of operational effectiveness, but to set the target high, to talk about *flawless execution.*

Yet in my experience, good intentions about execution are often undermined by a few basic errors. Let me explain. Recently I attended a presentation by a senior executive of a well-known multinational company who spoke to forty of his managers from around the world. The company is a high performer, a leader in its industry, not a troubled firm. At the beginning of his talk, as he described the challenges facing the company, the executive said emphatically: "We have the right strategy. We just need to execute better." Everyone in the room nodded in agreement, and the discussion moved on, covering a wide range of topics over the next hour. What could be wrong with that? Only this: There are dozens of dimensions of execution, and the forty people in the room may well have been thinking about forty different things. When the session ended, they had no greater understanding of the most difficult challenges facing their company, nor were they any closer to agreement on specific actions that they should take–which made it unlikely that much would change for the better. Saying, "We need to execute better," is about as helpful as saying, "Let's all do a better job." It's just motherhood and apple pie.

Rather than merely state the importance of flawless execution–after all, who could be *against* flawless execution?–managers would do better to identify those few elements of execution that are most important to deliver on the chosen strategy. For one company, it could be the reduction of manufacturing cycle time. Or lowering defect levels. For another, it could be improving speed to market for new products. Or achieving higher levels of customer retention. Or improving the rate of on-time delivery. Of course, it's tempting to say that everything is important, but that's too easy. The key is to ask: For *our* company, at *this* time, competing against *our* rivals, which of the many dimensions of execution are *most* important? Which ones are most vital for *us* at *this* time? That's a tougher question, but it's necessary if we want to develop a shared sense of priorities. And it can be done. When Larry Bossidy was CEO of AlliedSignal, he didn't just talk about the importance of execution in a general way, he focused attention on four specific dimensions: accelerating new product development, improving the order-fill rate, superior inventory management, and better working capital management. It was a short list that everyone in the company could understand and everyone could focus on.

A second challenge is our old friend, the Halo Effect. If we're not careful, *any* successful company can be said to execute well, and *any* failure can be explained, after the fact, as a failure to execute. By now, we know how to avoid Halos–we have to rely on measures that aren't shaped by performance. We have to separate inputs from outcomes. For the last decade, Dell Inc. has been a leading example of great execution. While it's natural to look at Dell's success and make an attribution of outstanding execution, a closer look makes clear that Dell rigorously measured many aspects of its operations, from the speed of its build-to-order production process, to its ability to squeeze time out of every step of the production cycle, to its superior inventory turnover (more than eighty times per year!). Dell also collected money from its customers before it paid suppliers–meaning, in accounting terms, that it had negative days of working capital, a rather remarkable achievement. Dell didn't just talk about the importance of flawless execution, it focused on a number of key elements and then measured them with precision. And the results have been, objectively measured, superb.

There's another reason I question the value of broad pronouncements about *flawless execution*–which is that it can divert attention from *strategy*. Remember when all forty managers nodded in agreement at the importance of execution? Of course they did–who could disagree? But by putting the attention on execution, the topic of strategic choice was neatly sidestepped. This happens all the time.

When my old company, Hewlett-Packard, announced disappointing results in August 2004, CEO Carly Fiorina stated, "The strategy is the right one. What we failed to do is execute the strategy." Her explanation sounded reasonable, and no one questioned her when she swiftly replaced a few key executives–it looked like an appropriate step to improve execution and raise company performance. Curiously, when Fiorina herself was fired just six months later in February 2005, a company spokesperson repeated the same line: HP was following the right strategy, but the chief executive was replaced because the board of directors wanted better execution! Again, it all sounded reasonable, and no alarms were raised about the company's basic choices. Six weeks later, when Mark Hurd was hired as the new CEO, Hewlett-Packard stuck to its message, announcing that it had "picked Mr. Hurd because of his execution skills." And therein lies the problem: It's *always* easier to bang the drum about execution than to address fundamental questions of strategy. It's always easier to insist we're going in the right direction but just need to run a little faster; it's far more painful to admit that the direction may be flawed, because the remedies are much more consequential. On closer inspection, Hewlett-Packard was beset on all sides by strategic worries. It enjoyed a strong position in printing and imaging products, but in personal computers it was locked in a losing battle with Dell; in corporate computers it was squeezed between Dell and IBM; in corporate data storage systems it trailed EMC; its information technology services lagged behind IBM, Accenture, and EDS; and in consumer electronics, HP faced a range of tough competitors from Kodak to Sony. In fact, there were plenty of reasons to question HP's strategy, but doing so raised serious issues that had far-reaching consequences. Managers quite naturally find it easier to keep the attention on execution, which everyone will always agree can be done better. And not even Dell is exempt: When it announced disappointing quarterly results in mid-2005, CEO Kevin Rollins explained away the problems as an "execution issue." In fact, there were ample reasons to question Dell's strategic choices regarding target markets and competitive positioning, but those discussions invariably have more troubling implications. It's far simpler to point the finger at execution. Which leads to an observation: Whenever someone says, "We have the right strategy, we just need to execute better," I make sure to take an extra-close look at the *strategy.*

And that brings us to the best answer I can provide to the question *What leads to high performance?* If we set aside the usual suspects of leadership and culture and focus and so on–which are perhaps better understood as attributions based on performance rather than causes of performance–we're left with two broad categories: strategic choice and execution. The former is inherently risky since it's based on our best guesses about customers, about competitors, and technology, as well as about our internal capabilities. The latter is uncertain because practices that work well in one company may not have the same effect in another. In spite of our desire for simple steps, the reality of management is much more uncertain than we would often like to admit–and much more so than our comforting stories would have us believe. Wise managers know that business is about finding ways to improve the odds of success–but never imagine that success is certain. If a company makes strategic choices that are shrewd, works hard to operate effectively, and is favored by Lady Luck, it may put some distance between itself and its rivals, at least for a time. But even those profits will tend to erode over time. Success at one moment doesn't ensure success in the next moment, because success invites new challengers, some of them willing to take greater risks than the incumbents. All of which helps explain why, seductive stories notwithstanding, there's simply no formula that can guarantee success. As Tom Peters observed: "To be excellent, you have to be consistent. When you're consistent, you're vulnerable to attack. Yes, it's a paradox. Now deal with it."

CHAPTER TEN

Managing Without Coconut Headsets

> Once you've internalized the concept that you can't prove anything in absolute terms, life becomes all the more about odds, chances, and trade-offs. In a world without provable truths, the only way to refine the probabilities that remain is through greater knowledge and understanding.
>
> Robert E. Rubin
> *In an Uncertain World: Tough Choices from Wall Street to Washington,* 2003

It's no wonder that managers, under pressure to deliver ever higher revenues and profits, are attracted to books that claim to reveal the secrets of success. Even some well-known business leaders occasionally look to bestsellers for help. In September 2005, *Business Week* wrote about Microsoft chief executive Steve Ballmer and his efforts to fend off challengers like Google and Yahoo!, to reinvigorate the company and return Microsoft to its former greatness. Among other things, Ballmer was said to have looked for inspiration to Jim Collins's *Good to Great.*

Now, Steve Ballmer is a smart fellow and I don't imagine he seriously thought that Collins's book held many answers for Microsoft. The problems facing the world's leading software company as it tries to maintain a dominant position in a highly dynamic industry, with challenges ranging from open-source software to a blizzard of online innovations with names like wikis and blogs and mash-ups, are light-years away from the problems that face midsize companies in consumer finance or retailing. But if Ballmer had tried to implement Collins's self-described immutable laws, he would have been disappointed: Since he became CEO, Microsoft's annual revenue growth fell from 36 percent to 8 percent and its stock price dropped by 40 percent. None of that should be very surprising. We know that high-performing companies usually fall back over time because of competitive forces. The core ideas in *Good to Great*–*have great people, stay focused,* and *be persistent*–are probably helpful for many companies in many circumstances, but there's no reason to believe they're sufficient to restore a mammoth software company to its glory days.

Unfortunately for Ballmer and every other manager, there's no magic formula, no way to crack the code, no genie in the bottle holding the secrets to success. The answer to the question *What really works?* is simple: *Nothing* really works, at least not all the time. That's not the nature of the business world. But that insight, however accurate, isn't of much comfort. Management is about taking action, about *doing things.* So what can be done? A first step is to set aside the delusions that color so much of our thinking about business performance. To recognize that stories of inspiration may give us comfort but have little more predictive power than a pair of coconut headsets on a tropical island. Instead, managers would do better to understand that business success is relative, not absolute, and that competitive advantage demands calculated risks. To accept that few companies achieve lasting success, and that those that do are perhaps best understood as having strung together several short-term successes rather than having consciously pursued enduring greatness. To admit that, as Tom Lester of the *Financial Times* so neatly put it, "the margin between success and failure is often very narrow, and never quite as distinct or as enduring as it appears at a distance." By extension, to recognize that good decisions don't always lead to favorable outcomes, that unfavorable outcomes are not always the result of mistakes, and therefore to resist the natural tendency to make attributions based solely on outcomes. And finally, to acknowledge that luck often plays a role in company success. Successful companies aren't "just lucky"–high performance is *not* purely random—but good fortune does play a role, and sometimes a pivotal one.

If all of this seems discouraging, it need not. The fact that business performance depends on so many things outside our control is no cause for despair. And fortunately, there are several good examples of managers who see the world clearly, accurately, without delusions. They don't write self-congratulatory accounts of their victorious careers or offer platitudes about authenticity and integrity and humility, as if those things—important though they may be—were sufficient to guarantee success. They don't cling to an idealized view of the business world. Rather, they are thoughtful managers who recognize that success comes about from a combination of shrewd judgment and hard work with a dose of good luck mixed in, and they're well aware that if the breaks of the game had gone just a bit differently, the results could have been vastly different. The executives we'll look at—Robert Rubin, Andy Grove, and Guerrino de Luca—have all been very successful, so it may seem that I'm selecting my sample based on outcomes, and to some extent that's true. We wouldn't know of them if they hadn't enjoyed some success. But I feature them here not for their successes—for the outcomes—but for the way they made decisions, for how they managed their companies, the way they made risky strategic choices with eyes wide open and then pushed for great execution. That sort of approach is an example to managers everywhere.

Robert Rubin and the Management of Probabilities

Robert Rubin is perhaps best known for his eight years in the Clinton administration, first as director of the White House National Economic Council and later as secretary of the Treasury. Before that, Rubin spent twenty-six years at the investment bank Goldman Sachs, eventually serving as co-senior partner. In his memoirs, Rubin described: "What has guided my career in both business and government is my fundamental view that nothing is provably certain. One corollary of this view is probabilistic decision making. Probabilistic thinking isn't just an intellectual construct for me, but a habit and discipline deeply rooted in my psyche." It was a view first developed in college, where he studied philosophy and learned never to take propositions at face value, but to approach what he read and heard with a skeptical mind. Rubin's thinking was further honed on Wall Street, where he saw there were no sure things, no formulas for success. Rather, "success came by evaluating all the information available to try to judge the odds of various outcomes and the possible gains and losses associated with each. My life on Wall Street was based on probabilistic decisions I made on a daily basis."

Many of Rubin's years at Goldman Sachs were spent in the field of risk arbitrage, which involves buying securities that are subject to a major event–a merger, for example, or a divestiture or a bankruptcy. It was highly complicated but also, if done well, highly profitable. Make the right bet and you come out far ahead. Make the wrong bet and *whoosh,* all gone. Risk arbitrage didn't lend itself to exact calculations but always involved a measure of risk. As Rubin recalled: "Flux and uncertainty made risk arbitrage quite nerve-racking for some people. But somehow or other, I was able to take it in reasonable stride. Arbitrage suited me, not only temperamentally but as a way of thinking–a kind of mental discipline. . . . Risk arbitrage sometimes involved taking large losses, but if you did your analysis properly and didn't get swept up into the psychology of the herd, you could be successful. Intermittent losses–sometimes greatly in excess of your worst-case expectations–were a part of the business."

One memorable deal was the proposed 1967 acquisition by Becton Dickinson of a rival company in the medical products industry, Univis. Under the terms of the stock-swap merger, one share of Univis would rise from its current market price of $24 1/2 to about $33. When the deal was announced, Univis shares rose partway, from $24 1/2 to $30 1/2, reflecting the market's uncertainty about whether the deal would come to pass. That was the question Rubin's department had to answer. If Goldman Sachs believed the merger would succeed, it would buy Univis at $30 1/2 and enjoy the further rise to $33; but if it expected the deal to fall through, it might sell Univis short. After much calculation, Goldman Sachs bought shares in Univis. It stood to gain $125,000 if the merger went through, a tidy chunk of change in 1967. But some weeks later, a disappointing earnings report at Univis caused Becton Dickinson to withdraw its bid, and Goldman Sachs ended up losing $675,000–more than five times what it had hoped to gain. Naturally there was a fair amount of second-guessing and finger-pointing along the corridors of Goldman Sachs, a normal reaction since many people infer that a bad outcome is the result of a bad decision. But even though the result turned out badly, Rubin knew the decision hadn't necessarily been wrong. He explained: "Even a large and painful loss doesn't mean that we had misjudged anything. As with any actuarial business, the essence of arbitrage is that if you calculate the odds correctly, you will make money on the majority of deals and on the sum total of all your deals. If you take a six-to-one risk, you will lose money every seventh time. . . . To an outsider, our business might have looked like gambling. In fact, it was the opposite of gambling, or at least of most amateur gambling. It was an investment business built on careful analysis, disciplined judgments–often made under considerable pressure–and the law of averages." As a veteran of deal making at Goldman Sachs, Robert Rubin knew that roughly one deal out of seven was likely to go bad. He and his colleagues tried to improve their success rate, sure, but they knew from experience that one loss out of seven was likely–and acceptable. (If the loss rate went much lower, it might signal that Goldman was not taking enough risks–which would also be a serious problem. The optimal rate of failure wasn't zero, any more than the optimal number of defaults on banks loans is zero. Just make sure that one loss doesn't break the bank!) This view of the world is based on an appreciation of probability, not a search for certainty.

If even a large and painful loss doesn't necessarily mean a bad decision, then what does? To answer that question, we have to get beyond the Halo Effect. We have to take a close look at the decision process itself, setting aside the eventual outcome. Had the right information been gathered, or had some important data been overlooked? Were the assumptions reasonable, or had they been flawed? Were calculations accurate, or had there been errors? Had the full set of eventualities been identified and their impact estimated? Had Goldman Sachs's overall risk portfolio been properly considered? This sort of rigorous analysis, with outcomes separated from inputs, isn't natural to many people. It requires an extra mental step, judging actions on their merits rather than simply making ex post facto attributions, be they favorable or unfavorable. It may not be an easy task, but it's essential. Only with such a sober assessment could Goldman Sachs hope to learn from this episode and do better next time. For Robert Rubin, this sort of thinking was natural. His view of the world was based on probabilities and uncertainty. He wrote: "Some people I've encountered in life seem more certain about everything than I am about anything. That kind of certainty isn't just a personality trait I lack. It's an attitude that seems to me to misunderstand the very nature of reality–its complexity and ambiguity–and thereby to provide a rather poor basis for working through decisions in a way that is likely to lead to the best results."

Rubin's attitude, which showed a respect for complexity and ambiguity, coupling humility in good times with an insistence on learning from bad times, served him well not only at Goldman Sachs but also in government service, where he faced decisions that were just as uncertain and for which there were no easy formulas. On his first day as secretary of the Treasury–the very day in 1995 that he was sworn in–Rubin faced a critical decision related to the Mexican peso crisis. With Mexico facing imminent default, should the United States intervene and support Mexico or not? As he had with arbitrage decisions, Rubin thought through the different options. What were the risks of intervention–what signals would it send, what precedents would it establish? What were the risks of *not* intervening—would Mexico default, and if so, with what repercussions for the United States and the global monetary system? There were no formulas to follow, just careful judgment of options and probabilities and consequences, each one with approximate odds, jotted down on a mental yellow pad, and forming the basis for an eventual judgment—not taken with any assurance of certain success, but aimed at improving the chances for success to as great a degree as possible. Based on as much sound information as it could gather, the Clinton administration provided much-needed financial support for the Mexican peso—which helped stabilize the market and begin the process of economic recovery. It was not without substantial risks and was hardly guaranteed to succeed, but it was based on the same dispassionate thinking and assessment of probabilities that had helped Robert Rubin do so well over the course of his career.

Andy Grove and the Gamble of New Technologies

Risk arbitrage at Goldman Sachs involved many transactions, none of which was large enough to break the bank—at least not if risks were managed properly. But companies also face strategic decisions where the risks are not known and where one bad outcome cannot be offset against other favorable outcomes. There are no base rates to consult and few guidelines about the potential magnitude of gains or losses. Some companies face these sorts of decisions only occasionally, but others—especially in industries where technology changes rapidly—have to confront them with regularity.

One manager who has navigated this uncertainty as well as anyone is Andy Grove of Intel. Born Andraz Graf in Hungary, he survived both Hitler and Stalin before immigrating to the United States in the late 1950s, and never lost a deeply held sense that success was never assured, that failure could strike at any time. After studying chemical engineering at the University of California, Grove went to work in the booming electronics industry. In 1968, he moved from Fairchild along with Gordon Moore and Robert Noyce to found Intel Corporation, a new semiconductor company. This was no dowdy industry. Semiconductor technology was advancing rapidly, with the capacity of chips doubling roughly every eighteen months, as Moore had famously observed.

In 1969, when Intel was just one year old and competing with tough, established players like Texas Instruments and Mostek, a major computer company called for proposals to build a new chip with a 64-bit memory. Intel was one of seven bidders, and after intense development efforts–working as if their life depended on it, Grove recalled–it came out on top. The 64-bit chip was a huge success for a young company, but there was no time for complacency. Rivals were aiming to build a chip with four times the memory, with 256 bits. Again, developing the new product was seen as a matter of life and death, and once again Intel met the challenge, coming up with the best design. And once again, rivals were busy at work, intent on developing the next generation beyond the 256-bit chip. To break clear of the pack, Intel decided to forgo the logical next step, a 512-bit device, and set its sights on leapfrogging its competitors with a chip that held 1,024 bits, four times the previous chip. That was a choice–a deliberate decision to do something very different but potentially beneficial. It was also very risky. It demanded Foxlike thinking: agility, a bit of cunning, sensing opportunities but also recognizing dangers. Recalled Andy Grove: "This required taking some big technological gambles." Once the decision was made, employees pulled together–engineers and technicians and manufacturing experts working under intense pressure to execute the strategy. And this time, Grove recalled: "We hit the jackpot. This device became a big hit." Note the words. Grove spoke about "gambles" and the need to "hit the jackpot." There was no talk about blueprints for enduring greatness or guaranteed success. Grove recognized the risks and uncertainties involved, and knew that Intel had to take bold chances that might let the company enjoy a temporary advantage, then leverage that advantage in new fields. It was a calculated gamble, but a gamble nonetheless.

The decision to press ahead with the 1,024-bit chip was one of many risky choices that Intel made over the years. As CEO, Grove continuously scanned the environment to learn of changes in technology and competitors and customers, gathering information that could be useful for Intel. He wrote: "Think of the change in your environment as a blip on the radar screen. You can't tell what the blip represents at first but you keep watching radar scan after radar scan, looking to see if the object is approaching, what its speed is and what shape it takes as it moves closer. Even if it lingers on your periphery, you still keep an eye on it because its speed and course may change." Grove's accounts of his years at Intel are full of instances where survival called for choices made under uncertainty. They weren't guaranteed to succeed–strategic bets never are. But as Robert Rubin said, you try to improve your chances of success by looking clearly and carefully at the odds, at your own capabilities, at the motives and abilities of your rivals, and make the best judgment you can, with the full knowledge that even the best decisions won't always turn out well, but that failing to take measured risks ensures that in a competitive marketplace you won't win. Yet once bets had been placed, Grove was also a believer in disciplined execution. He explained: "How can you hope to mobilize a large team of executives to pull together, accept new and different job assignments, work in an uncertain environment and work hard despite uncertainty in the future, if the leader of the company can't or won't articulate the shape of the [future]?" Now the Fox made way for the Hedgehog.

Building on its early successes, Intel became a powerhouse in the semiconductor industry during the 1970s and early 1980s, commanding a large share of the memory chip market. But by the mid-1980s, Intel was under attack again, this time from Japanese firms. Intel hadn't declined in any absolute sense but was slipping relative to its surging Japanese rivals. In 1985, Noyce and Grove made another gutsy call: to get out of memory chips entirely and shift the company into microprocessors, which were less cyclical and offered higher margins. It was hugely risky, but it paid off brilliantly. Over the next years, Intel's sales and profits soared, thanks in part to its partnership with Microsoft. Under Andy Grove's leadership, Intel maintained a dominant market share in microprocessors, releasing generation after generation of fast and powerful chips–the 286, 386, Pentium, and beyond.

Was Intel just lucky? I don't think so. Throughout these and other decisions, Grove knew that company performance is relative. It's not enough to do something well; you have to do it better than others–and that means you have to take chances. Grove's 1996 book, *Only the Paranoid Survive,* is a thoughtful primer for managers about strategic inflection points–moments of extreme risk when the company's life is at risk. He showed a keen understanding of industry dynamics, of changing technology, and of the necessity for making calculated bets. Grove had no delusions about following blueprints that claim to guarantee success. A willingness to take risks is essential–and not for the faint of heart. It's stimulated in part by a measure of fear. Grove commented:

> The quality guru W. Edwards Deming advocated stamping out fear in corporations. I have trouble with the simplemindedness of this dictum. The most important role of managers is to create an environment where people are passionately dedicated to winning in the marketplace. Fear plays a major role in creating and maintaining such passion. Fear of competition, fear of bankruptcy, fear of being wrong and fear of losing all can be powerful motivators.

Business bestsellers don't normally talk about fear. Fear has no place in the rosy world of rags to riches or in the cozy confines of *Mister Rogers' Neighborhood,* where good always triumphs and a handful of simple rules are said to lead predictably to success, regardless of what anyone else does. Fear doesn't make a comforting bedtime story. But by now, we know that this sort of simpleminded confidence is based on delusions and not likely to lead to the best results.

I'm not alone in my admiration for Andy Grove. In 2004, he was named the most influential CEO of the previous twenty-five years by the Wharton School. Jeffrey Garten, former dean of the Yale School of Management, went a step further, calling Grove "a superb model for future generations of CEOs." Why the accolades? Not just because of Intel's superb results–other companies have done as well if we rely on measures of stock performance–but because of Grove's ability to respond to change, to rebound from crises. Garten wrote that Grove's genius was to align strategy and execution even as forces of globalization were causing massive shifts in the business environment. He made calculated strategic choices, never overlooking the huge risks involved. As for execution, Grove allowed his managers room for initiative, but "he was brutal in demanding that they measure their performance every step of the way." He demanded that his managers think for themselves, not accept bromides because others say they happen to be true. More recently, Grove has been the subject of a full-length biography by Richard Tedlow of Harvard Business School, who called his subject "the best model we have for leading a business in the 21st century," not because Grove followed a blueprint for long-term success and followed it with the tenacity of a Hedgehog, but because he was vigilant about changes to the competitive landscape and adapted to new circumstances–technological, competitive, regulatory, and consumer. As Tedlow wrote: "Grove has escaped natural selection by doing the evolving himself. Forcibly adapting himself to a succession of new realities, he has left a trail of discarded assumptions in his wake."

Even as Intel prospered in the 1990s, Grove never took success for granted, never lost his Hungarian refugee's apprehension about the risk of imminent failure. He worked closely with Clayton Christensen of Harvard Business School in an effort to avoid the trauma of disruptive technologies. He also collaborated with Robert Burgelman of Stanford Business School, whose 2002 book, *Strategy Is Destiny,* detailed Intel's strategy-making process. If any company has shown the industry awareness, the management savvy, the track record of innovation, and the combination of deep talent as well as deep pockets, it would be Intel. *The New York Times* observed: "For two decades, Intel has been the most sure-footed of Silicon Valley companies." In 2005, it was the seventh largest American company in market capitalization, worth more than $180 billion, and ranked eighteenth in profits.

So for all of that, was Intel assured of continuing success? Not at all. Like every successful company, Intel struggled to find new avenues for profitable growth. A competitor, AMD, made important inroads into Intel's dominance in microprocessors, and the market for personal computers was slowing down. Intel's early efforts to expand into new markets hadn't been successful. Its entry into the digital television market had sputtered. It had also been slow to recognize a shift in the chip market away from an emphasis on speed and toward the integration of microprocessors with other technologies. In 2005, the *Financial Times* reported: "Intel's confidence, built on its dominance of the PC market, where its chips are inside four out of five machines sold, has been shaken over the past year."

What was needed to improve performance? Strategic choice, of course–which always involves risk. In 2006, new CEO Paul Otellini announced that Intel would move well beyond its traditional core of microprocessors and toward a new emphasis on chips and software, combining them into platforms aimed at a variety of fields–from the laptop to the living room to wireless applications. Along with this shift came a redesign of Intel's brand and new company tagline–Leap Ahead. It was a bold redirection, a break from the past, and to some observers seemed almost a repudiation of the course steered by Andy Grove. So how did Grove react? Did he denounce Intel's new approach as an example of *straying from the core?* Quite the contrary. At a meeting of top Intel managers, Grove signaled his strong approval, proclaiming Intel's new direction as "one of the best manifestations incorporating Intel values of risk taking, discipline, and results orientation I have ever seen here." Was success assured? Hardly–Intel's new strategic thrust was fraught with risk. But once again, Intel was forced to reinvent itself or accept declining growth and shrinking margins. Smart companies assess their options and do their best to raise the probabilities of success, but even then their fortunes are *still* uncertain, and the wisest managers, like Andy Grove, know it.

A Look at Logitech

For a last example, we'll look at a company that doesn't have a famous CEO or a thirty-five-year record of success. Logitech is one of the world's leading producers of computer interface devices–mice, keyboards, peripherals, speakers, and more. It was founded in Switzerland and is now headquartered in Fremont, California, with design and manufacturing operations in Europe, North America, and Asia. In a tough market with very intense competition, some of it coming from Microsoft–and now, perhaps Intel as well–Logitech has done very well. From 1999 through 2005, sales tripled and profits grew by even more.

How can we explain Logitech's success? It's appealing to say that Logitech has great people, and by any objective measure it probably does, but we know that it's easy to say favorable things about a company's employees as long as it's successful. We might also guess that Logitech has a great corporate culture, and if we asked Logitech employees, they would probably say that it does, but that's not an adequate explanation, either. Successful companies are usually said to have great cultures–because employees like to play on a winning team and feel confident about their future. As for customer orientation, that's an easy one, too, because Logitech's rapid sales growth must mean that customers like their products (like the cordless mouse I'm using right now). We might also claim that Logitech is successful because it has followed a focused strategy, and it's true that Logitech has focused on a fairly narrow range of products, but we can debate whether Logitech has been successful because of its focus or whether it has remained focused because of its success. And let's not forget leadership. We can always find reasons to infer that a rapidly growing and profitable company has a brilliant leader. We can always find evidence of a CEO's strong vision, ability to inspire, and personal integrity. But by now we know more. Success is relative, never absolute. Competitors imitate and advantages erode. Even good decisions sometimes turn out badly–which doesn't mean they were mistakes or blunders. The practices that work at one company won't have quite the same effect at another. So how should we explain Logitech's success?

In 2005, I attended a talk by Logitech's president and CEO, Guerrino de Luca. A native of Italy, de Luca has lengthy U.S. experience, serving for many years as a senior marketing manager for Apple Computer. I was curious: Would he try to explain Logitech's success in terms of great people and strong values and employee morale? In fact, de Luca described Logitech's success–and its challenges–explicitly in terms of strategy and execution, and did not shy away from addressing the intrinsic uncertainty of each. First he reviewed the conscious strategic choices that Logitech had made. It focused on a single segment: products that provide the interface between people and technology. Within that segment, it emphasized design, functionality, and technology. It cared about the user experience–and wanted to build products that people love to use and were proud to show friends. Logitech explicitly avoided commodity products–"The minute you think your product is a commodity, it will be," warned de Luca. And while Logitech refused to compete on price, it deliberately maintained affordable price points so that people could make buying decisions without lengthy consultation. Recognizing the speed of technological change, Logitech also aggressively replaced its own products. De Luca pointed out that Logitech had repeatedly "killed the golden goose that brought eggs more quickly than the market would have." It also refrained from pursuing new products that didn't seem to offer a chance for competitive advantage. De Luca noted: "You have to choose your playing field very carefully, and be the best there. We've said 'no' many, many times to opportunities that seemed obvious, to markets that grew fast. The reason why we said no? Either because we could not differentiate, or we felt we could not be a reasonably sized fish in those markets." This was an ever vigilant manager, scanning the competitive environment and making choices accordingly. Logitech's emphasis on innovation called for massive spending on research and product development, which de Luca described as "a fundamental choice that we make consciously." Did these choices involve risk? Yes. You read the marketplace, you study the competition, you examine trends, you look at your skills and capabilities–and you make a bet. Was Logitech, to use Andy Grove's phrase, a bit "paranoid"? Absolutely. In this industry, given the intensity of competition and rapidity of change, it had to be.

De Luca also emphasized the importance of execution. Once strategic choices were made, the focus shifted to getting things done. "We learned many times," he said, "modestly defined strategies have given dramatic success through great execution." But de Luca didn't just invoke the word "execution," he identified key elements that were most important for his company to succeed in its competitive environment. One was new product development, conducted through explicit methods and processes. Another was supply chain management, which used distribution centers in North America and Europe. Logitech also invested heavily in state-of-the-art manufacturing sites, recently opening a new facility in China. Yet for all of its attention to strategy and execution, de Luca also recognized that success is never guaranteed. There was, he said, a concerted effort at Logitech to avoid arrogance and complacency: "We force ourselves to try to avoid the syndrome of 'If it ain't broke, don't fix it.' We are constantly making changes to the way we run our business. We make changes to our organization. We make changes to our system. Resistance to change in successful companies is very high, and yet we have to drive that change." Did Logitech's CEO think he was following a blueprint for enduring success? No. But he made thoughtful decisions about strategic choices–deciding what *not* to do as much as what to do–followed up with disciplined execution based on clear priorities and explicit measures.

And Last

Will Logitech always thrive? Probably not. The brutal nature of market competition means that for Logitech, and for just about every other company, getting to the top is hard–it's a mix of shrewd strategy, superb execution, and good luck. Staying at the top is even harder, because success attracts imitators, some of which are willing to take risks that appear foolish to incumbents–but a few of which may turn out spectacularly well, even disrupting established players. Sooner or later, the forces of competition, coupled with technological change, will erode Logitech's position. And when its performance falters, whether in two years or twenty, it's inevitable that someone will say the company blundered or that the chief executive blew it. Monday morning quarterbacks will say that Logitech should have done more of this or less of that, that it erred by straying from the core, or perhaps that it failed by staying too long in its core. Decisions that turned out badly will be castigated as bad decisions. There will always be a temptation to tell a neat story that makes everything sensible and logical and that suggests why the deserving succeeded while the wicked or arrogant failed.

There will always be, as well, books that try to discover the elements that separate the best companies from the rest of the pack and that advise managers on what they might do to lead their companies to loftier heights, to join the ranks of the great, the winners, the outstanding successes. Some of these books will be good, some not. Managers will continue to read them, eager to learn any new insights, hoping to discover new things they can apply. That's not only inevitable, that's healthy.

The central idea in this book has been that our thinking about business is shaped by a number of delusions. My hope is that managers will read business books a bit more critically, free from delusions, their deepest fantasies and fondest hopes tempered by a bit of realism. I would hope in particular that managers would remember:

- If independent variables aren't measured independently, we may find ourselves standing hip-deep in Halos.

- If the data are full of Halos, it doesn't matter how much we've gathered or how sophisticated our analysis appears to be.

- Success rarely lasts as long as we'd like–for the most part, long-term success is a delusion based on selection after the fact.

- Company performance is relative, not absolute. A company can get better and fall further behind at the same time.

- It may be true that many successful companies bet on long shots, but betting on long shots does not often lead to success.

- Anyone who claims to have found *laws of business physics* either understands little about business, or little about physics, or both.

- Searching for the secrets of success reveals little about the world of business but speaks volumes about the searchers–their aspirations and their desire for certainty.

Once we've swept away these delusions, what then? When it comes to managing a company for high performance, a wise manager knows:

- Any good strategy involves risk. If you think your strategy is foolproof, the fool may well be you.

- Execution, too, is uncertain–what works in one company with one workforce may have different results elsewhere.

- Chance often plays a greater role than we think, or than successful managers usually like to admit.

- The link between inputs and outcomes is tenuous. Bad outcomes don't always mean that managers made mistakes; and good outcomes don't always mean they acted brilliantly.

- But when the die is cast, the best managers act as if chance is irrelevant–persistence and tenacity are everything.

Will all of this guarantee success? Of course not. But I suspect it will improve your chances of success, which is a more sensible goal to pursue. And you won't find yourself on the shore of a tropical island, wondering why, despite all your earnest efforts to follow the formula of success, the cargo planes still haven't landed.