Master Smart Strategic Experiments

Don't Abandon Innovation in Tough Times

Excerpted from

The Silver Lining:
An Innovation Playbook for Uncertain Times

By

Scott D. Anthony

Buy the book:
Amazon
Barnes & Noble
HarvardBusiness.org

Harvard Business Press
Boston, Massachusetts

ISBN-13: 978-1-4221-3328-6
3319BC

This chapter was originally published as chapter 5 of *The Silver Lining: An Innovation Playbook for Uncertain Times*,
copyright 2009 Innosight LLC.

5

Master Smart Strategic Experiments

In 2004, Procter & Gamble almost shut down a potentially game-changing project due to a low market forecast. The team found creative, affordable ways to test critical assumptions, and the project lived on. Companies need to find ways to remove the risk from innovation by smartly running strategic experiments.

The popular perception that innovation is risky and expensive presents challenges in uncertain economic times. That perception makes it far too easy for naysayers to justify curtailing innovation investments. After all, most of that investment isn't going to pay off anyway, right?

The constant change brought about by the Great Disruption requires companies to develop ideas more quickly and

cheaply, and with lower risk. This chapter describes how the proper management of strategic experiments can help companies achieve these objectives.

The Nature of the Challenge

Many companies are very good at managing *technological* assumptions. Companies use modeling and simulation techniques, run careful experiments in laboratories, build small-scale production lines, and create physical prototypes to determine whether they can make a technology work. This carefully staged approach allows them to ensure they don't waste millions of dollars trying to commercialize a technology that just won't work.

Amazingly, companies that carefully manage these technological assumptions will fling products into the market without paying similar attention to *strategic* assumptions. And those strategic assumptions are just as important to long-term success. Is there a market need? Is the solution good enough to lead to trial? Will the customer be happy enough to repurchase, if that's required? Can you reach the customer? Will an organization's bureaucracy bog down an idea?

The principles of good experimentation are just as critical, if not more so, for strategic innovation efforts as they are for technological issues. It is very rare for an innovator to have a perfect idea from the get-go. Early discipline expedites learning that leads to critical course corrections. Conversely, companies often run into trouble when they invest tens or hundreds of millions of dollars only to learn that the strategy that looked perfect on paper flops in the market.

It is not as though companies don't *try* to confirm an idea's potential before full-scale commercialization. It's just that most techniques don't provide sufficient insight, particularly for innovations designed to create completely new markets.

For example, one way to estimate the size of a market is to gather historical data. As noted in chapter 2, the past data companies turn to suggests what *has* happened, as opposed to what *could* happen. Further, if a company has a legitimately innovative idea, what data does the researcher gather? A great example comes from IBM's exploration into the nonexistent photocopier market. In the 1950s, the company hired Arthur D. Little (ADL) to help it decide whether to purchase Xerox's patents. ADL dutifully measured the market for carbon paper, dittograph, and hectograph, and determined that capturing even 100 percent of the market at the time wouldn't justify IBM's potential investment. Of course, Xerox went on to create a multibillion-dollar market.[1] There's a reason people say markets that don't exist can't be measured and analyzed.

Another popular technique is to run concept tests. That is, show people a brief description of an idea and ask them whether they would like to purchase it. The results of these tests can feed into models that draw on historical analogues to determine an idea's potential.

This approach carries three risks. First, a consumer who *should* like an idea because it would help him or her solve an important problem might dislike a *particular* concept for myriad reasons. Maybe the concept description had words the consumer didn't like. Or the color in a picture wasn't quite right. Or the consumer simply couldn't imagine how

the product or solution would fit into his or her life. The consumer's negative reaction to a concept obscures the fact that the company is working in the right market space. Statisticians would call this kind of "false negative" a Type II error. For example, Procter & Gamble's Swiffer product showed sufficiently low scores in its concept test that the company considered killing the product, which, of course, went on to become a blockbuster success.

Second, consumers might also react positively to a concept because they find it novel, but when it comes to actual purchase, they realize that the problem the product addresses isn't important enough to part with their hard-earned cash. Statisticians would call this a Type I error; we call it a Pet Rock problem after a line of products from the mid-1970s that had six months of popularity before becoming a historical footnote.

Finally, models that draw on historical analogues rely on comparing the results of a concept test to similar products or services. This approach works well when the comparison set is clear. For example, a line extension for Tide can draw on dozens of clearly parallel examples in the laundry aisle of the supermarket. But what is the right comparison for a category-creating product?

For example, in 2004 P&G faced a critical decision about a project to introduce a probiotic solution to treat the symptoms of irritable bowel syndrome (IBS). While IBS is a common condition—estimates suggest that at least 30 million people in the United States alone suffer from IBS—many sufferers don't even know they have IBS. They just know they can't eat certain foods, have to be near a restroom when

they go to the movies, and should avoid activities where there are no toilets, such as boat rides.

P&G was on the verge of killing the idea. Despite clinical evidence that the product—now in the market under the brand name Align—worked, and despite positive response from consumers who tried the product, the projected market seemed small, especially given looming expenditures related to improving the product's shelf stability so that Align could safely reside in P&G's traditional retail sales channel.

One challenge facing the deal was determining the right comparison for Align. Was it like vitamin supplements that come in pill form? Some other supplement? Or was it Prilosec, a treatment for symptoms of heartburn that requires daily use? The lack of an obvious comparison made it difficult to accurately forecast the product's sales.

A final challenge related to drawing on historical analogues is the fact that most disruptive success stories involve course correction on the path to success. A comparison that looks appropriate in the concept phase can be 180 degrees off as the strategy shifts.

A Different Approach

Relying on past data or customer response to a proposal on paper is inadequate. Fortunately, innovators can draw on research by Howard Mintzberg, Robert Burgelman, Rita McGrath, Clayton Christensen, and others to improve their ability to manage strategic experiments.[2] Following the approach discussed next—focusing on key assumptions and

finding simple and cheap ways to test those assumptions—can also substantially improve the productivity of idea development efforts.

The first step is to pinpoint the most critical assumptions behind success.[3] The best way to identify critical assumptions is to pick up an idea and look at it from multiple perspectives. Think about what would have to go right strategically for an idea to succeed. A simple way to do this is to tell a story of what the business looks like at steady state. Start by evaluating how consumers experience the business. What are they buying? Why are they buying it? How do they find out about it? How do they pay for it? Then, talk about how the *company* experiences the business. What is it doing? How is it doing it? How does money flow? Why is leadership excited? Then look through the lens of the channel, partners, and key suppliers. What are they doing to support the business? Why are they excited to do what they are doing? Telling this kind of story can help to identify whether there is a weak link in the business, such as consumers never hearing about the idea, the company squashing the idea, or a key partner not helping to support the idea.

While each innovation is different, companies should watch for four common strategic traps that might appear in their story:

1. **No customer job-to-be-done.** The best innovations help customers solve a pressing problem. Companies can sometimes fall in love with an idea without truly understanding whether the customer is similarly in love with it.

2. **Underestimation of competitive response.** Clayton Christensen's research on disruptive innovation shows that market leaders almost always win when the battle is about bringing better products and services to existing markets (in Christensen's language, "sustaining" innovations). Powerful companies that try to edge into new markets can underestimate how fiercely the market leader will defend its turf.

3. **The sucking sound of the core.** A company's core business is a powerful magnet. It can take the most interesting idea and slowly, subtly reshape it so it resembles what the company has done in the past, rather than something truly novel. After all, you *can* fit a square peg in a round hole if you make the peg small enough. Unless managed carefully, internal stakeholders can squash or shrink potentially novel ideas.

4. **Channel misalignment.** People don't do what doesn't make sense to them. When a company asks a sales channel to prioritize something that promises to make less money than other alternatives, it should not be surprised when the channel balks.

Next, think about the financial aspect. Instead of creating a spreadsheet several tabs deep, build a "three *p*" market-sizing calculation. What is the addressable *population*? Is there any analogue product or service that might show what kind of *penetration* might be possible? What *price* points are possible? This kind of simple calculation can help identify critical

assumptions behind success (see "Financials and Disruptive Innovation" for more).

Generating assumptions shouldn't be a solitary exercise. Seek to involve as many perspectives as possible, because people with topical expertise might see a risk that appears invisible to nonexperts.

The next step is to identify the most critical assumptions. Generally speaking, a critical assumption has one or more of the following characteristics:

- Deal killers—an assumption proving false would derail the entire business. For example, if you are assuming that customers will pay for a product and they won't, the odds that you will be able to create a successful strategy are very low.
- Path dependencies—addressing a critical assumption unlocks other assumptions. Imagine you are walking down a path, and you decide to take a fork that leads to a river. Once you get to the river you find it is frozen. You could not have known that the river was frozen if you hadn't first decided to follow the fork in the path. Assumptions about channel choices can have high path dependencies because they influence other aspects of the strategy.
- Importance to investors or stakeholders who ultimately make decisions about innovations.
- Uncertain areas that have significant impact on an idea's ultimate potential.

FINANCIALS AND DISRUPTIVE INNOVATION

In an interview with Innosight in 2007, Intuit founder and chairman Scott Cook expressed his view of the utility of detailed financial forecasts for disruptive initiatives.

> We tell our disruptive teams to not do volume forecasts. Do not do a spreadsheet with volume forecasts on it, because it is unforecastable. You really cannot know. So why waste the time doing bogus numbers that are unknowable. The finance department may ask for them, so spend five minutes, do something quickly, but the leadership should not focus on those numbers. They are wrong, you just don't know in what direction. Instead we have teams focus on how deep is the customer problem that's unserved and how good is our solution at solving it. If those two are strong, then we have a reasonable shot at a good business. If either of those is weak, then no matter what the spreadsheet says, no matter what the volume forecast says, there is not a business here.

We generally agree with Cook. Trying to piece together financials can be a useful way to identify critical assumptions, or to ensure that a business passes a basic "sniff test." Using the results of assumptions layered upon assumptions to make decisions is silly.

It is vital to focus on the short list of critical areas to address at any one time. A team will have dozens, if not hundreds, of assumptions. But it is impossible to test hundreds of things simultaneously, particularly when resources are scarce. Our experience suggests that in a given time period, a team should be thinking about no more than three primary assumptions and three secondary assumptions. The discipline from identifying this short list helps to ensure proper focus in the next part of the process.

Testing Critical Assumptions

P&G chairman and CEO A. G. Lafley is a strong believer in moving beyond traditional concept tests to more transaction-oriented learning, where people in essence "vote with their wallet." In a May 2008 discussion, Lafley noted that transaction-based learning

> is really important because it's when you begin to understand who the prospect really is for this new product or service, will they purchase it and for how much, and then, once they purchase it, what's the usage cycle like, what prompts repurchase?
>
> In my world consumers cannot really tell us what they want. If we exposed something to them as a stimulus, they can say, "I like it," or "I don't like it," and they can tell you why they like it or why they don't like it, but they cannot tell you what they want. Nobody told us that they wanted Crest Whitestrips; nobody told us that they were dying for a Swiffer; nobody told

> us that Febreze would make their life better. So, consumers cannot tell us what they want . . . I've become a pretty big believer in getting that idea or technology to some relatively clear concept expression and some relatively crude prototype as fast as you possibly can, and then get that in front of prospective consumers or customers.[4]

As P&G has worked to develop a competency in disruptive innovation over the past few years, it has focused on finding ways to test key assumptions in the market. For example, in early 2008, it distributed a potentially game-changing diaper to a handful of consumers at a local amusement park. It then began selling the product over the Internet. These low-visibility efforts helped the team learn what kind of parents would seek out the product. Interacting closely with Internet purchasers and watching chatter in the blogosphere allowed the company to learn a great deal about how parents viewed the product.

Another disruptive initiative is Swash, a line of fabric-care products targeting the 30 percent of garments that are re-worn without being laundered. P&G had natural questions about whether the target consumer was a college student or a time-starved mother. So it opened a small store in Ohio and started selling Swash over the Internet. It found that the proposition appealed to both consumer groups—for different reasons, of course.

A final example is Align, the probiotic discussed earlier. In 2004, Innosight helped the Align team reframe its approach.

Instead of trying to come up with a definitive answer about Align's potential, the team identified what would have to be true to create a business about which P&G could get excited. The team zeroed in on the critical assumptions it would have to test to believe in the idea's potential. Would doctors promote Align? Would consumers who had to take a pill every day for three weeks before they saw results and keep taking the pill to stop symptoms from reappearing actually comply with the daily regimen?

P&G decided to learn about these assumptions without making the product shelf stable enough to sell in mass-market retailers. The team quietly offered the product on the Internet. It didn't invest tens of millions of dollars on advertising. Rather, its existing pharmaceutical salesforce promoted the product to doctors in three cities. Internet-based sales facilitated P&G's ability to learn whether consumers were trying, repeating, and repurchasing.

Important insights came from this process. Instead of simply having a vial with a bunch of pills in it, the Align team created a blister pack with days of the week to remind consumers to take the pill every day. Branding changed as well. Initial packaging said Align was "from the makers of Metamucil." P&G dropped Metamucil and let Align stand alone.

As the team began expanding to work with more doctors in more cities, some of P&G's traditional retail customers like Walgreens began offering the product online. In 2007, the team started using a pharmacy distribution channel, which meant any pharmacy in the United States could call a wholesaler and receive the product within twenty-four hours. It

continued to sell the product directly online. In 2008, it made the product available to traditional retailers in three cities. In early 2009, the product launched nationally. "The team stair-stepped to market, never investing ahead of learning," P&G Chief Technology Officer Bruce Brown noted. "It is very consistent with the disruptive pattern."[5]

Of course, there are other strategic experiments (the appendix highlights strategic experiments from *The Innovator's Guide to Growth*) in addition to running transaction tests. The simplest experiment involves picking up the phone and talking to industry experts, venture capitalists, or prospective customers to pick their brains about the idea.

Companies can also run focused experiments on key assumptions. For example, Turner Broadcasting had a novel advertising idea for its television networks. Inspired by the success of online and print contextual advertisement placement, a team at Turner wondered if it could introduce a similar idea on television. Imagine a scene in a television show that ends with a child covered in mud, followed immediately with an advertisement for laundry detergent. Academic research indicated that this kind of contextual linkage made an advertisement more memorable. Turner was in a unique position to commercialize this idea, as networks like TNT and TBS present hundreds of movies and popular television series, such as *Law & Order* and *Seinfeld,* next to advertising units that could be strategically placed.

Building the systems to do contextual advertising at sufficient scale looked expensive. Before making the investment, Turner wanted to make sure that its programs had enough

scenes that would be of interest to traditional television advertisers. By having interns tag a handful of programs, it quickly learned that their movies and shows did have meaningful moments of context, and that those contextual moments would appeal to leading advertisers. Turner decided to move the idea forward. Its system, called TVinContext, debuted in early 2008.

Keys to Success

Successfully managing strategic experiments requires a delicate balance. Experimentation is critical, but not without risk. Experiments can take time and can be expensive. They can perpetuate, which may lead a company to miss an opportunity while continuing to experiment (see "The Hidden Value of Fast Kills"). And they can expose an idea to the market prematurely.

The following tips can help companies maximize returns on investments in learning.

FOCUS CAREFULLY ON THE METRICS THAT DETERMINE WHETHER AN EXPERIMENT IS A SUCCESS OR FAILURE. Companies should carefully predict the results of an experiment—even if the actual answer is completely unknown and unknowable. Steven Spear's excellent book *Chasing the Rabbit* describes how the navy followed this approach in the early days of a program to create nuclear-powered submarines. The head of the project asked scientists to predict how neutron bombardments would fatigue the metal that shielded the reactor. No one really knew what the results would be. But making

THE HIDDEN VALUE OF FAST KILLS

One of the hardest decisions companies face is when to put a project on ice. After all, companies don't start an effort without a belief in an ultimate payoff. And even ideas that struggle to gain traction present compelling "what if" scenarios.

"Fast kills," where a company pulls the plug on a doomed effort early, have three advantages:

1. **Cost savings.** Commercializing projects takes money. If companies can identify flawed ideas early, they can avoid having to make investments that won't pay out.
2. **Increased throughput.** The time a customer spends waiting in a supermarket queue is a function of the number of customers, the speed of each transaction, and the number of open registers. Projects can sit in the corporate equivalent of a supermarket checkout line, with the number of project contributors, the scarcity of executive bandwidth, and so on having an impact on the throughput. Lowering the number of projects can dramatically increase the speed of the remaining projects.
3. **Improved output.** Companies that shut down bad projects early can reallocate talented managers to the remaining ideas. This kind of "doubling down" can make the good projects even better.

(Continued)

Data-minded companies can roughly quantify these benefits. For example, calculating the average investment and success rate for similar projects can provide an estimate of potential cost savings. Using a concept called "Little's Law" can help to estimate the increase in throughput by running fewer projects through a system. Determining the impact of a greater probability of realizing positive scenarios can provide a ballpark estimate of the value of focusing more deeply on fewer ideas.

When companies decide to shutter efforts early, they should make sure they capture key learning from the effort and publicly laud the no-longer-in-existence team. Not every idea is destined for greatness. Teams that learn this early have helped to advance the company's overall innovation efforts.

predictions, designing sensors to check the predictions, and carefully evaluating the results helped scientists develop a much deeper understanding about the process. This kind of approach has helped the naval program experience an astonishingly low rate of accidents during its fifty-year tenure.[6]

Spear notes that this focus on measuring, learning, and adjusting is common in "high velocity organizations" that constantly outpace their competitors. "By making abundantly clear what is expected to occur, it is much easier to be surprised by the things that happen which have not been anticipated," Spear writes. "Clear expectations don't, in

themselves, make things go right. Clear expectations simply make it obvious when things do not go as expected. So it is easier to say, 'Oh, that's not what I thought would happen. There is something about this process I don't understand and need to learn.'"[7]

Companies that use this approach to manage strategic assumptions will similarly find that careful consideration of the results of an experiment before (and after) it is run can help to expedite rapid testing and adjustment. Without this careful process, companies can miss important strategic insights.

In 2006, a newspaper company ran a six-week test of a new online advertising offering. We held a meeting to discuss the test's results. "The idea was a bomb," said the manager who oversaw the pilot. "Why?" I asked. "We only recorded $500 in revenue in six weeks!" he responded. That certainly didn't sound good, but I decided to probe a bit more deeply. "How many sales did that represent?" I asked. "Four," the manager responded. No hope yet. "How many salespeople were involved in the trial?" "Ten." Still sounds reasonable. "How many businesses did the ten salespeople approach?" The manager rifled through his notes. "Seven," he responded. Aha!

We had stumbled on an important insight. In fact, the offering connected quite closely with the target customer—four of seven customers approached by the company purchased the product, a rate that exceeded expectations. The problem was that the salesforce wasn't particularly interested in pushing the product, even after extensive training with senior leaders. Most salespeople viewed the idea as a

costly distraction. The price point was low, so commissions would be low too. Salespeople had to call on customers they didn't usually serve, which they perceived to be a low-return effort. Better understanding of the key variables behind success would have helped the company do a better job managing the experiment and increased the odds of developing a successful business.

USE THE SCARCITY PRINCIPLE TO LOWER THE COST OF EXPERIMENTATION. One of the mantras of the disruptive innovator is "patient for growth, impatient for profits." Companies that follow this approach test their critical assumptions quickly and avoid overdesigning offerings with features that aren't meaningful to end customers.

It has never been easier to test an idea quickly and cheaply. Recall the examples in chapter 1: $15,000 in 2007 for Guy Kawasaki to launch his business, and $500 by Jessica Mah in 2008 to launch an internship job board. The ability to use the Internet, low-cost specialists, and modeling and simulation tools make it easy for companies to innovate on the cheap.

There are many ways to embrace the scarcity principle. Instead of building a physical prototype, use a three-dimensional illustration. Run quick-and-dirty market research using tools such as SurveyMonkey.com. Have consumers request samples via a Web site instead of through in-store trials. Tap into prediction markets to develop rough market forecasts. Use friends, families, and coworkers as sources of inspiration for ideas.

LIMIT THE TIME FOR AN EXPERIMENT. Companies that don't make decisions rapidly can find that their innovation process slows to a crawl as experiments perpetuate. Coupling the scarcity principle with quick decision making can paradoxically make innovation faster, cheaper, and more successful.

THINK CAREFULLY ABOUT ISOLATED (FOCUSED ON ONE ELEMENT) VERSUS INTEGRATED (END-TO-END) EXPERIMENTS. Testing too many things at once can make it hard to tease out whether an experiment proved or disproved a hypothesis. On the other hand, some assumptions can be tested only using more integrated approaches such as test markets.

MONITOR YOUR PROJECT'S BURN RATE. One ten-person project team spent three months debating whether to run a $500,000 test. The fully loaded cost of the team per month was about $175,000, meaning the cost of deciding whether to run the test ($525,000) was more than the test itself. Teams within large companies often forget that even when they are not spending money, *they are spending money*. Understanding burn rates at a detailed level can be critical to help manage the strategic experimentation process. Of course, good entrepreneurs generally seek to minimize fixed costs as much as possible. Using contractors or having other flexible arrangements can help to keep costs under control (the next chapter has more about sharing innovation risks).

REMEMBER TO SAVOR SURPRISES. One company used its technological acumen to earn a commanding lead in a

category. Its competitor launched a product that intentionally traded off pure performance to provide much greater convenience to the customer. The market leader conducted research that discovered customers preferred the competitor's product. The market leader's first reaction? The survey must be wrong. There was no way the customer could prefer a lower-performing product. The company took more than two years to respond, not because of the technical sophistication of the competitor's product, but because it didn't "savor," or embrace and explore the implications of, the surprising news that it discovered.

EXPECT AND TOLERATE FAILURE. While General Motors has had its share of problems over the past few years, one shining success is its OnStar telematics business. The company's first strategy—OnStar as a completely separate business sold as a postsales add-on—was problematic. Consumers didn't understand OnStar's benefit, and dealers struggled to push the product. So GM decided to install OnStar as a standard option on many of its cars and give customers free service for a year so they could experience the system's benefit. While OnStar has a separate internal board of directors and is run fairly independently, integration with the car business turned out to be crucial. Costs are lower and margins are higher by making the technology factory-standard, and because the unit is in all GM cars, many more customers end up subscribing to the monthly service.

In 2007 as the business crossed $1 billion in revenue, CEO Rick Wagoner reflected on the lessons he learned. "With a

new business you may start out with a strategy, but after about four days you probably change it, and that has been very much the way OnStar's played out for us," Wagoner said. "It's been fascinating to see how it's developed, and it has changed how I think about opportunities in the rest of our business. You don't have to figure it out a hundred percent. If you think it is right, get on the road and adjust as you go."[8]

REMEMBER THAT PEOPLE, NOT TOOLS, MAKE DECISIONS ABOUT INNOVATION PROJECTS. Innovation still requires intuition and judgment. Making decisions based purely on the numbers can be a strategic mistake.

"If you use the spreadsheets to try to discriminate and predict which businesses will succeed and fail you'll be utterly off," Intuit founder and chairman Scott Cook says. "Because the failures had just as pretty spreadsheets as the successes."[9] Instead, Cook and his team push project teams to test ideas in the marketplace in four weeks. The short time frame forces teams to focus on critical assumptions and design "good enough" solutions.

For example, one team was working on an idea that would be a matching service between accountants who had too much work at a given time and accountants who had some extra capacity. According to Cook, "I said, 'That's a nice theory but this won't work unless you have both demand from accountants with too much work and supply from accountants who are good but for some reason are sitting on their hands.'" In three weeks, the team built a functional prototype and did a mailing to about fifty thousand accountants to test

demand. They learned that there were indeed competent accountants who had excess capacity, supporting further investment in the idea.

Summary

Often, the reason that people perceive innovation to be risky and expensive is their failure to couple technical experimentation with strategic experimentation. The right strategic experiments can help companies improve the productivity of their learning efforts. To master strategic experimentation, remember to:

- Focus on the most critical risks—potential deal killers, path-dependent elements, items that matter to stakeholders, and so on
- Design smart, cheap ways to learn about those risks, with a bias toward market-based learning
- Balance the need to experiment with some of the risks of experimentation
- Embrace the scarcity principle
- Savor surprises

APPENDIX

Selected Experiments from *The Innovator's Guide to Growth*

Internal best-practice assessment. Talk to other people in the company who have addressed similar assumptions and risks to see how their efforts panned out. Use this information to assess whether you prioritized your assumptions and risks correctly. Always be careful of assuming you can be better than the best.

Secondary market research. Focused external research helps to quickly spot developments in a market space or gives a window into the actions of competitors.

External benchmarking. Look to the external market to see how other companies addressed similar issues. If your success is predicated on doing something better than it has ever been

done, at least ask whether your assumption is reasonable. Market research or analyst reports are good sources for external information, as are consultants who specialize in an industry. Of course, remember that market research reports describe what has already been done, and experts are experts in what has been done before, not necessarily what could be done.

Business modeling or simulations. Combine your financial assumptions to see how the business model might work. Run scenarios to see what happens when assumptions change. Use this approach to find the real pivot points in your model. Also try to find assumptions that influence several others.

Competitive war games. Put yourself in your competitors' shoes and imagine what they would do in response to your approach. This exercise helps you understand how you can influence your strategy so it looks unattractive to your competitor. It also helps you develop systems that spot competitive moves early.

Patent analysis. Patents hold a wealth of information about an emerging market space. Patent activity or regulatory permit filings indicate how companies are approaching a space well before they announce official strategies.

Focus groups. Focus groups are useful ways to start conversations with customers. Be careful, however, about reading too much into a single focus group. One loud voice can dominate discussion, and it's always dangerous to draw conclusions from a sample size of six. Try to bring stimuli to the focus group to encourage expansive discussion.

Thought leader roundtable. Bringing together thought leaders in a defined space with a diverse set of perspectives helps you see things you might otherwise miss. Consider a way to have regular interactions with thought leaders, such as a standing advisory board.

Customer observations. Observing customers is a great way to identify the real innovation jobs that people are trying to do. While it takes time and can be expensive, sometimes there is no substitute for getting out in the field and watching a customer try to solve the problem you are hoping to help them solve or use the solution you are providing to them.

Concept tests. These tests involve describing a fully formulated concept to a customer to assess his or her willingness to purchase it. Concept tests should be used carefully for new-to-the-world or game-changing initiatives.

Quantitative market research. More detailed market research helps in developing market sizes, understanding how customers would trade off feature improvements, and identifying customer clusters. It is getting easier to design and execute good quantitative research using the Internet and other means.

Prototypes. No matter how much effort you expend, it is hard to get meaningful feedback for an idea described on paper. Similarly, there can be unpredictable interactions between components of a product that are invisible until you actually build the product. Physical or virtual prototypes can test those interactions, while also providing a more tangible

vehicle to garner customer feedback. Some managers think that prototyping is only relevant for companies that make physical products. Creating Web screen shots or detailed process maps are helpful ways to develop a deeper understanding of intangible offerings. A simulated conversation between a customer and a salesperson is another way to prototype a service concept.

Test markets. Some of the most important assumptions, such as pricing, relations with the channel to market, and buyer behavior, are hard to simulate or test accurately until you actually get to market. Creating a localized test market in a particular geography, or among a particular group of customers, provides critical insight into these variables. It is important to try to simulate real market conditions. In other words, it is possible to rig the test market so it apparently succeeds, but that is not in a company's long-term best interests.

NOTES

Chapter 5

1. Michael Hammer and James Champy, *Reengineering the Corporation* (New York: HarperCollins, 1993).

2. See, for example, Clayton M. Christensen and Michael Raynor, *The Innovator's Solution* (Boston: Harvard Business School Press, 2003), chapter 7; Robert A. Burgelman and Andrew S. Grove, "Let Chaos Reign, Then Rein in Chaos Repeatedly: Managing Strategic Dynamics for Corporate Longevity," *Strategic Management Journal* 28, no. 10 (2007): 965-979; Henry Mintzberg and James Waters, "Of Strategies, Deliberate and Emergent," *Strategic Management Journal* 6 (1985): 257; and Rita Gunther McGrath and Ian MacMillan, "Discovery-Driven Planning," *Harvard Business Review*, July–August 1995, 44-54.

3. For more on identifying and prioritizing assumptions, see Scott D. Anthony, Mark W. Johnson, Joseph V. Sinfield, and Elizabeth J. Altman, *The Innovator's Guide to Growth: Putting Disruptive Innovation to Work* (Boston: Harvard Business Press, 2008), chapter 7.

4. The quote was from a discussion with the author at the Front End of Innovation conference in Boston. For excerpts from the interview, see Scott D. Anthony, "The Game Changer," Forbes.com, August 28, 2008. Available at http://www.forbes.com/2008/08/28/pg-lafley-innovation-lead-clayton-in_sa_0828claytonchristensen_inl.html.

5. Scott D. Anthony and Clayton M. Christensen, "Disruption, One Step at a Time," *Forbes*, October 27, 2008 (available online at http://www.forbes.com/claytonchristensen/forbes/2008/1027/097.html).

6. Steven Spear, *Chasing the Rabbit: How Market Leaders Outdistance the Competition and How Great Companies Can Catch Up and Win* (New York: McGraw-Hill, 2008).

7. Spear, *Chasing the Rabbit*, 188.

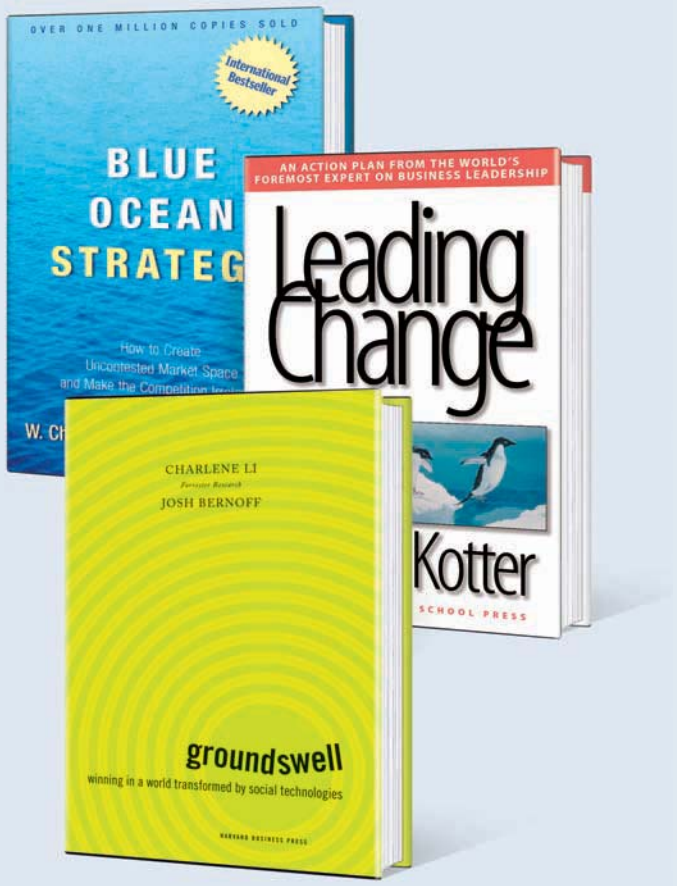
8. Anthony, et al. *The Innovator's Guide to Growth*, chapter 7.

9. Scott D. Anthony, "Three Questions Every Innovation-Minded CEOs Should Ask," *Chief Executive*, November/December 2008 (available online at http://www.chiefexecutive.net).

THE ANSWERS YOU NEED,

WHEN YOU NEED THEM

DOWNLOAD **BOOK CHAPTERS** NOW



NOT ALL BUSINESS CHALLENGES ARE CREATED EQUAL.

Some require detailed analysis and others demand a thoughtful solution—but in a quick and easily accessible format.

Now you can get instant access to the answers you need by downloading **individual chapters** from our most popular books, including:

- *Blue Ocean Strategy* by W. Chan Kim and Renée Mauborgne
- *Leading Change* by John P. Kotter
- *Groundswell* by Charlene Li and Josh Bernoff
- And many others

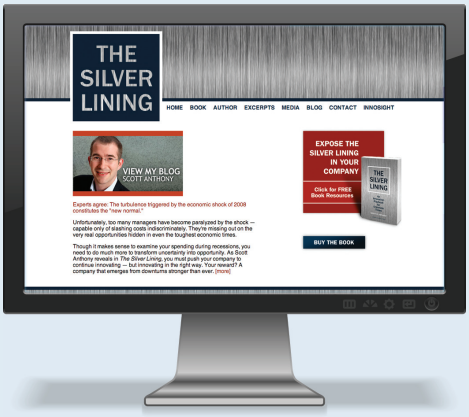
The solutions to your toughest challenges are just a click away.

HARVARD BUSINESS Press

LEARN MORE ABOUT HARVARD BUSINESS PRESS CHAPTERS:
www.harvardbusiness.org/press

TURN IDEAS INTO ACTION WITH

SILVERLININGPLAYBOOK.COM



Take these ideas to the next level, with free online resources created by author Scott Anthony and designed to help you drive innovation and growth in your company.

Silverliningplaybook.com offers you:

- Access to Scott Anthony's most current thinking on innovation
- An "experimentation handbook" that will help you develop and expand your own ideas
- An inside look at how companies like Disney, GE, and Apple stay ahead even in a recession

These free online resources will help you stay strategic and innovative in today's tough economy.

HARVARD BUSINESS PUBLISHING

VISIT THE WEB SITE TODAY:
silverliningplaybook.com