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Beating the Odds When You Launch a New Venture

Smart entrepreneurs aren't cowboys—they're methodical managers of risk. *by Clark G. Gilbert and Matthew J. Eyring*

For nearly 20 years the case study used to introduce Harvard Business School's Entrepreneurial Management course has been Howard Stevenson's "R&R." It looks at Bob Reiss, an entrepreneur who launches a venture in the board-game industry. Students are encouraged to explore all the production, development, distribution, and marketing costs associated with the new venture.

A cursory reading of the case suggests that it's a lesson in the rewards that come to an entrepreneur who is willing to take on an enormous amount of risk. Reiss capitalizes on what he correctly foresees is an ephemeral opportunity to ride the coattails

of the Trivial Pursuit craze before me-too products flood the market. But a more careful analysis reveals something else entirely. At every turn, Reiss seeks to reduce his risks before making any significant financial investments or operational commitments. For example, he presells a sizable number of units to ensure cash flow. As students come to understand, Reiss actually limits his at-risk capital to the cost of the game design and the prototype. Rather than the high-risk, high-reward seeker he initially seems, Reiss proves to be a manager who constantly identifies risks and finds creative ways to remove them.

Over the past decade we have participated in the development of a dozen or so corporate ventures and served on new-venture boards at a host of companies, including Johnson & Johnson, the Scripps Media Center, and Landmark Media Enterprises. Although many of the ideas in this article come from our direct

Tackling the Right Risks First

Risk and value are inversely proportional: When you remove risk, you increase value. But it matters in what sequence you tackle risks, because not all of them are created equal.

Suppose a manager is launching a new e-commerce business. He must remove a number of risks before the venture reaches its peak value. He could simply remove them as they occur to him.



But unless he confirms demand, it doesn't matter how provocative his website is; customers won't buy. And if he doesn't answer the product-mix question, he will fill his warehouse with products he can't sell.



Addressing these two risks early creates disproportionate value quickly, not only saving critical resources but also moving the venture in the right direction sooner.

work with new ventures, they also reflect more than 10 years of collaborative thinking by the Entrepreneurial Management teaching group at HBS.

What has become clear to us is that the most effective corporate innovators are the ones who follow the same discipline Bob Reiss did. Success comes to those who quickly identify and systematically eliminate risks in the right order, using the right level of resources and the right methods.

Recognize That Not All Risks Are Created Equal

New ventures fairly bristle with risks. If managers attempted to eliminate all of them, the products or services would never get to market. The key question is "What's the most important uncertainty?" and the answer should be targeted early. In considering how to answer that question, we have found it useful to think in three broad, sometimes overlapping categories: deal-killer risks, path-dependent risks, and easy-win, high-ROI risks.

Deal-killer risks. As the name implies, these are uncertainties that, if left unresolved, could undermine the entire venture. Such risks may be less obvious in the moment than they appear in hindsight, after catastrophe has struck. That's because they often take the form of unwarranted or unexamined assumptions about the premises underpinning the venture. For example, a colleague of ours was an early employee at a start-up satellite radio company aimed at consumers in the developing world. The premise of the venture was that satellite broadcasting technology would be a relatively cost-effective way to bring mass media to markets that lacked infrastructure. Market research suggested that a huge latent need would turn into a booming business. The company deftly negotiated broadcasting licenses in several developing countries and solved a number of complex technological challenges. Nevertheless, the business imploded. What was the problem?

As it turned out, the demand identified by market research depended on customers' being able to access the broadcasts through low-cost radio receivers—which turned out to be impossible. The radio receiver required complex features such as multi-mode playback, a keypad for ordering subscription services, and—worst of all—professional installation, which made the device unaffordable in most of the developing world. Having failed to identify this fatal vulnerability, the company invested hundreds of millions of dollars to reach consumers who couldn't

Idea in Brief

Despite stereotypes to the contrary, the best entrepreneurs are relentless about managing risks—indeed, that’s their core competency. As the risk level of a new venture goes down, the value goes up.

Risks should be uncovered and hedged in order of their importance and affordability: deal-killers first; then the risk of settling too early on a strategic direction; and finally, operational risks that can be disposed of quickly and cheaply.

All new ventures are partly wrong and partly right. Run small, cheap, fast experiments to determine which bits are which and what course corrections you need to make.

pay for its service. The business limped along before ultimately going bankrupt. The company should not have left this key deal-killer assumption so utterly untested until late in the life of the venture. Quick-hit market research and rapid prototyping could have provided early warning signals.

Path-dependent risks. Rare is the new venture that never has to confront strategic forks in the road to success. Path-dependent risks arise when pursuing the wrong path would involve wasting large sums of money or time or both. For example, consider the question confronting E Ink, a supplier of electronic paper display technologies in Cambridge, Massachusetts. In the company’s early days there was great debate over whether its electronic “ink” would best be used for large-area display signage, flat-panel screens for e-books, or the more ambitious radio-paper products, which could be programmed and updated remotely. Each option had different technical, marketing, and distribution requirements; if the company chose wrong, it risked misallocating millions of dollars.

Rather than choosing one path and hoping for the best, E Ink reduced the cost of pursuing all three by outsourcing its marketing and production capabilities and then focused on resolving the risks associated with the core technology for all three applications. Thus, when display signage proved less successful, the company was not locked into a single market, and the technical knowledge it had developed allowed the fledgling venture to successfully license its technology for more viable products—most notably Amazon’s Kindle.

Risks that can be resolved without spending a lot of time and money. Even after entrepreneurs have considered both deal-killer and path-dependent risks, many uncertainties will remain on the table. If every one were addressed, they’d never get their products to market. But the more risks that

can be eliminated, and the faster they can be removed, the greater the odds of success. Accordingly, successful entrepreneurs also look for risks that are quick and cheap to resolve, applying a cost-benefit approach that we think of as the “experimental ROI”—the amount of risk that can be reduced for each dollar invested in an experiment designed to resolve it. For example, one of the earliest experiments that Reed Hastings, the founder of Netflix, conducted in developing his movie-rental-by-mail business was to mail himself a CD in an envelope. By the time it arrived undamaged, he had spent 24 hours and the cost of postage to test one of the venture’s key operational risks.

Fail to spot a deal-killer risk, and your venture is doomed. Fail to hedge a path-dependent risk, and you dramatically raise the odds that you’ll run out of funds before you ever come to market—or will get there far too late. Fail to address a high-ROI risk in an orderly way, and you may transform a temporary setback into an insurmountable obstacle.

Such was the fate of a start-up we worked with that targeted the nascent medical tourism market. The venture’s value proposition was to fly patients overseas for high-quality, inexpensive medical care, which it expected to deliver at half the cost of the same care in the United States. Several deal-killer risks faced the venture. Unfortunately, rather than tackling them early, by beginning with those that could be tested most quickly and at the least cost, team members plunged into a time-consuming and expensive effort. To gauge demand, they conducted a series of long interviews with *Fortune* 500 corporate benefits managers and insurers around the country. Things looked very promising. However, not until they’d put in nearly six months of work and spent considerable money on travel did they decide to do something they should have done early on: run two simple, high-ROI experiments to test key risks.

WHEN RISKS ARE OVERLOOKED...

15%

Fewer than 15% of firms are still in operation three years after initial funding, according to one study of venture-backed start-ups.

Test Early, Test Cheaply

Perhaps the most dangerous result of injecting too much money too soon into a venture is that it creates a confirmation bias in the minds of venture managers. Instead of testing their assumptions, they become more and more invested in confirming them. But successful entrepreneurs do the opposite: They devise low-cost experiments to disprove a concept before it's too late.

We've found two types of experiments helpful in our work.

TARGETED EXPERIMENTS

These are designed to pinpoint a deal-killer or path-dependent risk. Examples might include running tests on battery life before launching a new portable device, checking for toxicity in a drug before running full-scale efficacy tests, and testing bandwidth and connectivity concerns before launching an online learning program at various locations across the country.



The first involved a seminar to introduce the concept to prospective patients. The second involved several phone calls to U.S. hospitals to discover their unpublished discount prices for certain procedures. In only two weeks (and at virtually no expense), the team learned that patient demand was actually quite tepid and limited to a very narrow band of procedures, and that U.S. hospitals were willing to lower their prices—to near international levels in some cases—if patients paid cash up front. By failing to address their greatest risk—that no market existed for their services—in the cheapest and fastest way, the team members wasted significant resources and missed a critical opportunity to redirect their strategy to something more promising, such as a venture restricted to regional medical travel within the U.S. or travel to a close international destination like Mexico.

A common mistake is to focus on one key risk to the exclusion of others. Sometimes you must be satisfied with partial risk resolution in one area, even as you start to consider and work on risk in another. As a general rule, we have found it's best to select a "stake in the ground" customer early in the life of the venture. You can then confirm a rough price point at which customers can be served, even as you continue to reduce related technical risk.

Be Judicious with Capital

All other things being equal, a large corporation's deep pockets should give it an advantage over bootstrap entrepreneurs when it comes to financing a new venture. But in practice, a parent company's funding procedures are often a major liability—something one of our colleagues, Brad Gambill, has referred to as "the curse of too much capital." Corporations typically allocate money for a new venture all at once, hoping for a large payoff fairly soon. The more money that is sunk into a project at the outset, the less patience the company tends to have

and the more people believe in the validity of their original approach, even in the face of evidence to the contrary.

The way venture capitalists invest in start-ups—by providing capital in multiple rounds as the value of the venture increases—is far more effective. As one of our colleagues puts it, "With each risk you pull off the table, value goes up proportionally." The lower the risk, the greater the value, so this approach favors entrepreneurs who use early funding to reduce the greatest risks—allocating sufficient funds to test the deal-killer risks first and the path-dependent risks as quickly as possible, and then squeezing the most value out of their scarce resources by systematically working through the remaining risks according to the principle of "spend a little to learn a lot."

At many big companies, a project's status correlates almost perfectly with the amount of money invested in it. The competitive advantage of autonomous start-ups is that they have too little money to go far in the wrong direction.

We can demonstrate the power of this dynamic with two very different examples. Vermeer Technologies, a start-up based in Cambridge, Massachusetts, had only one product: a website development tool called FrontPage. The company was eventually sold to Microsoft, and Microsoft FrontPage became the most widely used web-design software package in the world. But that's not where Vermeer's strategy began. In the early 1990s its founders had hoped to create an interface that would allow users to access content through a common reader across a wide network of computers all over the world. There was only one problem: A nascent service—the World Wide Web—was free to anyone who wanted to access it. After Vermeer's founders learned more about the Web, they decided to take another path altogether, devising a software tool that let nontechnical programmers create their own websites. Reflecting on their

INTEGRATED EXPERIMENTS

These are designed to test how various elements—the actual business model and operations—work together. In essence, they involve launching the business, or some part of it, in miniature. Although pilot programs are nothing new, our experience suggests that entrepreneurs rarely give them sufficient time to play out. An exception is

Aaron Kennedy, who founded Noodles & Company, a chain of quick-casual restaurants. From the beginning Kennedy intended to take his concept nationwide, but he started with just three restaurants. He revised the menu, varied the décor, and tested several pricing structures. For almost an entire year he focused on sharpening the concept and

making it work on a small scale. Today the chain has more than 218 locations in 18 states.

An integrated experiment may be a pilot, a test-site location, a prototype, or any other trial operation. It might include tests to “launch” the business in a way that allows customers to purchase the product in a real transactional environment. Targeted experiments such as

surveys and focus groups can provide insights, but those that come from placing the product in a sales channel where customers make actual purchase decisions are often much deeper.

original strategy, the founders laugh in relief that they didn’t make any significant investment at the outset, because they might have poured their capital into building an ultimately worthless company.

An equally instructive example with a less fortunate outcome is that of Joint Juice, a Bay Area company founded by an orthopedic surgeon who came up with the breakthrough idea of converting glucosamine, effective in reducing joint pain, from a large pill into a more convenient liquid. A strong conviction that his target market was young to middle-aged athletes led to a series of expensive choices relating to the product’s caloric load, packaging, distribution channel, and marketing approach. Lavish advertising campaigns were built around professional and Olympic athletes. These early, high-cost investments became self-reinforcing.

Just as data were beginning to reveal that the real demand lay with an older demographic—people who wanted lower-calorie, less-expensive products—an opportunity arose to go national with two large grocery chains. Sunk costs made the opportunity more tempting than it should have been, and Joint Juice signed an expansion contract replete with the high slotting fees associated with grocery retail. When it became clear that the channel and market were wrong, the enterprise was already locked in to a product incorrectly formulated, positioned, and distributed. Today Joint Juice has been adapted to the right market, but only after millions of dollars more were invested—and significant changes were made to the management team.

We cannot make this point too strongly: At the start of a new venture, the only thing you can know about your initial strategy is that it’s probably part right and part wrong. One of our colleagues conducted a study of the *Inc.* 500 entrepreneurs and found that most successful ventures had redirected their strategy at least five times before they hit a solid

Experiments should help redirect a venture, not confirm that your initial ideas were correct.

growth trajectory. If you go full speed in your first direction, you’ll compromise your ability to figure out which part is wrong—and pay a high price when you eventually do figure it out. But if you invest in stages, spending small sums on the assumption that your strategy will need adjustment, you’ll find it much easier to adapt quickly and reach a winning outcome.

Manage Experiments Efficiently

Identifying and prioritizing risks correctly and then conceiving and funding experiments to resolve them systematically will make the unpredictable process of launching a new venture as efficient as it can be. You can take several steps to make your experiments more effective.

Limit the duration. According to Meg Whitman, the former president and CEO of eBay, the company succeeded in its earliest days by recognizing that perfection is sometimes the enemy of the good. It’s often better to get something into the market quickly, learn from it, and move on to the next phase of development than to analyze an idea to death and try to perfect it before launch. Even deal-killer risks can sometimes be tested quickly and simply. For example, Innosight Ventures saw an opportunity to serve consumers in India who couldn’t afford washing machines but wanted an alternative to the traditional *dhobi* services, which are slow, use dirty water and inferior detergents, and beat clothes on rocks to remove the water from them. The venture managers needed only 60 days to move from completion of the

Case Study

Robin Wolaner, who launched *Parenting* magazine, began with an insight: Large numbers of highly educated women were having children much later in their professional careers than had been true in the past. She raised a small amount of seed capital to push her idea for a magazine

forward and chose to spend it on answering the one question that, if unresolved, would render all other risks moot: Is there a differentiated need and a real demand for this product? Wolaner sent out direct-response cards describing a magazine that would focus on both parents and would have a

uniquely sophisticated editorial orientation. Early market tests typically get a response rate of 3% to 4%. Her cards came back at greater than 7%. Because this deal-killer risk was pulled off the table at the outset, valuation jumped from less than \$500,000 to more than \$5 million.

\$5 million

business plan to an initial market test. The test was simple but powerful: They invested a few thousand dollars to build a kiosk that contained a washing machine and a dryer and put it on a busy street corner to see if people were willing to pay 40 rupees (about \$1) per kilogram to wash their clothes. It was essentially a mini-launch designed to answer the key question in their business plan: Is there unmet demand for an inexpensive laundry service? Several weeks of growing customer demand at the site indicated a high likelihood that the concept and pricing were essentially sound and with further refinement could exceed estimated break-even levels. Today more than two dozen kiosks have been set up in several Indian cities, and there are plans to expand the business to more than a thousand over the next few years.

Test one thing at a time. Poorly designed experiments vary too many factors at once, increasing the expense and making it difficult to determine what causes what. Experiments should be simple and focused on resolving uncertainties one by one. At a large media company we worked with, the venture managers ran experiments to test a new website registration system that would allow them to target various demographic segments with ads. They didn't know whether registration should be required or optional. Accordingly, their experiment was designed to answer the questions Will people be discouraged from visiting the sites if they are forced to register? and Will people register at all if they aren't required to? Instead of running tests over an entire network of websites, they picked two comparable sites and for a month ran one with an opt-in registration and the other with a forced registration. Everything else was held constant—promotion, launch, investment, and so forth. When the forced registration didn't reduce site visits significantly, they had their answer.

Apply the lessons learned. Too often managers miss the whole point of these experiments. They

are meant to help redirect a venture, not to confirm that your initial ideas were correct. Some of our colleagues call this discovery-driven learning. Recall the data on the *Inc.* 500 ventures—five major course corrections for every successful venture. Sometimes those corrections come painfully, but it's better to choose to adjust early than be forced to adjust later.

Be willing to turn off experiments. This idea is closely related to the previous point, but requires far more discipline. Some ventures are simply not going to work. A deal-killer risk may in fact kill the deal. The sooner you cut your losses in such cases, the sooner you can go on to the next venture. More often, though, the principle applies to some specific component of the venture. We've watched executives in the newspaper industry struggle with this as they've tried to migrate from print media to digital content. One senior manager confessed to us, "We had a thousand experiments running; some of them were working and some of them were not. Sometimes the challenge isn't turning them on—it's turning them off." When an entrepreneur learns that a product or an approach won't work, it is critical to end the experiment and move in a new direction.

NEW VENTURE formation will always be fraught with risks. We don't want to imply that a systematic approach to identifying and mitigating them will eliminate them. But we do take issue with the notion that it's the risks that produce the rewards. As Bob Reiss's story has illustrated for decades—and our experience continues to confirm—great entrepreneurs don't take risks; they manage them. Quickly determining what's right and what's wrong with key assumptions and then making speedy adjustments often means the difference between failure and success. As entrepreneurial managers learn to do this, they bend the risk-reward curve in their favor and beat the odds. ▢

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