1
START

ENTREPRENEURIAL MANAGEMENT

Building a startup is an exercise in institution building; thus, it necessarily involves management. This often comes as a surprise to aspiring entrepreneurs, because their associations with these two words are so diametrically opposed. Entrepreneurs are rightly wary of implementing traditional management practices early on in a startup, afraid that they will invite bureaucracy or stifle creativity.

Entrepreneurs have been trying to fit the square peg of their unique problems into the round hole of general management for decades. As a result, many entrepreneurs take a "just do it" attitude, avoiding all forms of management, process, and discipline. Unfortunately, this approach leads to chaos more often than it does to success. I should know: my first startup failures were all of this kind.

The tremendous success of general management over the last century has provided unprecedented material abundance, but those management principles are ill suited to handle the chaos and uncertainty that startups must face.

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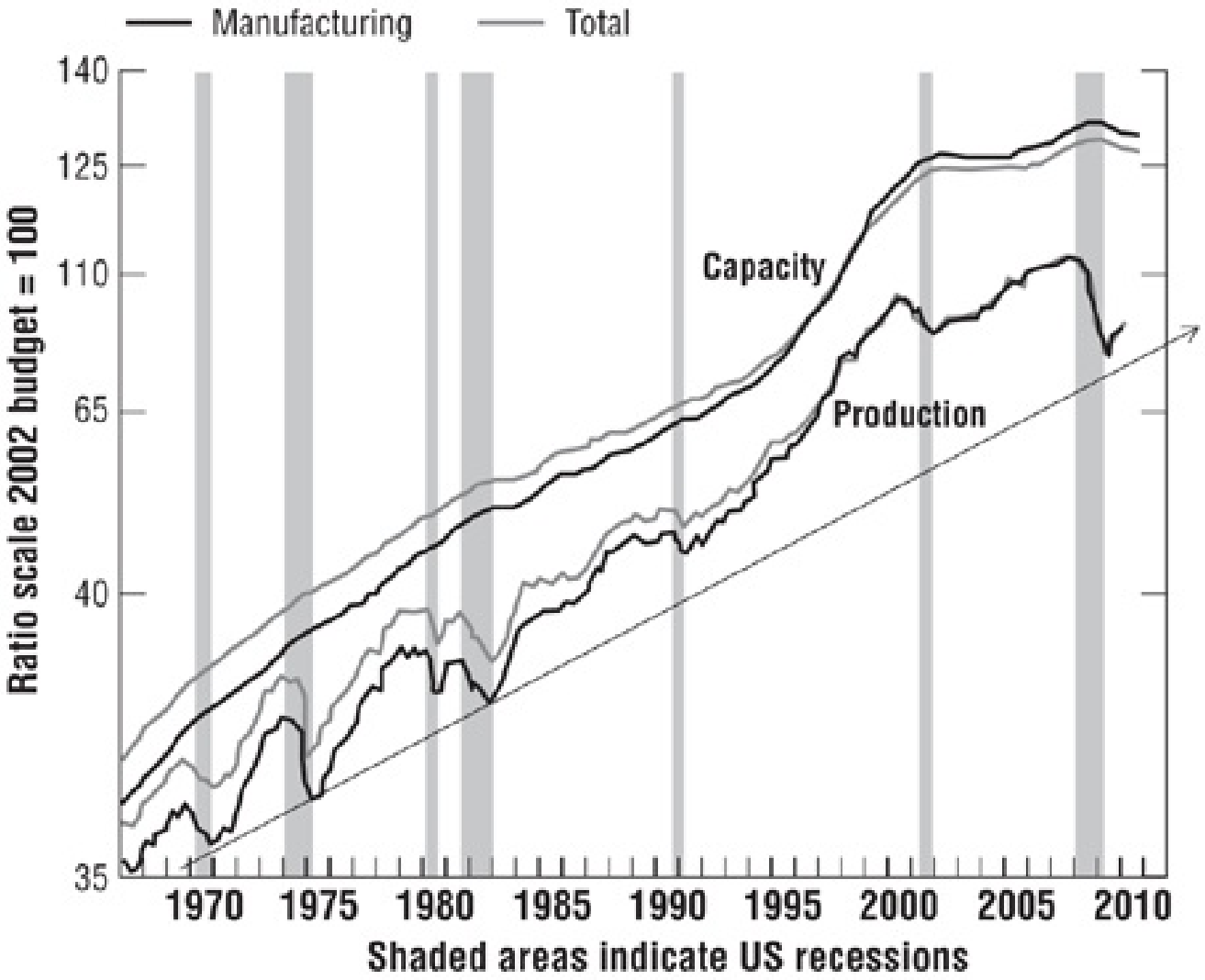
I believe that entrepreneurship requires a managerial discipline to harness the entrepreneurial opportunity we have been given.

There are more entrepreneurs operating today than at any previous time in history. This has been made possible by dramatic

changes in the global economy. To cite but one example, one often hears commentators lament the loss of manufacturing jobs in the United States over the previous two decades, but one rarely hears about a corresponding loss of manufacturing capability. That's because total manufacturing output in the United States is increasing (by 15 percent in the last decade) even as jobs continue to be lost (see the charts below). In effect, the huge productivity increases made possible by modern management and technology have created more productive capacity than firms know what to do with.[1]

We are living through an unprecedented worldwide entrepreneurial renaissance, but this opportunity is laced with peril. Because we lack a coherent management paradigm for new innovative ventures, we're throwing our excess capacity around with wild abandon. Despite this lack of rigor, we are finding some ways to make money, but for every success there are far too many failures: products pulled from shelves mere weeks after being launched, high-profile startups lauded in the press and forgotten a few months later, and new products that wind up being used by nobody. What makes these failures particularly painful is not just the economic damage done to individual employees, companies, and investors; they are also a colossal waste of our civilization's most precious resource: the time, passion, and skill of its people. The Lean Startup movement is dedicated to preventing these failures.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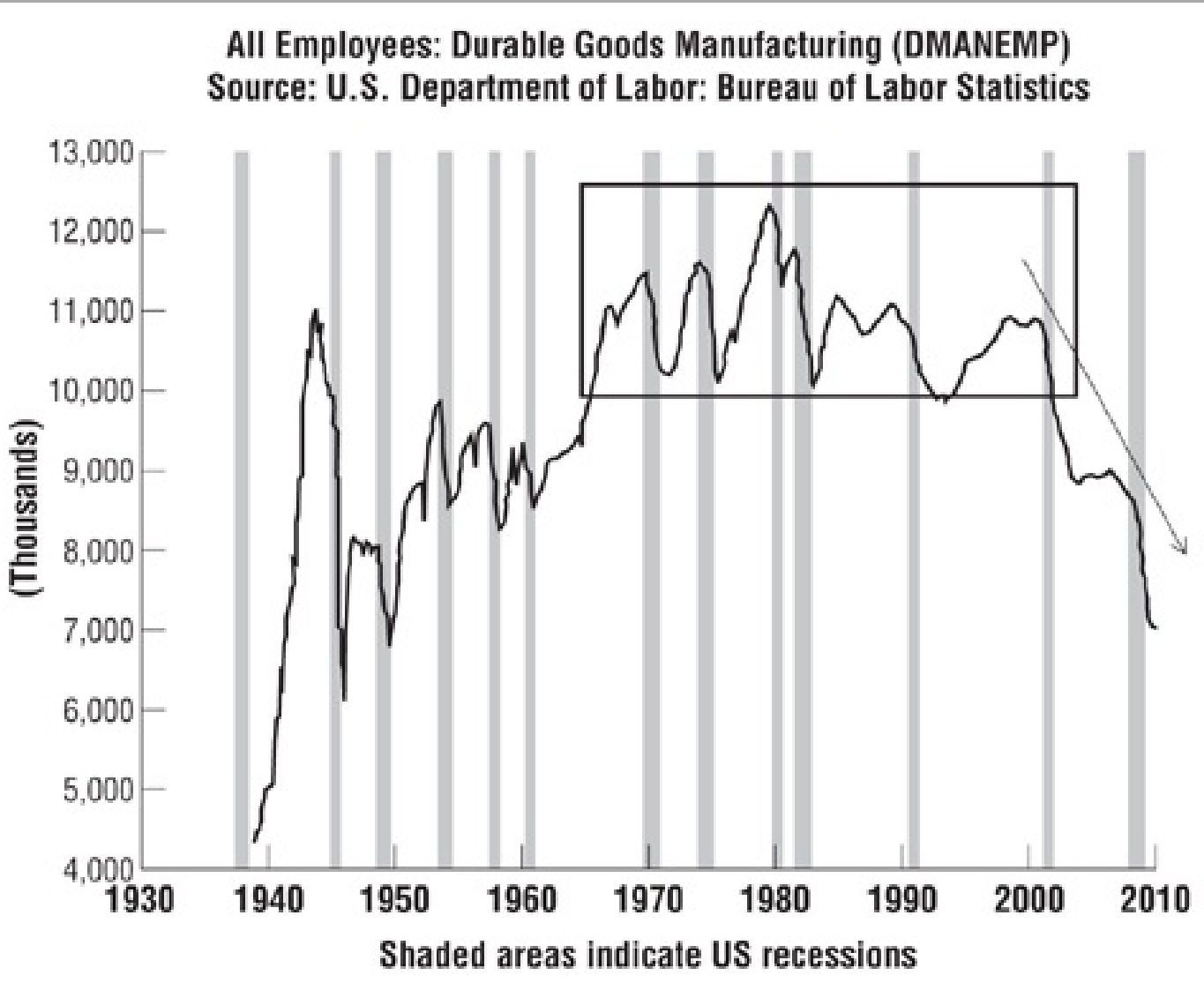
Shaded areas indicate US recessions


All Employees: Durable Goods Manufacturing (DMANEMP)
Source: U.S. Department of Labor: Bureau of Labor Statistics
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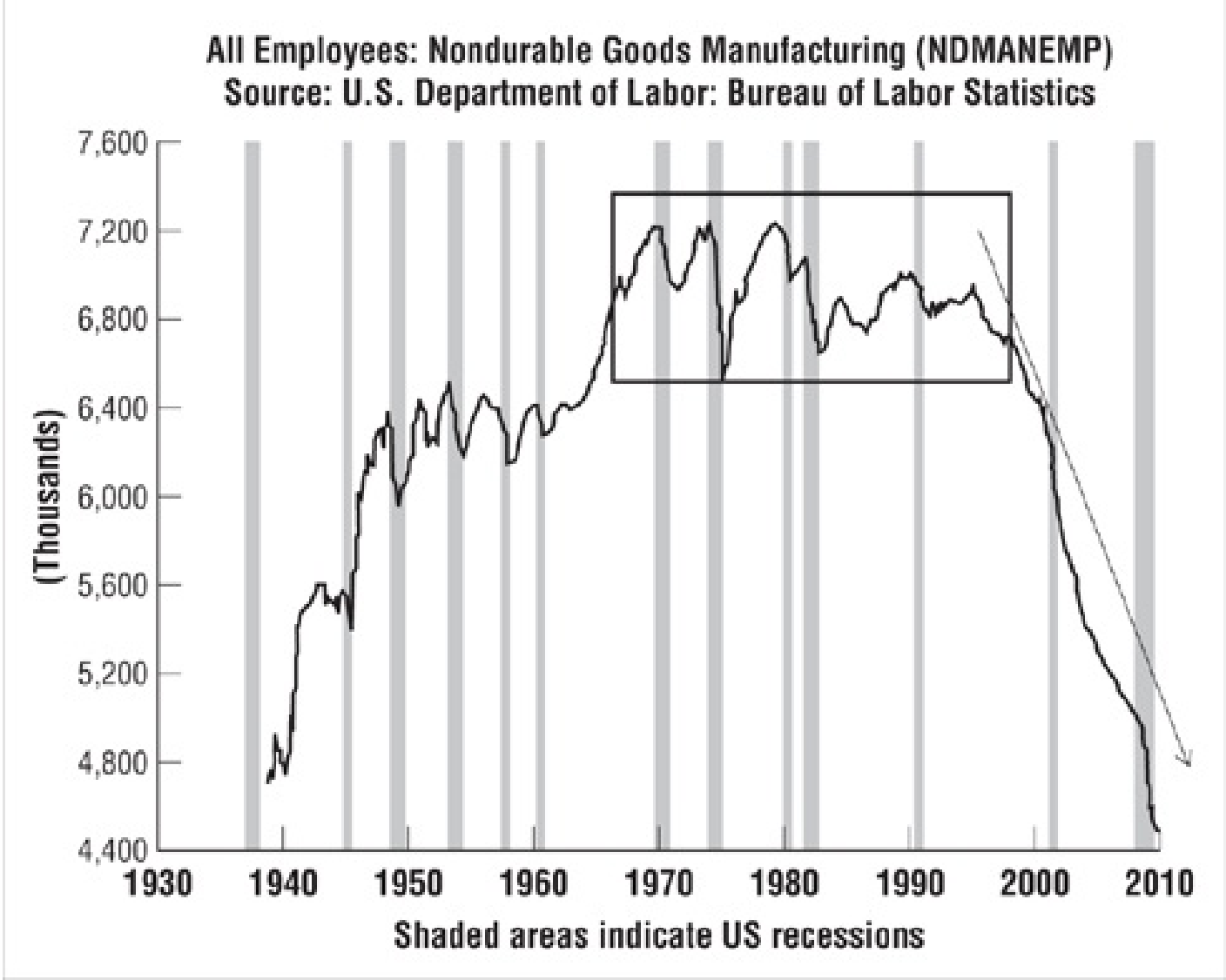

THE ROOTS OF THE LEAN STARTUP

The Lean Startup takes its name from the lean manufacturing revolution that Taiichi Ohno and Shigeo Shingo are credited with developing at Toyota. Lean thinking is radically altering the way supply chains and production systems are run. Among its tenets are drawing on the knowledge and creativity of individual workers, the shrinking of batch sizes, just-in-time production and inventory control, and an acceleration of cycle times. It taught the world the difference between value-creating activities and waste and showed how to build quality into products from the inside out.

The Lean Startup adapts these ideas to the context of entrepreneurship, proposing that entrepreneurs judge their progress differently from the way other kinds of ventures do. Progress in

manufacturing is measured by the production of high-quality physical goods. As we'll see in Chapter 3, the Lean Startup uses a different unit of progress, called validated learning. With scientific learning as our yardstick, we can discover and eliminate the sources of waste that are plaguing entrepreneurship.

A comprehensive theory of entrepreneurship should address all the functions of an early-stage venture: vision and concept, product development, marketing and sales, scaling up, partnerships and distribution, and structure and organizational design. It has to provide a method for measuring progress in the context of extreme uncertainty. It can give entrepreneurs clear guidance on how to make the many trade-off decisions they face: whether and when to invest in process; formulating, planning, and creating infrastructure; when to go it alone and when to partner; when to respond to feedback and when to stick with vision; and how and when to invest in scaling the business. Most of all, it must allow entrepreneurs to make testable predictions.

For example, consider the recommendation that you build cross-functional teams and hold them accountable to what we call learning milestones instead of organizing your company into strict functional departments (marketing, sales, information technology, human resources, etc.) that hold people accountable for performing well in their specialized areas (see Chapter 7). Perhaps you agree with this recommendation, or perhaps you are skeptical. Either way, if you decide to implement it, I predict that you pretty quickly will get feedback from your teams that the new process is reducing their productivity. They will ask to go back to the old way of working, in which they had the opportunity to "stay efficient" by working in larger batches and passing work between departments.

It's safe to predict this result, and not just because I have seen it many times in the companies I work with. It is a straightforward prediction of the Lean Startup theory itself. When people are used to evaluating their productivity locally, they feel that a good day is one in which they did their job well all day. When I worked as a programmer, that meant eight straight hours of programming without interruption. That was a good day. In contrast, if I was

interrupted with questions, process, or—heaven forbid—meetings, I felt bad. What did I really accomplish that day? Code and product features were tangible to me; I could see them, understand them, and show them off. Learning, by contrast, is frustratingly intangible.

The Lean Startup asks people to start measuring their productivity differently. Because startups often accidentally build something nobody wants, it doesn't matter much if they do it on time and on budget. The goal of a startup is to figure out the right thing to build—the thing customers want and will pay for—as quickly as possible. In other words, the Lean Startup is a new way of looking at the development of innovative new products that emphasizes fast iteration and customer insight, a huge vision, and great ambition, all at the same time.

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Henry Ford is one of the most successful and celebrated entrepreneurs of all time. Since the idea of management has been bound up with the history of the automobile since its first days, I believe it is fitting to use the automobile as a metaphor for a startup.

An internal combustion automobile is powered by two important and very different feedback loops. The first feedback loop is deep inside the engine. Before Henry Ford was a famous CEO, he was an engineer. He spent his days and nights tinkering in his garage with the precise mechanics of getting the engine cylinders to move. Each tiny explosion within the cylinder provides the motive force to turn the wheels but also drives the ignition of the next explosion. Unless the timing of this feedback loop is managed precisely, the engine will sputter and break down.

Startups have a similar engine that I call the engine of growth. The markets and customers for startups are diverse: a toy company, a consulting firm, and a manufacturing plant may not seem like they have much in common, but, as we'll see, they operate with the same engine of growth.

Every new version of a product, every new feature, and every

new marketing program is an attempt to improve this engine of growth. Like Henry Ford's tinkering in his garage, not all of these changes turn out to be improvements. New product development happens in fits and starts. Much of the time in a startup's life is spent tuning the engine by making improvements in product, marketing, or operations.

The second important feedback loop in an automobile is between the driver and the steering wheel. This feedback is so immediate and automatic that we often don't think about it, but it is steering that differentiates driving from most other forms of transportation. If you have a daily commute, you probably know the route so well that your hands seem to steer you there on their own accord. We can practically drive the route in our sleep. Yet if I asked you to close your eyes and write down exactly how to get to your office—not the street directions but every action you need to take, every push of hand on wheel and foot on pedals—you'd find it impossible. The choreography of driving is incredibly complex when one slows down to think about it.

By contrast, a rocket ship requires just this kind of in-advance calibration. It must be launched with the most precise instructions on what to do: every thrust, every firing of a booster, and every change in direction. The tiniest error at the point of launch could yield catastrophic results thousands of miles later.

Unfortunately, too many startup business plans look more like they are planning to launch a rocket ship than drive a car. They prescribe the steps to take and the results to expect in excruciating detail, and as in planning to launch a rocket, they are set up in such a way that even tiny errors in assumptions can lead to catastrophic outcomes.

One company I worked with had the misfortune of forecasting significant customer adoption—in the millions—for one of its new products. Powered by a splashy launch, the company successfully executed its plan. Unfortunately, customers did not flock to the product in great numbers. Even worse, the company had invested in massive infrastructure, hiring, and support to handle the influx of customers it expected. When the customers failed to materialize, the

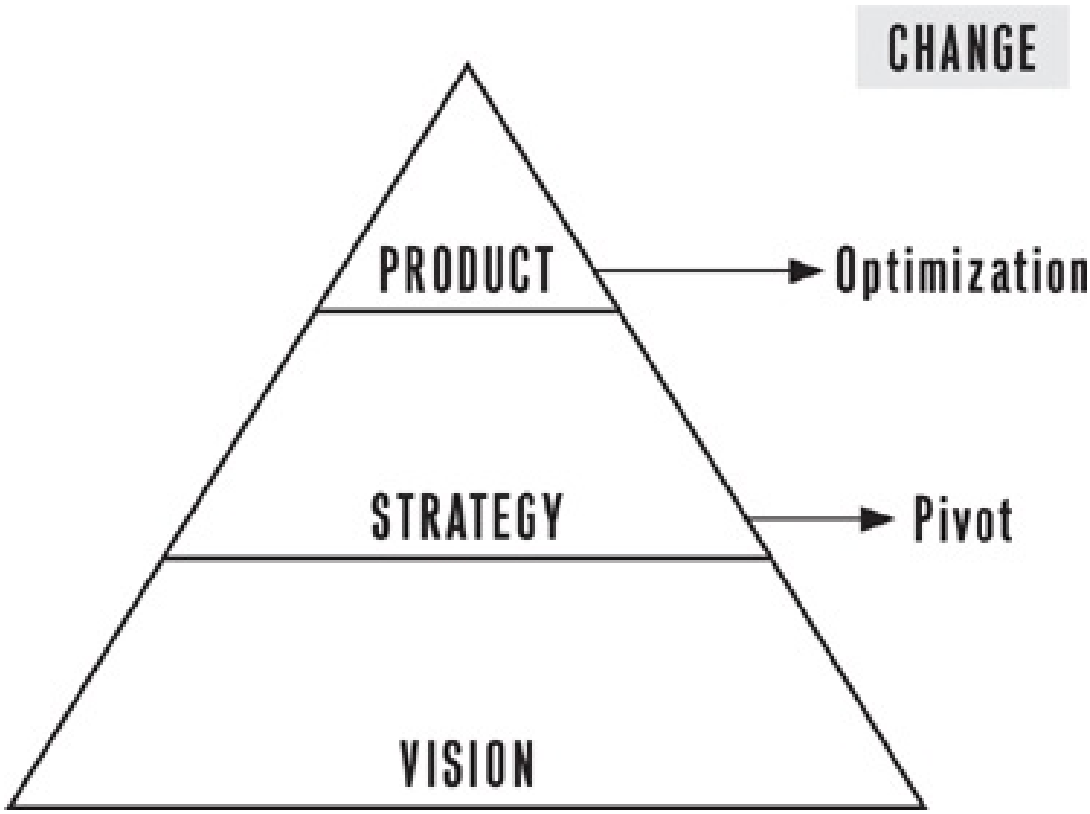
company had committed itself so completely that they could not adapt in time. They had "achieved failure"—successfully, faithfully, and rigorously executing a plan that turned out to have been utterly flawed.

The Lean Startup method, in contrast, is designed to teach you how to drive a startup. Instead of making complex plans that are based on a lot of assumptions, you can make constant adjustments with a steering wheel called the Build-Measure-Learn feedback loop. Through this process of steering, we can learn when and if it's time to make a sharp turn called a pivot or whether we should persevere along our current path. Once we have an engine that's revved up, the Lean Startup offers methods to scale and grow the business with maximum acceleration.

Throughout the process of driving, you always have a clear idea of where you're going. If you're commuting to work, you don't give up because there's a detour in the road or you made a wrong turn. You remain thoroughly focused on getting to your destination.

Startups also have a true north, a destination in mind: creating a thriving and world-changing business. I call that a startup's vision. To achieve that vision, startups employ a strategy, which includes a business model, a product road map, a point of view about partners and competitors, and ideas about who the customer will be. The product is the end result of this strategy (see the chart on this page).

Products change constantly through the process of optimization, what I call tuning the engine. Less frequently, the strategy may have to change (called a pivot). However, the overarching vision rarely changes. Entrepreneurs are committed to seeing the startup through to that destination. Every setback is an opportunity for learning how to get where they want to go (see the chart below).



In real life, a startup is a portfolio of activities. A lot is happening simultaneously: the engine is running, acquiring new customers and serving existing ones; we are tuning, trying to improve our product, marketing, and operations; and we are steering, deciding if and when to pivot. The challenge of entrepreneurship is to balance all these activities. Even the smallest startup faces the challenge of supporting existing customers while trying to innovate. Even the most established company faces the imperative to invest in innovation lest it become obsolete. As companies grow, what changes is the mix of these activities in the company's portfolio of work.

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Entrepreneurship is management. And yet, imagine a modern manager who is tasked with building a new product in the context of an established company. Imagine that she goes back to her company's chief financial officer (CFO) a year later and says, "We have failed to meet the growth targets we predicted. In fact, we have almost no new customers and no new revenue. However, we have learned an incredible amount and are on the cusp of a breakthrough new line of business. All we need is another year." Most of the time, this would be the last report this intrapreneur would give her employer. The reason is that in general management, a failure to deliver results is due to either a failure to plan adequately or a failure to execute properly. Both are significant lapses, yet new product development in our modern economy routinely requires exactly this kind of failure on the way to greatness. In the Lean Startup movement, we have come to realize that these internal innovators are actually entrepreneurs, too, and that entrepreneurial management can help them succeed; this is the subject of the next chapter.

2
DEFINE

WHO, EXACTLY, IS AN ENTREPRENEUR?

As I travel the world talking about the Lean Startup, I'm consistently surprised that I meet people in the audience who seem out of place. In addition to the more traditional startup entrepreneurs I meet, these people are general managers, mostly working in very large companies, who are tasked with creating new ventures or product innovations. They are adept at organizational politics: they know how to form autonomous divisions with separate profit and loss statements (P&Ls) and can shield controversial teams from corporate meddling. The biggest surprise is that they are visionaries. Like the startup founders I have worked with for years, they can see the future of their industries and are prepared to take bold risks to seek out new and innovative solutions to the problems their companies face.

Mark, for example, is a manager for an extremely large company who came to one of my lectures. He is the leader of a division that recently had been chartered to bring his company into the twenty-first century by building a new suite of products designed to take advantage of the Internet. When he came to talk to me afterward, I started to give him the standard advice about how to create innovation teams inside big companies, and he stopped me in midstream: "Yeah, I've read The Innovator's Dilemma.[1] I've got that all taken care of." He was a long-term employee of the company and a successful manager to boot, so managing internal politics was

the least of his problems. I should have known; his success was a testament to his ability to navigate the company's corporate policies, personnel, and processes to get things done.

Next, I tried to give him some advice about the future, about cool new highly leveraged product development technologies. He interrupted me again: "Right. I know all about the Internet, and I have a vision for how our company needs to adapt to it or die."

Mark has all the entrepreneurial prerequisites nailed—proper team structure, good personnel, a strong vision for the future, and an appetite for risk taking—and so it finally occurred to me to ask why he was coming to me for advice. He said, "It's as if we have all of the raw materials: kindling, wood, paper, flint, even some sparks. But where's the fire?" The theories of management that Mark had studied treat innovation like a "black box" by focusing on the structures companies need to put in place to form internal startup teams. But Mark found himself working inside the black box—and in need of guidance.

What Mark was missing was a process for converting the raw materials of innovation into real-world breakthrough successes. Once a team is set up, what should it do? What process should it use? How should it be held accountable to performance milestones? These are questions the Lean Startup methodology is designed to answer.

My point? Mark is an entrepreneur just like a Silicon Valley high-tech founder with a garage startup. He needs the principles of the Lean Startup just as much as the folks I thought of as classic entrepreneurs do.

Entrepreneurs who operate inside an established organization sometimes are called "intrapreneurs" because of the special circumstances that attend building a startup within a larger company. As I have applied Lean Startup ideas in an ever-widening variety of companies and industries, I have come to believe that intrapreneurs have much more in common with the rest of the community of entrepreneurs than most people believe. Thus, when I use the term entrepreneur, I am referring to the whole startup ecosystem regardless of company size, sector, or stage of

development.

This book is for entrepreneurs of all stripes: from young visionaries with little backing but great ideas to seasoned visionaries within larger companies such as Mark—and the people who hold them accountable.

IF I'M AN ENTREPRENEUR, WHAT'S A STARTUP?

The Lean Startup is a set of practices for helping entrepreneurs increase their odds of building a successful startup. To set the record straight, it's important to define what a startup is:

> A startup is a human institution designed to create a new product or service under conditions of extreme uncertainty.

I've come to realize that the most important part of this definition is what it omits. It says nothing about size of the company, the industry, or the sector of the economy. Anyone who is creating a new product or business under conditions of extreme uncertainty is an entrepreneur whether he or she knows it or not and whether working in a government agency, a venture-backed company, a nonprofit, or a decidedly for-profit company with financial investors.

Let's take a look at each of the pieces. The word institution connotes bureaucracy, process, even lethargy. How can that be part of a startup? Yet successful startups are full of activities associated with building an institution: hiring creative employees, coordinating their activities, and creating a company culture that delivers results.

We often lose sight of the fact that a startup is not just about a product, a technological breakthrough, or even a brilliant idea. A startup is greater than the sum of its parts; it is an acutely human enterprise.

The fact that a startup's product or service is a new innovation is also an essential part of the definition and a tricky part too. I prefer to use the broadest definition of product, one that encompasses any

source of value for the people who become customers. Anything those customers experience from their interaction with a company should be considered part of that company's product. This is true of a grocery store, an e-commerce website, a consulting service, and a nonprofit social service agency. In every case, the organization is dedicated to uncovering a new source of value for customers and cares about the impact of its product on those customers.

It's also important that the word innovation be understood broadly. Startups use many kinds of innovation: novel scientific discoveries, repurposing an existing technology for a new use, devising a new business model that unlocks value that was hidden, or simply bringing a product or service to a new location or a previously underserved set of customers. In all these cases, innovation is at the heart of the company's success.

There is one more important part of this definition: the context in which the innovation happens. Most businesses—large and small alike—are excluded from this context. Startups are designed to confront situations of extreme uncertainty. To open up a new business that is an exact clone of an existing business all the way down to the business model, pricing, target customer, and product may be an attractive economic investment, but it is not a startup because its success depends only on execution—so much so that this success can be modeled with high accuracy. (This is why so many small businesses can be financed with simple bank loans; the level of risk and uncertainty is understood well enough that a loan officer can assess its prospects.)

Most tools from general management are not designed to flourish in the harsh soil of extreme uncertainty in which startups thrive. The future is unpredictable, customers face a growing array of alternatives, and the pace of change is ever increasing. Yet most startups—in garages and enterprises alike—still are managed by using standard forecasts, product milestones, and detailed business plans.

THE SNAPTAX STORY

In 2009, a startup decided to try something really audacious. They wanted to liberate taxpayers from expensive tax stores by automating the process of collecting information typically found on W-2 forms (the end-of-year statement that most employees receive from their employer that summarizes their taxable wages for the year). The startup quickly ran into difficulties. Even though many consumers had access to a printer/scanner in their home or office, few knew how to use those devices. After numerous conversations with potential customers, the team lit upon the idea of having customers take photographs of the forms directly from their cell phone. In the process of testing this concept, customers asked something unexpected: would it be possible to finish the whole tax return right on the phone itself?

That was not an easy task. Traditional tax preparation requires consumers to wade through hundreds of questions, many forms, and a lot of paperwork. This startup tried something novel by deciding to ship an early version of its product that could do much less than a complete tax package. The initial version worked only for consumers with a very simple return to file, and it worked only in California.

Instead of having consumers fill out a complex form, they allowed the customers to use the phone's camera to take a picture of their W-2 forms. From that single picture, the company developed the technology to compile and file most of the 1040 EZ tax return. Compared with the drudgery of traditional tax filing, the new product—called SnapTax—provides a magical experience. From its modest beginning, SnapTax grew into a significant startup success story. Its nationwide launch in 2011 showed that customers loved it, to the tune of more than 350,000 downloads in the first three weeks.

This is the kind of amazing innovation you'd expect from a new startup.

However, the name of this company may surprise you. SnapTax was developed by Intuit, America's largest producer of finance, tax, and accounting tools for individuals and small businesses. With

more than 7,700 employees and annual revenues in the billions, Intuit is not a typical startup.[2]

The team that built SnapTax doesn't look much like the archetypal image of entrepreneurs either. They don't work in a garage or eat ramen noodles. Their company doesn't lack for resources. They are paid a full salary and benefits. They come into a regular office every day. Yet they are entrepreneurs.

Stories like this one are not nearly as common inside large corporations as they should be. After all, SnapTax competes directly with one of Intuit's flagship products: the fully featured TurboTax desktop software. Usually, companies like Intuit fall into the trap described in Clayton Christensten's The Innovator's Dilemma: they are very good at creating incremental improvements to existing products and serving existing customers, which Christensen called sustaining innovation, but struggle to create breakthrough new products—disruptive innovation—that can create new sustainable sources of growth.

One remarkable part of the SnapTax story is what the team leaders said when I asked them to account for their unlikely success. Did they hire superstar entrepreneurs from outside the company? No, they assembled a team from within Intuit. Did they face constant meddling from senior management, which is the bane of innovation teams in many companies? No, their executive sponsors created an "island of freedom" where they could experiment as necessary. Did they have a huge team, a large budget, and lots of marketing dollars? Nope, they started with a team of five.

What allowed the SnapTax team to innovate was not their genes, destiny, or astrological signs but a process deliberately facilitated by Intuit's senior management. Innovation is a bottoms-up, decentralized, and unpredictable thing, but that doesn't mean it cannot be managed. It can, but to do so requires a new management discipline, one that needs to be mastered not just by practicing entrepreneurs seeking to build the next big thing but also by the people who support them, nurture them, and hold them accountable. In other words, cultivating entrepreneurship is the

responsibility of senior management. Today, a cutting-edge company such as Intuit can point to success stories like SnapTax because it has recognized the need for a new management paradigm. This is a realization that was years in the making.[3]

A SEVEN-THOUSAND-PERSON LEAN STARTUP

In 1983, Intuit's founder, the legendary entrepreneur Scott Cook, had the radical notion (with cofounder Tom Proulx) that personal accounting should happen by computer. Their success was far from inevitable; they faced numerous competitors, an uncertain future, and an initially tiny market. A decade later, the company went public and subsequently fended off well-publicized attacks from larger incumbents, including the software behemoth Microsoft. Partly with the help of famed venture capitalist John Doerr, Intuit became a fully diversified enterprise, a member of the Fortune 1000 that now provides dozens of market-leading products across its major divisions.

This is the kind of entrepreneurial success we're used to hearing about: a ragtag team of underdogs who eventually achieve fame, acclaim, and significant riches.

Flash-forward to 2002. Cook was frustrated. He had just tabulated ten years of data on all of Intuit's new product introductions and had concluded that the company was getting a measly return on its massive investments. Simply put, too many of its new products were failing. By traditional standards, Intuit is an extremely well-managed company, but as Scott dug into the root causes of those failures, he came to a difficult conclusion: the prevailing management paradigm he and his company had been practicing was inadequate to the problem of continuous innovation in the modern economy.

By fall 2009, Cook had been working to change Intuit's management culture for several years. He came across my early work on the Lean Startup and asked me to give a talk at Intuit. In Silicon Vallev this is not the kind of invitation vou turn down. I

admit I was curious. I was still at the beginning of my Lean Startup journey and didn't have much appreciation for the challenges faced by a Fortune 1000 company like his.

My conversations with Cook and Intuit chief executive officer (CEO) Brad Smith were my initiation into the thinking of modern general managers, who struggle with entrepreneurship every bit as much as do venture capitalists and founders in a garage. To combat these challenges, Scott and Brad are going back to Intuit's roots. They are working to build entrepreneurship and risk taking into all their divisions.

For example, consider one of Intuit's flagship products. Because TurboTax does most of its sales around tax season in the United States, it used to have an extremely conservative culture. Over the course of the year, the marketing and product teams would conceive one major initiative that would be rolled out just in time for tax season. Now they test over five hundred different changes in a two-and-a-half-month tax season. They're running up to seventy different tests per week. The team can make a change live on its website on Thursday, run it over the weekend, read the results on Monday, and come to conclusions starting Tuesday; then they rebuild new tests on Thursday and launch the next set on Thursday night.

As Scott put it, "Boy, the amount of learning they get is just immense now. And what it does is develop entrepreneurs, because when you have only one test, you don't have entrepreneurs, you have politicians, because you have to sell. Out of a hundred good ideas, you've got to sell your idea. So you build up a society of politicians and salespeople. When you have five hundred tests you're running, then everybody's ideas can run. And then you create entrepreneurs who run and learn and can retest and relearn as opposed to a society of politicians. So we're trying to drive that throughout our organization, using examples which have nothing to do with high tech, like the website example. Every business today has a website. You don't have to be high tech to use fast-cycle testing."

This kind of change is hard. After all, the company has a

significant number of existing customers who continue to demand exceptional service and investors who expect steady, growing returns.

> Scott says,
>
> > It goes against the grain of what people have been taught in business and what leaders have been taught. The problem isn't with the teams or the entrepreneurs. They love the chance to quickly get their baby out into the market. They love the chance to have the customer vote instead of the suits voting. The real issue is with the leaders and the middle managers. There are many business leaders who have been successful because of analysis. They think they're analysts, and their job is to do great planning and analyzing and have a plan.

The amount of time a company can count on holding on to market leadership to exploit its earlier innovations is shrinking, and this creates an imperative for even the most entrenched companies to invest in innovation. In fact, I believe a company's only sustainable path to long-term economic growth is to build an "innovation factory" that uses Lean Startup techniques to create disruptive innovations on a continuous basis. In other words, established companies need to figure out how to accomplish what Scott Cook did in 1983, but on an industrial scale and with an established cohort of managers steeped in traditional management culture.

Ever the maverick, Cook asked me to put these ideas to the test, and so I gave a talk that was simulcast to all seven thousand–plus Intuit employees during which I explained the theory of the Lean Startup, repeating my definition: an organization designed to create new products and services under conditions of extreme uncertainty.

What happened next is etched in my memory. CEO Brad Smith had been sitting next to me as I spoke. When I was done, he got up and said before all of Intuit's employees, "Folks, listen up. You

heard Eric's definition of a startup. It has three parts, and we here at Intuit match all three parts of that definition."

Scott and Brad are leaders who realize that something new is needed in management thinking. Intuit is proof that this kind of thinking can work in established companies. Brad explained to me how they hold themselves accountable for their new innovation efforts by measuring two things: the number of customers using products that didn't exist three years ago and the percentage of revenue coming from offerings that did not exist three years ago.

Under the old model, it took an average of 5.5 years for a successful new product to start generating $50 million in revenue. Brad explained to me, "We've generated $50 million in offerings that did not exist twelve months ago in the last year. Now it's not one particular offering. It's a combination of a whole bunch of innovation happening, but that's the kind of stuff that's creating some energy for us, that we think we can truly short-circuit the ramp by killing things that don't make sense fast and doubling down on the ones that do." For a company as large as Intuit, these are modest results and early days. They have decades of legacy systems and legacy thinking to overcome. However, their leadership in adopting entrepreneurial management is starting to pay off.

Leadership requires creating conditions that enable employees to do the kinds of experimentation that entrepreneurship requires. For example, changes in TurboTax enabled the Intuit team to develop five hundred experiments per tax season. Before that, marketers with great ideas couldn't have done those tests even if they'd wanted to, because they didn't have a system in place through which to change the website rapidly. Intuit invested in systems that increased the speed at which tests could be built, deployed, and analyzed.

As Cook says, "Developing these experimentation systems is the responsibility of senior management; they have to be put in by the leadership. It's moving leaders from playing Caesar with their thumbs up and down on every idea to—instead—putting in the culture and the systems so that teams can move and innovate at the speed of the experimentation system."

3
LEARN

As an entrepreneur, nothing plagued me more than the question of whether my company was making progress toward creating a successful business. As an engineer and later as a manager, I was accustomed to measuring progress by making sure our work proceeded according to plan, was high quality, and cost about what we had projected.

After many years as an entrepreneur, I started to worry about measuring progress in this way. What if we found ourselves building something that nobody wanted? In that case what did it matter if we did it on time and on budget? When I went home at the end of a day's work, the only things I knew for sure were that I had kept people busy and spent money that day. I hoped that my team's efforts took us closer to our goal. If we wound up taking a wrong turn, I'd have to take comfort in the fact that at least we'd learned something important.

Unfortunately, "learning" is the oldest excuse in the book for a failure of execution. It's what managers fall back on when they fail to achieve the results we promised. Entrepreneurs, under pressure to succeed, are wildly creative when it comes to demonstrating what we have learned. We can all tell a good story when our job, career, or reputation depends on it.

However, learning is cold comfort to employees who are following an entrepreneur into the unknown. It is cold comfort to the investors who allocate precious money, time, and energy to entrepreneurial teams. It is cold comfort to the organizations—large

and small—that depend on entrepreneurial innovation to survive. You can't take learning to the bank; you can't spend it or invest it. You cannot give it to customers and cannot return it to limited partners. Is it any wonder that learning has a bad name in entrepreneurial and managerial circles?

Yet if the fundamental goal of entrepreneurship is to engage in organization building under conditions of extreme uncertainty, its most vital function is learning. We must learn the truth about which elements of our strategy are working to realize our vision and which are just crazy. We must learn what customers really want, not what they say they want or what we think they should want. We must discover whether we are on a path that will lead to growing a sustainable business.

In the Lean Startup model, we are rehabilitating learning with a concept I call validated learning. Validated learning is not after-the-fact rationalization or a good story designed to hide failure. It is a rigorous method for demonstrating progress when one is embedded in the soil of extreme uncertainty in which startups grow. Validated learning is the process of demonstrating empirically that a team has discovered valuable truths about a startup's present and future business prospects. It is more concrete, more accurate, and faster than market forecasting or classical business planning. It is the principal antidote to the lethal problem of achieving failure: successfully executing a plan that leads nowhere.

VALIDATED LEARNING AT IMVU

Let me illustrate this with an example from my career. Many audiences have heard me recount the story of IMVU's founding and the many mistakes we made in developing our first product. I'll now elaborate on one of those mistakes to illustrate validated learning clearly.

Those of us involved in the founding of IMVU aspired to be serious strategic thinkers. Each of us had participated in previous ventures that had failed, and we were loath to repeat that

experience. Our main concerns in the early days dealt with the following questions: What should we build and for whom? What market could we enter and dominate? How could we build durable value that would not be subject to erosion by competition?[1]

Brilliant Strategy

We decided to enter the instant messaging (IM) market. In 2004, that market had hundreds of millions of consumers actively participating worldwide. However, the majority of the customers who were using IM products were not paying for the privilege. Instead, large media and portal companies such as AOL, Microsoft, and Yahoo! operated their IM networks as a loss leader for other services while making modest amounts of money through advertising.

IM is an example of a market that involves strong network effects. Like most communication networks, IM is thought to follow Metcalfe's law: the value of a network as a whole is proportional to the square of the number of participants. In other words, the more people in the network, the more valuable the network. This makes intuitive sense: the value to each participant is driven primarily by how many other people he or she can communicate with. Imagine a world in which you own the only telephone; it would have no value. Only when other people also have a telephone does it become valuable.

In 2004, the IM market was locked up by a handful of incumbents. The top three networks controlled more than 80 percent of the overall usage and were in the process of consolidating their gains in market share at the expense of a number of smaller players.[2] The common wisdom was that it was more or less impossible to bring a new IM network to market without spending an extraordinary amount of money on marketing.

The reason for that wisdom is simple. Because of the power of network effects, IM products have high switching costs. To switch from one network to another, customers would have to convince

their friends and colleagues to switch with them. This extra work for customers creates a barrier to entry in the IM market: with all consumers locked in to an incumbent's product, there are no customers left with whom to establish a beachhead.

At IMVU we settled on a strategy of building a product that would combine the large mass appeal of traditional IM with the high revenue per customer of three-dimensional (3D) video games and virtual worlds. Because of the near impossibility of bringing a new IM network to market, we decided to build an IM add-on product that would interoperate with the existing networks. Thus, customers would be able to adopt the IMVU virtual goods and avatar communication technology without having to switch IM providers, learn a new user interface, and—most important—bring their friends with them.

In fact, we thought this last point was essential. For the add-on product to be useful, customers would have to use it with their existing friends. Every communication would come embedded with an invitation to join IMVU. Our product would be inherently viral, spreading throughout the existing IM networks like an epidemic. To achieve that viral growth, it was important that our add-on product support as many of the existing IM networks as possible and work on all kinds of computers.

Six Months to Launch

With this strategy in place, my cofounders and I began a period of intense work. As the chief technology officer, it was my responsibility, among other things, to write the software that would support IM interoperability across networks. My cofounders and I worked for months, putting in crazy hours struggling to get our first product released. We gave ourselves a hard deadline of six months —180 days—to launch the product and attract our first paying customers. It was a grueling schedule, but we were determined to launch on time.

The add-on product was so large and complex and had so many

moving parts that we had to cut a lot of corners to get it done on time. I won't mince words: the first version was terrible. We spent endless hours arguing about which bugs to fix and which we could live with, which features to cut and which to try to cram in. It was a wonderful and terrifying time: we were full of hope about the possibilities for success and full of fear about the consequences of shipping a bad product.

Personally, I was worried that the low quality of the product would tarnish my reputation as an engineer. People would think I didn't know how to build a quality product. All of us feared tarnishing the IMVU brand; after all, we were charging people money for a product that didn't work very well. We all envisioned the damning newspaper headlines: "Inept Entrepreneurs Build Dreadful Product."

As launch day approached, our fears escalated. In our situation, many entrepreneurial teams give in to fear and postpone the launch date. Although I understand this impulse, I am glad we persevered, since delay prevents many startups from getting the feedback they need. Our previous failures made us more afraid of another, even worse, outcome than shipping a bad product: building something that nobody wants. And so, teeth clenched and apologies at the ready, we released our product to the public.

Launch

And then—nothing happened! It turned out that our fears were unfounded, because nobody even tried our product. At first I was relieved because at least nobody was finding out how bad the product was, but soon that gave way to serious frustration. After all the hours we had spent arguing about which features to include and which bugs to fix, our value proposition was so far off that customers weren't getting far enough into the experience to find out how bad our design choices were. Customers wouldn't even download our product.

Over the ensuing weeks and months, we labored to make the

product better. We brought in a steady flow of customers through our online registration and download process. We treated each day's customers as a brand-new report card to let us know how we were doing. We eventually learned how to change the product's positioning so that customers at least would download it. We were making improvements to the underlying product continuously, shipping bug fixes and new changes daily. However, despite our best efforts, we were able to persuade only a pathetically small number of people to buy the product.

In retrospect, one good decision we made was to set clear revenue targets for those early days. In the first month we intended to make $300 in total revenue, and we did—barely. Many friends and family members were asked (okay, begged). Each month our small revenue targets increased, first to $350 and then to $400. As they rose, our struggles increased. We soon ran out of friends and family; our frustration escalated. We were making the product better every day, yet our customers' behavior remained unchanged: they still wouldn't use it.

Our failure to move the numbers prodded us to accelerate our efforts to bring customers into our office for in-person interviews and usability tests. The quantitative targets created the motivation to engage in qualitative inquiry and guided us in the questions we asked; this is a pattern we'll see throughout this book.

I wish I could say that I was the one to realize our mistake and suggest the solution, but in truth, I was the last to admit the problem. In short, our entire strategic analysis of the market was utterly wrong. We figured this out empirically, through experimentation, rather than through focus groups or market research. Customers could not tell us what they wanted; most, after all, had never heard of 3D avatars. Instead, they revealed the truth through their action or inaction as we struggled to make the product better.

Talking to Customers

Out of desperation, we decided to talk to some potential customers. We brought them into our office, and said, "Try this new product; it's IMVU." If the person was a teenager, a heavy user of IM, or a tech early adopter, he or she would engage with us. In constrast, if it was a mainstream person, the response was, "Right. So exactly what would you like me to do?" We'd get nowhere with the mainstream group; they thought IMVU was too weird.

Imagine a seventeen-year-old girl sitting down with us to look at this product. She chooses her avatar and says, "Oh, this is really fun." She's customizing the avatar, deciding how she's going to look. Then we say, "All right, it's time to download the instant messaging add-on," and she responds, "What's that?"

"Well, it's this thing that interoperates with the instant messaging client." She's looking at us and thinking, "I've never heard of that, my friends have never heard of that, why do you want me to do that?" It required a lot of explanation; an instant messaging add-on was not a product category that existed in her mind.

But since she was in the room with us, we were able to talk her into doing it. She downloads the product, and then we say, "Okay, invite one of your friends to chat." And she says, "No way!" We say, "Why not?" And she says, "Well, I don't know if this thing is cool yet. You want me to risk inviting one of my friends? What are they going to think of me? If it sucks, they're going to think I suck, right?" And we say, "No, no, it's going to be so much fun once you get the person in there; it's a social product." She looks at us, her face filled with doubt; you can see that this is a deal breaker. Of course, the first time I had that experience, I said, "It's all right, it's just this one person, send her away and get me a new one." Then the second customer comes in and says the same thing. Then the third customer comes in, and it's the same thing. You start to see patterns, and no matter how stubborn you are, there's obviously something wrong.

Customers kept saying, "I want to use it by myself. I want to try it out first to see if it's really cool before I invite a friend." Our team was from the video game industry, so we understood what that meant: single-player mode. So we built a single-player version.

We'd bring new customers into our office. They'd customize the avatar and download the product like before. Then they would go into single-player mode, and we'd say, "Play with your avatar and dress it up; check out the cool moves it can make." Followed by, "Okay, you did that by yourself; now it's time to invite one of your friends." You can see what's coming. They'd say, "No way! This isn't cool." And we'd say, "Well, we told you it wasn't going to be cool! What is the point of a single-player experience for a social product?" See, we thought we should get a gold star just for listening to our customers. Except our customers still didn't like the product. They would look at us and say, "Listen, old man, you don't understand. What is the deal with this crazy business of inviting friends before I know if it's cool?" It was a total deal breaker.

Out of further desperation, we introduced a feature called ChatNow that allows you to push a button and be randomly matched with somebody else anywhere in the world. The only thing you have in common is that you both pushed the button at the same time. All of a sudden, in our customer service tests, people were saying, "Oh, this is fun!"

So we'd bring them in, they'd use ChatNow, and maybe they would meet somebody they thought was cool. They'd say, "Hey, that guy was neat; I want to add him to my buddy list. Where's my buddy list?" And we'd say, "Oh, no, you don't want a new buddy list; you want to use your regular AOL buddy list." Remember, this was how we planned to harness the interoperability that would lead to network effects and viral growth. Picture the customer looking at us, asking, "What do you want me to do exactly?" And we'd say, "Well, just give the stranger your AIM screen name so you can put him on your buddy list." You could see their eyes go wide, and they'd say, "Are you kidding me? A stranger on my AIM buddy list?" To which we'd respond, "Yes; otherwise you'd have to download a whole new IM client with a new buddy list." And they'd say, "Do you have any idea how many IM clients I already run?"

"No. One or two, maybe?" That's how many clients each of us in the office used. To which the teenager would say, "Duh! I run

eight." We had no idea how many instant messaging clients there were in the world.

We had the incorrect preconception that it's a challenge to learn new software and it's tricky to move your friends over to a new buddy list. Our customers revealed that this was nonsense. We wanted to draw diagrams on the whiteboard that showed why our strategy was brilliant, but our customers didn't understand concepts like network effects and switching costs. If we tried to explain why they should behave the way we predicted, they'd just shake their heads at us, bewildered.

We had a mental model for how people used software that was years out of date, and so eventually, painfully, after dozens of meetings like that, it started to dawn on us that the IM add-on concept was fundamentally flawed.[3]

Our customers did not want an IM add-on; they wanted a stand-alone IM network. They did not consider having to learn how to use a new IM program a barrier; on the contrary, our early adopters used many different IM programs simultaneously. Our customers were not intimidated by the idea of having to take their friends with them to a new IM network; it turned out that they enjoyed that challenge. Even more surprising, our assumption that customers would want to use avatar-based IM primarily with their existing friends was also wrong. They wanted to make new friends, an activity that 3D avatars are particularly well suited to facilitating. Bit by bit, customers tore apart our seemingly brilliant initial strategy.

Throwing My Work Away

Perhaps you can sympathize with our situation and forgive my obstinacy. After all, it was my work over the prior months that needed to be thrown away. I had slaved over the software that was required to make our IM program interoperate with other networks, which was at the heart of our original strategy. When it came time to pivot and abandon that original strategy, almost all of

my work—thousands of lines of code—was thrown out. I felt betrayed. I was a devotee of the latest in software development methods (known collectively as agile development), which promised to help drive waste out of product development. However, despite that, I had committed the biggest waste of all: building a product that our customers refused to use. That was really depressing.

I wondered: in light of the fact that my work turned out to be a waste of time and energy, would the company have been just as well off if I had spent the last six months on a beach sipping umbrella drinks? Had I really been needed? Would it have been better if I had not done any work at all?

There is, as I mentioned at the beginning of this chapter, always one last refuge for people aching to justify their own failure. I consoled myself that if we hadn't built this first product—mistakes and all—we never would have learned these important insights about customers. We never would have learned that our strategy was flawed. There is truth in this excuse: what we learned during those critical early months set IMVU on a path that would lead to our eventual breakout success.

For a time, this "learning" consolation made me feel better, but my relief was short-lived. Here's the question that bothered me most of all: if the goal of those months was to learn these important insights about customers, why did it take so long? How much of our effort contributed to the essential lessons we needed to learn? Could we have learned those lessons earlier if I hadn't been so focused on making the product "better" by adding features and fixing bugs?

VALUE VS. WASTE

In other words, which of our efforts are value-creating and which are wasteful? This question is at the heart of the lean manufacturing revolution; it is the first question any lean manufacturing adherent is trained to ask. Learning to see waste and then systematically

eliminate it has allowed lean companies such as Toyota to dominate entire industries. In the world of software, the agile development methodologies I had practiced until that time had their origins in lean thinking. They were designed to eliminate waste too.

Yet those methods had led me down a road in which the majority of my team's efforts were wasted. Why?

The answer came to me slowly over the subsequent years. Lean thinking defines value as providing benefit to the customer; anything else is waste. In a manufacturing business, customers don't care how the product is assembled, only that it works correctly. But in a startup, who the customer is and what the customer might find valuable are unknown, part of the very uncertainty that is an essential part of the definition of a startup. I realized that as a startup, we needed a new definition of value. The real progress we had made at IMVU was what we had learned over those first months about what creates value for customers.

Anything we had done during those months that did not contribute to our learning was a form of waste. Would it have been possible to learn the same things with less effort? Clearly, the answer is yes.

For one thing, think of all the debate and prioritization of effort that went into features that customers would never discover. If we had shipped sooner, we could have avoided that waste. Also consider all the waste caused by our incorrect strategic assumptions. I had built interoperability for more than a dozen different IM clients and networks. Was this really necessary to test our assumptions? Could we have gotten the same feedback from our customers with half as many networks? With only three? With only one? Since the customers of all IM networks found our product equally unattractive, the level of learning would have been the same, but our effort would have been dramatically less.

Here's the thought that kept me up nights: did we have to support any networks at all? Is it possible that we could have discovered how flawed our assumptions were without building anything? For example, what if we simply had offered customers

the opportunity to download the product from us solely on the basis of its proposed features before building anything? Remember, almost no customers were willing to use our original product, so we wouldn't have had to do much apologizing when we failed to deliver. (Note that this is different from asking customers what they want. Most of the time customers don't know what they want in advance.) We could have conducted an experiment, offering customers the chance to try something and then measuring their behavior.

Such thought experiments were extremely disturbing to me because they undermined my job description. As the head of product development, I thought my job was to ensure the timely delivery of high-quality products and features. But if many of those features were a waste of time, what should I be doing instead? How could we avoid this waste?

I've come to believe that learning is the essential unit of progress for startups. The effort that is not absolutely necessary for learning what customers want can be eliminated. I call this validated learning because it is always demonstrated by positive improvements in the startup's core metrics. As we've seen, it's easy to kid yourself about what you think customers want. It's also easy to learn things that are completely irrelevant. Thus, validated learning is backed up by empirical data collected from real customers.

WHERE DO YOU FIND VALIDATION?

As I can attest, anybody who fails in a startup can claim that he or she has learned a lot from the experience. They can tell a compelling story. In fact, in the story of IMVU so far, you might have noticed something missing. Despite my claims that we learned a lot in those early months, lessons that led to our eventual success, I haven't offered any evidence to back that up. In hindsight, it's easy to make such claims and sound credible (and you'll see some evidence later in the book), but imagine us in IMVU's early months

trying to convince investors, employees, family members, and most of all ourselves that we had not squandered our time and resources. What evidence did we have?

Certainly our stories of failure were entertaining, and we had fascinating theories about what we had done wrong and what we needed to do to create a more successful product. However, the proof did not come until we put those theories into practice and built subsequent versions of the product that showed superior results with actual customers.

The next few months are where the true story of IMVU begins, not with our brilliant assumptions and strategies and whiteboard gamesmanship but with the hard work of discovering what customers really wanted and adjusting our product and strategy to meet those desires. We adopted the view that our job was to find a synthesis between our vision and what customers would accept; it wasn't to capitulate to what customers thought they wanted or to tell customers what they ought to want.

As we came to understand our customers better, we were able to improve our products. As we did that, the fundamental metrics of our business changed. In the early days, despite our efforts to improve the product, our metrics were stubbornly flat. We treated each day's customers as a new report card. We'd pay attention to the percentage of new customers who exhibited product behaviors such as downloading and buying our product. Each day, roughly the same number of customers would buy the product, and that number was pretty close to zero despite the many improvements.

However, once we pivoted away from the original strategy, things started to change. Aligned with a superior strategy, our product development efforts became magically more productive—not because we were working harder but because we were working smarter, aligned with our customers' real needs. Positive changes in metrics became the quantitative validation that our learning was real. This was critically important because we could show our stakeholders—employees, investors, and ourselves—that we were making genuine progress, not deluding ourselves. It is also the right way to think about productivity in a startup: not in terms of how

much stuff we are building but in terms of how much validated learning we're getting for our efforts.[4]

For example, in one early experiment, we changed our entire website, home page, and product registration flow to replace "avatar chat" with "3D instant messaging." New customers were split automatically between these two versions of the site; half saw one, and half saw the other. We were able to measure the difference in behavior between the two groups. Not only were the people in the experimental group more likely to sign up for the product, they were more likely to become long-term paying customers.

We had plenty of failed experiments too. During one period in which we believed that customers weren't using the product because they didn't understand its many benefits, we went so far as to pay customer service agents to act as virtual tour guides for new customers. Unfortunately, customers who got that VIP treatment were no more likely to become active or paying customers.

Even after ditching the IM add-on strategy, it still took months to understand why it hadn't worked. After our pivot and many failed experiments, we finally figured out this insight: customers wanted to use IMVU to make new friends online. Our customers intuitively grasped something that we were slow to realize. All the existing social products online were centered on customers' real-life identity. IMVU's avatar technology, however, was uniquely well suited to help people get to know each other online without compromising safety or opening themselves up to identity theft. Once we formed this hypothesis, our experiments became much more likely to produce positive results. Whenever we would change the product to make it easier for people to find and keep new friends, we discovered that customers were more likely to engage. This is true startup productivity: systematically figuring out the right things to build.

These were just a few experiments among hundreds that we ran week in and week out as we started to learn which customers would use the product and why. Each bit of knowledge we

gathered suggested new experiments to run, which moved our metrics closer and closer to our goal.

THE AUDACITY OF ZERO

Despite IMVU's early success, our gross numbers were still pretty small. Unfortunately, because of the traditional way businesses are evaluated, this is a dangerous situation. The irony is that it is often easier to raise money or acquire other resources when you have zero revenue, zero customers, and zero traction than when you have a small amount. Zero invites imagination, but small numbers invite questions about whether large numbers will ever materialize. Everyone knows (or thinks he or she knows) stories of products that achieved breakthrough success overnight. As long as nothing has been released and no data have been collected, it is still possible to imagine overnight success in the future. Small numbers pour cold water on that hope.

This phenomenon creates a brutal incentive: postpone getting any data until you are certain of success. Of course, as we'll see, such delays have the unfortunate effect of increasing the amount of wasted work, decreasing essential feedback, and dramatically increasing the risk that a startup will build something nobody wants.

However, releasing a product and hoping for the best is not a good plan either, because this incentive is real. When we launched IMVU, we were ignorant of this problem. Our earliest investors and advisers thought it was quaint that we had a $300-per-month revenue plan at first. But after several months with our revenue hovering around $500 per month, some began to lose faith, as did some of our advisers, employees, and even spouses. In fact, at one point, some investors were seriously recommending that we pull the product out of the market and return to stealth mode. Fortunately, as we pivoted and experimented, incorporating what we learned into our product development and marketing efforts, our numbers started to improve.

But not by much! On the one hand, we were lucky to see a growth pattern that started to look like the famous hockey stick graph. On the other hand, the graph went up only to a few thousand dollars per month. These early graphs, although promising, were not by themselves sufficient to combat the loss of faith caused by our early failure, and we lacked the language of validated learning to provide an alternative concept to rally around. We were quite fortunate that some of our early investors understood its importance and were willing to look beyond our small gross numbers to see the real progress we were making. (You'll see the exact same graphs they did in Chapter 7.)

Thus, we can mitigate the waste that happens because of the audacity of zero with validated learning. What we needed to demonstrate was that our product development efforts were leading us toward massive success without giving in to the temptation to fall back on vanity metrics and "success theater"—the work we do to make ourselves look successful. We could have tried marketing gimmicks, bought a Super Bowl ad, or tried flamboyant public relations (PR) as a way of juicing our gross numbers. That would have given investors the illusion of traction, but only for a short time. Eventually, the fundamentals of the business would win out and the PR bump would pass. Because we would have squandered precious resources on theatrics instead of progress, we would have been in real trouble.

Sixty million avatars later, IMVU is still going strong. Its legacy is not just a great product, an amazing team, and promising financial results but a whole new way of measuring the progress of startups.

LESSONS BEYOND IMVU

I have had many opportunities to teach the IMVU story as a business case ever since Stanford's Graduate School of Business wrote an official study about IMVU's early years.[5] The case is now part of the entrepreneurship curriculum at several business schools, including Harvard Business School, where I serve as an

entrepreneur in residence. I've also told these stories at countless workshops, lectures, and conferences.

Every time I teach the IMVU story, students have an overwhelming temptation to focus on the tactics it illustrates: launching a low-quality early prototype, charging customers from day one, and using low-volume revenue targets as a way to drive accountability. These are useful techniques, but they are not the moral of the story. There are too many exceptions. Not every kind of customer will accept a low-quality prototype, for example. If the students are more skeptical, they may argue that the techniques do not apply to their industry or situation, but work only because IMVU is a software company, a consumer Internet business, or a non-mission-critical application.

None of these takeaways is especially useful. The Lean Startup is not a collection of individual tactics. It is a principled approach to new product development. The only way to make sense of its recommendations is to understand the underlying principles that make them work. As we'll see in later chapters, the Lean Startup model has been applied to a wide variety of businesses and industries: manufacturing, clean tech, restaurants, and even laundry. The tactics from the IMVU story may or may not make sense in your particular business.

Instead, the way forward is to learn to see every startup in any industry as a grand experiment. The question is not "Can this product be built?" In the modern economy, almost any product that can be imagined can be built. The more pertinent questions are "Should this product be built?" and "Can we build a sustainable business around this set of products and services?" To answer those questions, we need a method for systematically breaking down a business plan into its component parts and testing each part empirically.

In other words, we need the scientific method. In the Lean Startup model, every product, every feature, every marketing campaign—everything a startup does—is understood to be an experiment designed to achieve validated learning. This experimental approach works across industries and sectors, as we'll

see in Chapter 4.

4
EXPERIMENT

I come across many startups that are struggling to answer the following questions: Which customer opinions should we listen to, if any? How should we prioritize across the many features we could build? Which features are essential to the product's success and which are ancillary? What can be changed safely, and what might anger customers? What might please today's customers at the expense of tomorrow's? What should we work on next?

These are some of the questions teams struggle to answer if they have followed the "let's just ship a product and see what happens" plan. I call this the "just do it" school of entrepreneurship after Nike's famous slogan.[1] Unfortunately, if the plan is to see what happens, a team is guaranteed to succeed—at seeing what happens—but won't necessarily gain validated learning. This is one of the most important lessons of the scientific method: if you cannot fail, you cannot learn.

FROM ALCHEMY TO SCIENCE

The Lean Startup methodology reconceives a startup's efforts as experiments that test its strategy to see which parts are brilliant and which are crazy. A true experiment follows the scientific method. It begins with a clear hypothesis that makes predictions about what is supposed to happen. It then tests those predictions empirically. Just as scientific experimentation is informed by theory, startup experimentation is guided by the startup's vision. The goal of every

startup experiment is to discover how to build a sustainable business around that vision.

Think Big, Start Small

Zappos is the world's largest online shoe store, with annual gross sales in excess of $1 billion. It is known as one of the most successful, customer-friendly e-commerce businesses in the world, but it did not start that way.

Founder Nick Swinmurn was frustrated because there was no central online site with a great selection of shoes. He envisioned a new and superior retail experience. Swinmurn could have waited a long time, insisting on testing his complete vision complete with warehouses, distribution partners, and the promise of significant sales. Many early e-commerce pioneers did just that, including infamous dot-com failures such as Webvan and Pets.com.

Instead, he started by running an experiment. His hypothesis was that customers were ready and willing to buy shoes online. To test it, he began by asking local shoe stores if he could take pictures of their inventory. In exchange for permission to take the pictures, he would post the pictures online and come back to buy the shoes at full price if a customer bought them online.

Zappos began with a tiny, simple product. It was designed to answer one question above all: is there already sufficient demand for a superior online shopping experience for shoes? However, a well-designed startup experiment like the one Zappos began with does more than test a single aspect of a business plan. In the course of testing this first assumption, many other assumptions were tested as well. To sell the shoes, Zappos had to interact with customers: taking payment, handling returns, and dealing with customer support. This is decidedly different from market research. If Zappos had relied on existing market research or conducted a survey, it could have asked what customers thought they wanted. By building a product instead, albeit a simple one, the company learned much more:

1. It had more accurate data about customer demand because it was observing real customer behavior, not asking hypothetical questions.
2. It put itself in a position to interact with real customers and learn about their needs. For example, the business plan might call for discounted pricing, but how are customer perceptions of the product affected by the discounting strategy?
3. It allowed itself to be surprised when customers behaved in unexpected ways, revealing information Zappos might not have known to ask about. For example, what if customers returned the shoes?

Zappos' initial experiment provided a clear, quantifiable outcome: either a sufficient number of customers would buy the shoes or they would not. It also put the company in a position to observe, interact with, and learn from real customers and partners. This qualitative learning is a necessary companion to quantitative testing. Although the early efforts were decidedly small-scale, that did not prevent the huge Zappos vision from being realized. In fact, in 2009 Zappos was acquired by the e-commerce giant Amazon.com for a reported $1.2 billion.[2]

For Long-Term Change, Experiment Immediately

Caroline Barlerin is a director in the global social innovation division at Hewlett-Packard (HP), a multinational company with more than three hundred thousand employees and more than $100 billion in annual sales. Caroline, who leads global community involvement, is a social entrepreneur working to get more of HP's employees to take advantage of the company's policy on volunteering.

Corporate guidelines encourage every employee to spend up to four hours a month of company time volunteering in his or her community; that volunteer work could take the form of any philanthropic effort: painting fences, building houses, or even using

pro bono or work-based skills outside the company. Encouraging the latter type of volunteering was Caroline's priority. Because of its talent and values, HP's combined workforce has the potential to have a monumental positive impact. A designer could help a nonprofit with a new website design. A team of engineers could wire a school for Internet access.

Caroline's project is just beginning, and most employees do not know that this volunteering policy exists, and only a tiny fraction take advantage of it. Most of the volunteering has been of the low-impact variety, involving manual labor, even when the volunteers were highly trained experts. Barlerin's vision is to take the hundreds of thousands of employees in the company and transform them into a force for social good.

This is the kind of corporate initiative undertaken every day at companies around the world. It doesn't look like a startup by the conventional definition or what we see in the movies. On the surface it seems to be suited to traditional management and planning. However, I hope the discussion in Chapter 2 has prompted you to be a little suspicious. Here's how we might analyze this project using the Lean Startup framework.

Caroline's project faces extreme uncertainty: there had never been a volunteer campaign of this magnitude at HP before. How confident should she be that she knows the real reasons people aren't volunteering? Most important, how much does she really know about how to change the behavior of hundreds of thousand people in more than 170 countries? Barlerin's goal is to inspire her colleagues to make the world a better place. Looked at that way, her plan seems full of untested assumptions—and a lot of vision.

In accordance with traditional management practices, Barlerin is spending time planning, getting buy-in from various departments and other managers, and preparing a road map of initiatives for the first eighteen months of her project. She also has a strong accountability framework with metrics for the impact her project should have on the company over the next four years. Like many entrepreneurs, she has a business plan that lays out her intentions nicely. Yet despite all that work, she is—so far—creating one-off

wins and no closer to knowing if her vision will be able to scale.

One assumption, for example, might be that the company's long-standing values included a commitment to improving the community but that recent economic trouble had resulted in an increased companywide strategic focus on short-term profitability. Perhaps longtime employees would feel a desire to reaffirm their values of giving back to the community by volunteering. A second assumption could be that they would find it more satisfying and therefore more sustainable to use their actual workplace skills in a volunteer capacity, which would have a greater impact on behalf of the organizations to which they donated their time. Also lurking within Caroline's plans are many practical assumptions about employees' willingness to take the time to volunteer, their level of commitment and desire, and the way to best reach them with her message.

The Lean Startup model offers a way to test these hypotheses rigorously, immediately, and thoroughly. Strategic planning takes months to complete; these experiments could begin immediately. By starting small, Caroline could prevent a tremendous amount of waste down the road without compromising her overall vision. Here's what it might look like if Caroline were to treat her project as an experiment.

Break It Down

The first step would be to break down the grand vision into its component parts. The two most important assumptions entrepreneurs make are what I call the value hypothesis and the growth hypothesis.

The value hypothesis tests whether a product or service really delivers value to customers once they are using it. What's a good indicator that employees find donating their time valuable? We could survey them to get their opinion, but that would not be very accurate because most people have a hard time assessing their feelings objectively.

Experiments provide a more accurate gauge. What could we see in real time that would serve as a proxy for the value participants were gaining from volunteering? We could find opportunities for a small number of employees to volunteer and then look at the retention rate of those employees. How many of them sign up to volunteer again? When an employee voluntarily invests their time and attention in this program, that is a strong indicator that they find it valuable.

For the growth hypothesis, which tests how new customers will discover a product or service, we can do a similar analysis. Once the program is up and running, how will it spread among the employees, from initial early adopters to mass adoption throughout the company? A likely way this program could expand is through viral growth. If that is true, the most important thing to measure is behavior: would the early participants actively spread the word to other employees?

In this case, a simple experiment would involve taking a very small number—a dozen, perhaps—of existing long-term employees and providing an exceptional volunteer opportunity for them. Because Caroline's hypothesis was that employees would be motivated by their desire to live up to HP's historical commitment to community service, the experiment would target employees who felt the greatest sense of disconnect between their daily routine and the company's expressed values. The point is not to find the average customer but to find early adopters: the customers who feel the need for the product most acutely. Those customers tend to be more forgiving of mistakes and are especially eager to give feedback.

Next, using a technique I call the concierge minimum viable product (described in detail in Chapter 6), Caroline could make sure the first few participants had an experience that was as good as she could make it, completely aligned with her vision. Unlike in a focus group, her goal would be to measure what the customers actually did. For example, how many of the first volunteers actually complete their volunteer assignments? How many volunteer a second time? How many are willing to recruit a colleague to

participate in a subsequent volunteer activity?

Additional experiments can expand on this early feedback and learning. For example, if the growth model requires that a certain percentage of participants share their experiences with colleagues and encourage their participation, the degree to which that takes place can be tested even with a very small sample of people. If ten people complete the first experiment, how many do we expect to volunteer again? If they are asked to recruit a colleague, how many do we expect will do so? Remember that these are supposed to be the kinds of early adopters with the most to gain from the program.

Put another way, what if all ten early adopters decline to volunteer again? That would be a highly significant—and very negative—result. If the numbers from such early experiments don't look promising, there is clearly a problem with the strategy. That doesn't mean it's time to give up; on the contrary, it means it's time to get some immediate qualitative feedback about how to improve the program. Here's where this kind of experimentation has an advantage over traditional market research. We don't have to commission a survey or find new people to interview. We already have a cohort of people to talk to as well as knowledge about their actual behavior: the participants in the initial experiment.

This entire experiment could be conducted in a matter of weeks, less than one-tenth the time of the traditional strategic planning process. Also, it can happen in parallel with strategic planning while the plan is still being formulated. Even when experiments produce a negative result, those failures prove instructive and can influence the strategy. For example, what if no volunteers can be found who are experiencing the conflict of values within the organization that was such an important assumption in the business plan? If so, congratulations: it's time to pivot (a concept that is explored in more detail in Chapter 8).[3]

AN EXPERIMENT IS A PRODUCT

In the Lean Startup model, an experiment is more than just a

theoretical inquiry; it is also a first product. If this or any other experiment is successful, it allows the manager to get started with his or her campaign: enlisting early adopters, adding employees to each further experiment or iteration, and eventually starting to build a product. By the time that product is ready to be distributed widely, it will already have established customers. It will have solved real problems and offer detailed specifications for what needs to be built. Unlike a traditional strategic planning or market research process, this specification will be rooted in feedback on what is working today rather than in anticipation of what might work tomorrow.

To see this in action, consider an example from Kodak. Kodak's history is bound up with cameras and film, but today it also operates a substantial online business called Kodak Gallery. Mark Cook is Kodak Gallery's vice president of products, and he is working to change Kodak Gallery's culture of development to embrace experimentation.

Mark explained, "Traditionally, the product manager says, 'I just want this.' In response, the engineer says, 'I'm going to build it.' Instead, I try to push my team to first answer four questions:

1. Do consumers recognize that they have the problem you are trying to solve?
2. If there was a solution, would they buy it?
3. Would they buy it from us?
4. Can we build a solution for that problem?"

The common tendency of product development is to skip straight to the fourth question and build a solution before confirming that customers have the problem. For example, Kodak Gallery offered wedding cards with gilded text and graphics on its site. Those designs were popular with customers who were getting married, and so the team redesigned the cards to be used at other special occasions, such as for holidays. The market research and design process indicated that customers would like the new cards, and that

finding justified the significant effort that went into creating them.

Days before the launch, the team realized the cards were too difficult to understand from their depiction on the website; people couldn't see how beautiful they were. They were also hard to produce. Cook realized that they had done the work backward. He explained, "Until we could figure out how to sell and make the product, it wasn't worth spending any engineering time on."

Learning from that experience, Cook took a different approach when he led his team through the development of a new set of features for a product that makes it easier to share photos taken at an event. They believed that an online "event album" would provide a way for people who attended a wedding, a conference, or another gathering to share photos with other attendees. Unlike other online photo sharing services, Kodak Gallery's event album would have strong privacy controls, assuring that the photos would be shared only with people who attended the same event.

In a break with the past, Cook led the group through a process of identifying risks and assumptions before building anything and then testing those assumptions experimentally.

There were two main hypotheses underlying the proposed event album:

1. The team assumed that customers would want to create the albums in the first place.
2. It assumed that event participants would upload photos to event albums created by friends or colleagues.

The Kodak Gallery team built a simple prototype of the event album. It lacked many features—so many, in fact, that the team was reluctant to show it to customers. However, even at that early stage, allowing customers to use the prototype helped the team refute their hypotheses. First, creating an album was not as easy as the team had predicted; none of the early customers were able to create one. Further, customers complained that the early product version lacked essential features.

Those negative results demoralized the team. The usability

problems frustrated them, as did customer complains about missing features, many of which matched the original road map. Cook explained that even though the product was missing features, the project was not a failure. The initial product—flaws and all—confirmed that users did have the desire to create event albums, which was extremely valuable information. Where customers complained about missing features, this suggested that the team was on the right track. The team now had early evidence that those features were in fact important. What about features that were on the road map but that customers didn't complain about? Maybe those features weren't as important as they initially seemed.

Through a beta launch the team continued to learn and iterate. While the early users were enthusiastic and the numbers were promising, the team made a major discovery. Through the use of online surveying tool KISSinsights, the team learned that many customers wanted to be able to arrange the order of pictures before they would invite others to contribute. Knowing they weren't ready to launch, Cook held off his division's general manager by explaining how iterating and experimenting before beginning the marketing campaign would yield far better results. In a world where marketing launch dates were often set months in advance, waiting until the team had really solved the problem was a break from the past.

This process represented a dramatic change for Kodak Gallery; employees were used to being measured on their progress at completing tasks. As Cook says, "Success is not delivering a feature; success is learning how to solve the customer's problem."[4]

THE VILLAGE LAUNDRY SERVICE

In India, due to the cost of a washing machine, less than seven percent of the population have one in their homes. Most people either hand wash their clothing at home or pay a Dhobi to do it for them. Dhobis take the clothes to the nearest river, wash them in the river water, bang them against rocks to get them clean, and hang

them to dry, which takes two to seven days. The result? Clothes are returned in about ten days and are probably not that clean.

Akshay Mehra had been working at Procter & Gamble Singapore for eight years when he sensed an opportunity. As the brand manager of the Tide and Pantene brands for India and ASEAN countries, he thought he could make laundry services available to people who previously could not afford them. Returning to India, Akshay joined the Village Laundry Services (VLS), created by Innosight Ventures. VLS began a series of experiments to test its business assumptions.

For their first experiment, VLS mounted a consumer-grade laundry machine on the back of a pickup truck parked on a street corner in Bangalore. The experiment cost less than $8,000 and had the simple goal of proving that people would hand over their laundry and pay to have it cleaned. The entrepreneurs did not clean the laundry on the truck, which was more for marketing and show, but took it off-site to be cleaned and brought it back to their customers by the end of the day.

The VLS team continued the experiment for a week, parking the truck on different street corners, digging deeper to discover all they could about their potential customers. They wanted to know how they could encourage people to come to the truck. Did cleaning speed matter? Was cleanliness a concern? What were people asking for when they left their laundry with them? They discovered that customers were happy to give them their laundry to clean. However, those customers were suspicious of the washing machine mounted on the back of the truck, concerned that VLS would take their laundry and run. To address that concern, VLS created a slightly more substantial mobile cart that looked more like a kiosk.

VLS also experimented with parking the carts in front of a local minimarket chain. Further iterations helped VLS figure out which services people were most interested in and what price they were willing to pay. They discovered that customers often wanted their clothes ironed and were willing to pay double the price to get their laundry back in four hours rather than twenty-four hours.

As a result of those early experiments, VLS created an end

product that was a three-foot by four-foot mobile kiosk that included an energy-efficient, consumer-grade washing machine, a dryer, and an extra-long extension cord. The kiosk used Western detergents and was supplied daily with fresh clean water delivered by VLS.

Since then, the Village Laundry Service has grown substantially, with fourteen locations operational in Bangalore, Mysore, and Mumbai. As CEO Akshay Mehra shared with me, "We have serviced 116,000 kgs. in 2010 (vs. 30,600 kg. in 2009). And almost 60 percent of the business is coming from repeat customers. We have serviced more than 10,000 customers in the past year alone across all the outlets."[5]

A LEAN STARTUP IN GOVERNMENT?

On July 21, 2010, President Obama signed the Dodd–Frank Wall Street Reform and Consumer Protection Act into law. One of its landmark provisions created a new federal agency, the Consumer Federal Protection Bureau (CFPB). This agency is tasked with protecting American citizens from predatory lending by financial services companies such as credit card companies, student lenders, and payday loan offices. The plan calls for it to accomplish this by setting up a call center where trained case workers will field calls directly from the public.

Left to its own devices, a new government agency would probably hire a large staff with a large budget to develop a plan that is expensive and time-consuming. However, the CFPB is considering doing things differently. Despite its $500 million budget and high-profile origins, the CPFB is really a startup.

President Obama tasked his chief technology officer, Aneesh Chopra, with collecting ideas for how to set up the new startup agency, and that is how I came to be involved. On one of Chopra's visits to Silicon Valley, he invited a number of entrepreneurs to make suggestions for ways to cultivate a startup mentality in the new agency. In particular, his focus was on leveraging technology

and innovation to make the agency more efficient, cost-effective, and thorough.

My suggestion was drawn straight from the principles of this chapter: treat the CFPB as an experiment, identify the elements of the plan that are assumptions rather than facts, and figure out ways to test them. Using these insights, we could build a minimum viable product and have the agency up and running—on a micro scale—long before the official plan was set in motion.

The number one assumption underlying the current plan is that once Americans know they can call the CFPB for help with financial fraud and abuse, there will be a significant volume of citizens who do that. This sounds reasonable, as it is based on market research about the amount of fraud that affects Americans each year. However, despite all that research, it is still an assumption. If the actual call volume differs markedly from that in the plan, it will require significant revision. What if Americans who are subjected to financial abuse don't view themselves as victims and therefore don't seek help? What if they have very different notions of what problems are important? What if they call the agency seeking help for problems that are outside its purview?

Once the agency is up and running with a $500 million budget and a correspondingly large staff, altering the plan will be expensive and time-consuming, but why wait to get feedback? To start experimenting immediately, the agency could start with the creation of a simple hotline number, using one of the new breed of low-cost and fast setup platforms such as Twilio. With a few hours' work, they could add simple voice prompts, offering callers a menu of financial problems to choose from. In the first version, the prompts could be drawn straight from the existing research. Instead of a caseworker on the line, each prompt could offer the caller useful information about how to solve her or his problem.

Instead of marketing this hotline to the whole country, the agency could run the experiment in a much more limited way: start with a small geographic area, perhaps as small as a few city blocks, and instead of paying for expensive television or radio advertising to let people know about the service, use highly targeted advertising,

Flyers on billboards, newspaper advertisements to those blocks, or specially targeted online ads would be a good start. Since the target area is so small, they could afford to pay a premium to create a high level of awareness in the target zone. The total cost would remain quite small.

As a comprehensive solution to the problem of financial abuse, this minimum viable product is not very good compared with what a $500 million agency could accomplish. But it is also not very expensive. This product could be built in a matter of days or weeks, and the whole experiment probably would cost only a few thousand dollars.

What we would learn from this experiment would be invaluable. On the basis of the selections of those first callers, the agency could immediately start to get a sense of what kinds of problems Americans believe they have, not just what they "should" have. The agency could begin to test marketing messages: What motivates people to call? It could start to extrapolate real-world trends: What percentage of people in the target area actually call? The extrapolation would not be perfect, but it would establish a baseline behavior that would be far more accurate than market research.

Most important, this product would serve as a seed that could germinate into a much more elaborate service. With this beginning, the agency could engage in a continuous process of improvement, slowly but surely adding more and better solutions. Eventually, it would staff the hotline with caseworkers, perhaps at first addressing only one category of problems, to give the caseworkers the best chance of success. By the time the official plan was ready for implementation, this early service could serve as a real-world template.

The CFPB is just getting started, but already they are showing signs of following an experimental approach. For example, instead of doing a geographically limited rollout, they are segmenting their first products by use case. They have established a preliminary order of financial products to provide consumer services for, with credit cards coming first. As their first experiment unfolds, they will

have the opportunity to closely monitor all of the other complaints and consumer feedback they receive. This data will influence the depth, breadth, and sequence of future offerings.

As David Forrest, the CFPB's chief technology officer, told me, "Our goal is to give American citizens an easy way to tell us about the problems they see out there in the consumer financial marketplace. We have an opportunity to closely monitor what the public is telling us and react to new information. Markets change all the time and our job is to change with them."[6]

The entrepreneurs and managers profiled in this book are smart, capable, and extremely results-oriented. In many cases, they are in the midst of building an organization in a way consistent with the best practices of current management thinking. They face the same challenges in both the public and private sectors, regardless of industry. As we've seen, even the seasoned managers and executives at the world's best-run companies struggle to consistently develop and launch innovative new products.

Their challenge is to overcome the prevailing management thinking that puts its faith in well-researched plans. Remember, planning is a tool that only works in the presence of a long and stable operating history. And yet, do any of us feel that the world around us is getting more and more stable every day? Changing such a mind-set is hard but critical to startup success. My hope is that this book will help managers and entrepreneurs make this change.