

LEFT BRAIN

HOW LEADERS MAKE
WINNING DECISIONS

PHIL
ROSENZWEIG

RIGHT STUFF

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PHIL ROSENZWEIG



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For the home team

Laura, Tom, and Caroline

CONTENTS

Chapter 1: Crunch Time on a Hot August Night	1
Chapter 2: The Question of Control	23
Chapter 3: Performance, Absolute and Relative	45
Chapter 4: What It Takes to Win	64
Chapter 5: Confidence . . . and Overconfidence	80
Chapter 6: Base Rates and Breaking Barriers	101
Chapter 7: Better Decisions over Time	123
Chapter 8: Decisions of a Leader	145
Chapter 9: Where Models Fear to Tread	165
Chapter 10: When Are Winners Cursed?	191
Chapter 11: Starting Up, Stepping Out	218
Chapter 12: The Stuff of Winning Decisions	238
<i>Acknowledgments</i>	253
<i>For Further Reading</i>	257
<i>Notes</i>	263
<i>Bibliography</i>	287
<i>Index</i>	303

CRUNCH TIME ON A HOT AUGUST NIGHT

1

COMPARED WITH MANAGING, TRADING WAS ADMIRABLY DIRECT.
YOU MADE YOUR BETS AND EITHER YOU WON OR YOU LOST.

MICHAEL LEWIS, *LIAR'S POKER*, 1989

It was a billion-dollar decision, give or take a few million.

On the night of August 12, 2010, Bill Flemming, president of Skanska USA Building, faced a tough choice. Get it right, and the upside could be very lucrative. Get it wrong, and the results could be disastrous.

The story began a year earlier, when the National Security Agency (NSA) announced plans to build a computer facility for the security information it gathers around the world. The Utah Data Center (UDC) would be fully self-contained, with its own power plant and water supply, and would come equipped with anti-terrorism defenses. The site for this sprawling complex was the abandoned airfield at Camp Williams, a National Guard base in a lonesome canyon south of Salt Lake City. A forbidding location, but ideal for this purpose. It was vast, remote, and very secure.

Skanska USA Building was a division of the Swedish-based company, Skanska, and a leader in the North American construction industry. It had a strong record of large and successful projects. The company had recently completed the MetLife Stadium in New Jersey, home of the football Giants and Jets, a state-of-the-art marvel that could hold eighty-two thousand fans. It was currently at work on dozens of projects, from the renovation of the United Nations building on Manhattan's East Side to the World Trade Center Transportation Hub, the network of train and subway stations under Ground Zero.

The UDC was doubly attractive to Flemming. It was a “design/build” project, meaning that the contractor handled both design and construction. Flemming explained: “If you can come in with a better design—more efficient and smarter functionality—and if you have methods to build the facility faster, then you might beat the other bidders.”¹

But Skanska wouldn't be bidding alone. Several other top construction companies were just as eager to win the deal.

The first step was to answer the NSA's Request for Qualifications, which asked for a list of relevant experience and resources. Skanska USA Building, working with a partner, Okland Construction Company, was one of twelve bidders to submit an RFQ in February 2010. Two months later, the NSA eliminated seven and invited just five—Skanska and four others—to submit formal bids. They were given sixty days.

Over the next weeks, Flemming and his top aides worked with a team of subcontractors to prepare the bid. The NSA was explicit about the layout and capacity it wanted and also defined the technical standards. Although no price was stated, it was rumored that Congress had appropriated more than \$1 billion. For prospective

bidder the message was clear: it was more important to provide the best performance than to meet a specific price.

On June 16 Skanska USA Building submitted a bid for the Utah Data Center with a price tag of \$1.475 billion. And then it waited.

In mid-July the NSA issued its reply. All five bids had come in between \$1.4 billion and \$1.8 billion, substantially more than the government wanted to spend. Now the NSA narrowed the scope of the project, keeping the key elements but eliminating a few redundancies. Technical performance was just as important, and the schedule was unchanged. For the first time the NSA specified a target price: \$1.212 billion. Now price was a crucial element. A bid in excess of that amount would be rejected for noncompliance.

The same five companies were invited to submit new bids, with Best and Final Offers due on August 13, just six weeks away.

Now it was game on. At Skanska USA Building's head offices in Parsippany, New Jersey, a large conference room was dedicated to the UDC bid. Only approved personnel were allowed inside, their access limited by swipe cards. A team of twenty-five pored over every aspect of the project, looking for ways to lower the cost. Removing some redundancies helped, but there was still a long way to go. Everyone was focused on a single goal: how to get to *one-point-two-one-two*.

For the next six weeks, during the hottest summer on record, Flemming's team looked for ways to drive down costs.² They worked with their subcontractors to streamline the procurement process, buying in bulk or working directly with suppliers to cut out middlemen. They looked closely at the contingency for risk, a standard part of any bid. After concluding that certain inputs were unlikely to rise over the next three years, they were able to reduce the contingency. They also reviewed the provision for management fees, in effect

what they would earn. By committing to working more quickly and efficiently, they were able to lower the bid still further.

By early August Skanska USA Building's bid stood at \$1.26 billion, now tantalizingly close to the target of \$1.212 billion. Could Skanska take out even more and eliminate that \$48 million gap, producing a bid that was low enough to win but high enough to earn a profit? Or would going lower expose Skanska to serious losses?

As he pondered the way forward, Flemming considered several factors. The UDC would take three years to build, during which time it should be possible to find additional savings. The question was how much. Skanska had reason to be optimistic. In an industry known for cost overruns, Skanska USA Building had often done better than budget. It had won the MetLife Stadium contract with a bid of \$998 million, well below its nearest rival, and still found a way to earn greater profits than expected.³ The World Trade Center Transportation Hub was currently running ahead of schedule and also below cost. Surely further savings would be possible for the UDC as well. Flemming commented: "The experience that I've had over the years is typically, you can get someplace between another 3 percent and 4 percent out." Taking out 3 percent would bring Skanska from \$1.26 billion to \$1.222 billion, still short of the goal. Reaching \$1.212 billion would require lowering the bid by 3.8 percent, which was very ambitious but not impossible.

The problem was that coming in exactly at the target might not be enough. Skanska was up against four large, experienced contenders. Although none was likely to "take a flier"—the industry phrase for a lowball bid designed to win at any price—the chances were good that at least one would come in below the threshold. If Skanska did no better than meet the target, it would lose to a more aggressive rival. To win would call for going even lower.

Flemming also considered the policies of his parent company. Skanska's headquarters in Stockholm had an edict known as the Five Zeros. All construction projects were to be safe (zero accidents), ethical (zero ethical breaches), of high quality (zero defects), and green (zero environmental incidents). Above all, they had to be profitable (zero loss making). There was good reason to stress profit in the construction industry. Even at the best of times, most projects earned only small margins, so that one loser could wipe out the profits of several good ones. Losing money on a large project was unacceptable, and Flemming knew it.

Yet playing it safe and missing out on a large, high-profile project was also unthinkable. As president, Flemming was concerned about questions of reputation. What would the joint venture partner think if, after months of hard work, the project fell through because Skanska had been unwilling to make a big bet? Would it ever work with Skanska again? As for rival companies, would they conclude that Skanska lacked nerve? And what about Flemming's staff, the people he worked with day in and day out? If he wasn't willing to go the extra mile, would they see him as prudent and wise, or would they wonder if he was overly cautious and risk averse? As for the parent company, it wanted to avoid losses, but also wanted to win big deals. Successful managers didn't just walk away from big-ticket projects; they found ways to win contracts *and* make them succeed. Of course, never far away was the specter of failure. The worst outcome of all would be to win the bid, but lose money.

The Best and Final Offer was due at four o'clock the next afternoon at Army Corps of Engineers offices in Baltimore. As night fell, Bill Flemming agonized: "I'm staring at a number that's about \$50 million too high, and I know I could be disqualified if I don't get to the government's number. Fifty million out of \$1.26 billion isn't that much. Do we risk going in with a bid that's over the limit, and

maybe not get it? Or do we take a lower amount and trust that we can find some innovation?”

Eventually Flemming made the call: Skanska would enter a bid of \$1.2107 billion, placing the company \$1.3 million under the target. How it would earn a profit wasn't entirely clear. Flemming explained: “We'll take the chance that somewhere along the line we can become even more productive—by improving our scheduling, or finding ways to work faster, or discovering ways to reduce our costs.”

With that, the meeting broke up. The bid team worked through the night, finalizing stacks of documents and assembling binders. The next afternoon, the final bid was delivered to ACE offices on South Howard Street in Baltimore barely an hour before the deadline.

Back at Skanska the mood was positive. Flemming recalled: “We were optimistic. We had been struggling to get to the number, but we did it. We felt good.”

THINKING ABOUT DECISIONS

Not many of us will have to make a decision quite like the one that Bill Flemming faced on that hot August night. We won't have to submit a bid worth more than a billion dollars, with hundreds of jobs at stake and with the clock ticking down. But in other respects, Flemming's decision was typical of thorny decisions we face in many walks of life, not just business but also politics, sports, and the military. These decisions are complex, consequential, and laden with uncertainty. Learning to make better decisions—smarter, wiser, and more likely to lead to success—is a high priority.

In recent years a large number of books have been written about decision making. Many draw on the findings of research in

cognitive psychology. Human beings, we have learned, are not the rational creatures we read about in economics textbooks. They make predictable errors, or biases, which often undermine their decisions.

By now we're familiar with many of these errors, including the following:

- People are said to be overconfident, too sure of themselves and unrealistically optimistic about the future.
- People look for information that will confirm what they want to believe, rather than seeking information that might challenge their hopes.
- People labor under the illusion of control, imagining they have more influence over events than they really do.
- People are fooled by random events, seeing patterns where none exist.
- People are not good intuitive statisticians, preferring a coherent picture to what makes sense according to the laws of probability.
- People suffer from a hindsight bias, believing that they were right all along.

The standard advice for making better decisions is to be aware of our tendency for common biases and to find ways to avoid them. That makes sense for many kinds of decisions. But as we'll see, it's not sufficient for others, including many of great importance.

THE VIEW FROM THE 32ND FLOOR

A few months after the events described here, I met with Bill Flemming and two of his colleagues at Skanska's US corporate

offices on the 32nd floor of the Empire State Building in New York City. It was one of those stunning winter days when you can see for miles in every direction and the world seems crystal clear. Our conference room, bordering Fifth Avenue, offered a dazzling view. Straight ahead, looking east, was the broad expanse of Queens and Brooklyn. To the left the skyscrapers of midtown Manhattan were arrayed against a deep blue sky; looking right, the towers of lower Manhattan stood out in a silvery silhouette against the winter sun.

As the Skanska executives described how they had determined what to bid, I listened for evidence of common errors. Had their bid been shaped by biases? Or had they managed to steer clear of biases and make a sound bid? Neither view was quite right.

I asked about the weeks leading up to August 12, as they tried to bring a bid of \$1.475 billion down to the target of \$1.212 billion. Had they searched for evidence that would help them win the bid, an error known as the confirmation trap? They answered without hesitation. *Of course* they had looked for confirmatory evidence! Anyone can find reasons *not* to do something, they said. If you don't push yourself to go ahead, you'll never be successful. That didn't mean they had disregarded potential problems. But in this sort of competition, finding ways to lower the bid was essential.

I also looked for signs of overconfidence. The final bid was based on finding another \$50 million in savings; had they committed to more than could be justified? *Definitely*, they agreed. But they didn't see that as excessive confidence at all. Betting that they could find further improvements was entirely reasonable. Moreover, in the context of competitive bidding, it was necessary. Anyone *not* willing to bet they could find further savings would have

no chance. What seemed excessive by one definition was essential by another.

I also inquired about something called the winner's curse. Were they aware that in a competitive bid, the winner often goes too far and ends up a loser? Yes, they said. As industry veterans, each one was acutely aware of the danger of runaway bids. The last thing they wanted to do was make a foolish bid. Yet they also knew that if they were too cautious, they would have no chance of winning. Worry too much about the winner's curse, and you'll have a different kind of problem: you will *never* win.

As I listened to the Skanska executives, the standard lessons of decision research—to be alert to common errors and to guard against them—didn't do justice to the complexities they faced.

DECISIONS IN LABORATORIES AND IN THE REAL WORLD

Over the past several decades we have learned a great deal about decision making, thanks in large part to finely crafted experiments. One important stream of research looked at the way people make choices under conditions of risk. An experiment asked: "Would you rather have \$90 for sure, or a gamble that provided a 90 percent chance of getting \$100 and a 10 percent chance of getting nothing?" From a strict mathematical perspective, the two options are identical. Both have what economists call an *expected value* of \$90. But people do not see them as the same. Most would much rather have \$90 for sure than run a 10 percent risk of winding up with nothing at all. (Many of us would even prefer \$80 for sure rather than take the gamble offered, even though on average it would give one \$90.)

A separate line of research set aside the question of risk and examined the way our choices can be affected by how options are

framed. Recently I have used an example in my executive education class based on an experiment devised by Amos Tversky and Itamar Simonson, which goes like this: imagine you're shopping for a camera and are offered two options. Which would you prefer?⁴

- Minolta S1 priced at \$269.99, rated 6 out of 10 by *Consumer Reports*
- Minolta S2 priced at \$539.99, rated 8 out of 10 by *Consumer Reports*

When I asked this question of a group of managers not long ago, most picked the S1. A large majority said they would rather spend less for a good camera, whereas only a minority said they'd be willing to spend twice as much for the better camera. In parallel, a different set of managers was offered a choice that included these two options, but added a third:

- Minolta S1 priced at \$269.99, rated 6 out of 10 by *Consumer Reports*
- Minolta S2 priced at \$539.99, rated 8 out of 10 by *Consumer Reports*
- Minolta S3 priced at \$839.99, rated 7 out of 10 by *Consumer Reports*

Faced with this choice, very few selected the S3, but the preference for the other two was reversed. Now a large majority preferred the S2, and fewer went for the S1.⁵ Merely by adding the very expensive S3, the S2 appeared to be the moderate middle option and offered the best combination of price and quality. According to economic theory, such a shift seems irrational. If customers prefer the S1 to the S2, they shouldn't switch to the S2 just because a

third option is added. But that's what happens. How decisions are framed can shape our choices.

Crucially, these experiments are designed so you can choose the option you want, but you can't alter the options. In the first example, you can either take the \$90 or accept a gamble, but you can't change the terms. You can't improve the odds of winning \$100 to something better than 90 percent, nor can you boost the amount you stand to win from \$100 to, say, \$120. In the second example, you can pick one camera or another, but you can't alter the choices. You can't do anything to make the cameras better, perhaps by adding features, nor can you bargain the prices down, nor can you ask to see a Canon or a Nikon. You respond to the choices presented, period.

There's a good reason that experiments about choice are designed this way. If you *could* alter the options, it would be much more difficult to compare answers. We would wind up with many different answers to a wide range of options, rather than a neatly comparable set of data. The way to learn about choice is to present people with a limited number of options, then compare their responses.

A separate line of research examined the way people make judgments under uncertainty. A well-known example asks people to make guesses about such things as the length of the Nile, or the year Mozart was born, or the weight of a Boeing 747. Asked to provide a range that they are 90 percent certain contains the correct answer, people routinely provide ranges that are far too narrow, leading to the conclusion that they are overconfident.

Here again, people are asked to make judgments about things they cannot influence. Your guess may be accurate or far off, but either way you won't change the length of the Nile, or the year Mozart was born, or the weight of a Boeing 747. Again, there's a good reason that

experiments about judgment ask about things we cannot influence. If we *could* influence what we evaluate, responses would reflect our different abilities, or at least our perceptions of those abilities. Ask two people to make a judgment about a simple task—how far they can throw a ball, for example—and their replies will differ based on their abilities. The best way to learn about judgment is precisely the approach researchers have followed: make sure everyone has the same ability to influence outcomes—that is, none at all.

Most experiments about judgment and choice also have a few other features. They typically ask you to make the decision that's best for you, without considering anyone else. There is no competitive dimension. You don't have to think about what someone else might do. They also usually involve decisions that are made quickly and have outcomes that are known right away. That's helpful to make sure everyone faces the exact same circumstances, so answers can be compared without worrying about intervening factors. Finally, these experiments ask participants to make their decisions alone, as individuals, not as members of a group. They don't have to worry about how they will be perceived by a subordinate, or whether their decisions today are consistent with those they made last week, or whether people will think they're bold and decisive as opposed to wishy-washy.

Thanks to carefully crafted laboratory experiments, we know a great deal about the way people make judgments and choices. As psychologist Dan Ariely explains: "For social scientists, experiments are like microscopes or strobe lights, magnifying and illuminating the complex, multiple forces that simultaneously exert their influences on us. They help us slow human behavior to a frame-by-frame narration of events, isolate individual factors, and examine them carefully and in more detail."⁶

Experiments about judgment and choice have made important contributions to many fields.⁷ In consumer behavior, we have a much better understanding of how people make purchasing decisions.⁸ For marketing managers, eager to entice buying behavior, it's immensely valuable to understand how small changes in pricing or in the presentation of options can lead customers to open their wallets. For consumers, also, it's useful to understand the forces that shape our choices, so we can see through marketing ploys and try to avoid being manipulated. These experiments typically involve individuals acting alone, making choices from fixed options, without regard to any competitive forces.

In public policy, we have learned a great deal about the way people save for retirement, how (or if) they purchase health insurance, and even how drivers respond to traffic signals on busy roads. Armed with a sharper understanding of the way people make decisions, government agencies can design services in a more cost-effective manner.⁹ Once more, these decisions involve individuals responding to options they cannot alter, without any competitive pressure.

In finance, too, we have learned a lot about the way people make investment decisions. We know they make predictable errors when managing their portfolios, often buying and selling at the wrong time. They neglect the natural tendency of regression toward the mean and succumb to the fixed cost fallacy.¹⁰ Once again, most investment decisions involve the purchase or sale of assets the performance of which cannot be affected directly. Traders place their bets and either win or lose, but can't influence the outcome. Similarly, as a private investor you can buy a share of IBM or a share of Google, but you can't improve the performance of either one after you buy it. You can't inspire a newly acquired share to rise faster, or

encourage it to outperform the market, nor will you hurt its feelings and cause it to fall if you sell. As Adam Smith (the pen name of George Goodman) wisely observed in his classic, *The Money Game*, “The stock doesn’t know you own it.”¹¹ There’s no place for optimism or wishful thinking. Furthermore, most of us manage our investments to do well, but not as part of a competition in which we try to amass more wealth than someone else.¹²

Yet for all we know about these sorts of decisions, we know less about others.

First, many decisions involve much more than choosing from options we cannot influence or evaluations of things we cannot affect. When he decided what to bid for the UDC, Bill Flemming wasn’t making a choice from options he could not alter. If Skanska USA Building won the contract, Flemming and his team would spend the next few years carrying out the project. By applying their skill and energy, by communicating goals and mobilizing employees, they would be able to influence outcomes—maybe by a little, maybe a lot.

Second, many decisions have a competitive dimension. We not only seek to do well, but to do better than our rivals. Flemming didn’t just have to meet the government’s price of \$1.212 billion; he had to come in below the others. For that, he had to size up his rivals and consider what they might bid. That’s the essence of strategy: to outperform rivals, who are trying to do better than us.

Third, many decisions take a long time before we know the results. Large construction projects like the UDC take place over years, meaning that feedback is slow and imperfect. They’re nothing like decisions in which results are known right away and feedback from one can be used to make adjustments to the next.

Fourth, many decisions are made by leaders of organizations. As president of Skanska USA Building, Flemming had a range of roles and responsibilities. He had to consider Skanska’s relationship with

its partners and its reputation in the industry, as well as what his colleagues would think of him. Matters of perception and credibility were important.

In sum, experiments have been very effective to isolate the processes of judgment and choice, but we should be careful when applying their findings to very different circumstances. As psychologist Philip Tetlock puts it, “Much mischief can be wrought by transplanting this hypothesis-testing logic, which flourishes in controlled lab settings, into the hurly-burly of real-world settings where *ceteris paribus* never is, and never can be, satisfied.”¹³ We have learned a lot about decisions in many fields—consumer choice, public policy, and financial investments—but much less about complex decisions in the real world.

THE KEY TO GREAT DECISIONS: LEFT BRAIN, RIGHT STUFF

In *Thinking, Fast and Slow*, psychologist and recipient of the 2002 Nobel Prize in economics, Daniel Kahneman, describes two systems of thought. Our intuitive mind follows the very rapid System 1, which is often effective but frequently leads to common errors. Our reflective mind uses the slower but more deliberate System 2. Kahneman advises: “The way to block errors that originate in System 1 is simple in principle: recognize the signs that you are in a cognitive minefield, slow down, and ask for reinforcement from System 2.”¹⁴

That’s good advice, provided we have educated our System 2 to provide the right kinds of reinforcement. The aim of this book is to describe what some of those reinforcements might look like. Its goal is to identify specific ways we should think about real-world decisions—not the sorts of judgments and choices commonly studied in the lab, but more complex decisions we often encounter in the real world.

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