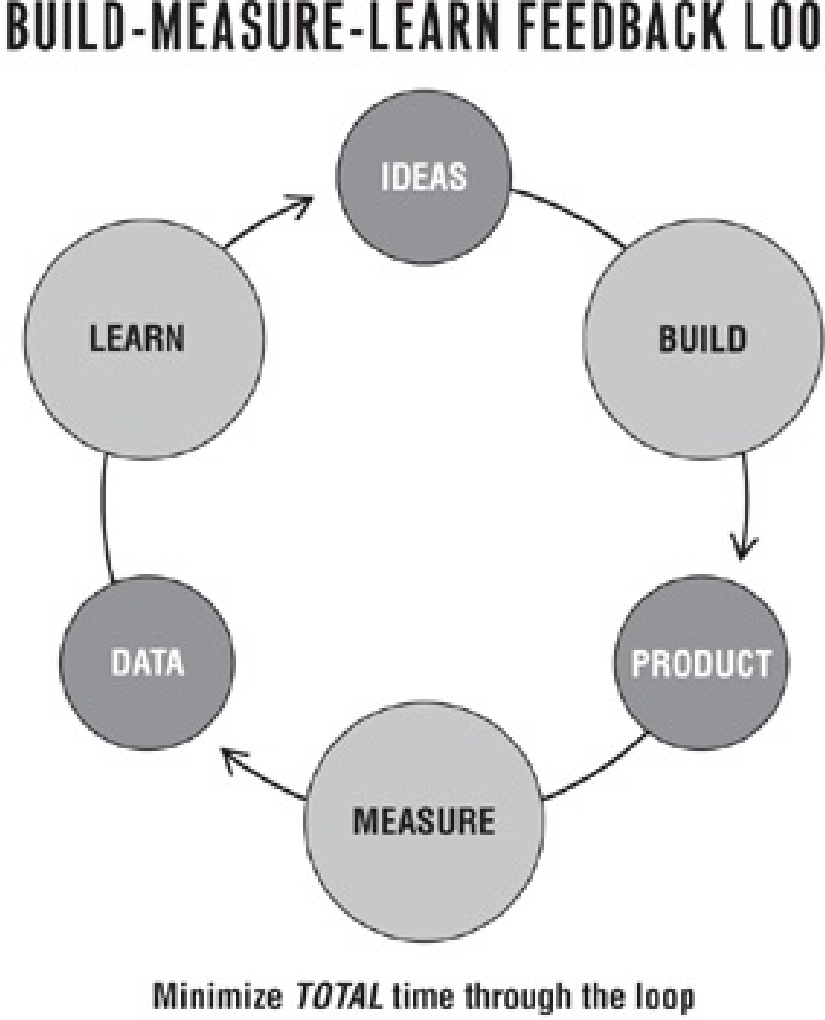
Part Two
STEER

How Vision Leads to Steering

At its heart, a startup is a catalyst that transforms ideas into products. As customers interact with those products, they generate feedback and data. The feedback is both qualitative (such as what they like and don't like) and quantitative (such as how many people use it and find it valuable). As we saw in Part One, the products a startup builds are really experiments; the learning about how to build a sustainable business is the outcome of those experiments. For startups, that information is much more important than dollars, awards, or mentions in the press, because it can influence and reshape the next set of ideas.

We can visualize this three-step process with this simple diagram:



This Build-Measure-Learn feedback loop is at the core of the Lean Startup model. In Part Two, we will examine it in great detail.

Many people have professional training that emphasizes one element of this feedback loop. For engineers, it's learning to build things as efficiently as possible. Some managers are experts at strategizing and learning at the whiteboard. Plenty of entrepreneurs focus their energies on the individual nouns: having the best product idea or the best-designed initial product or obsessing over data and metrics. The truth is that none of these activities by itself is of paramount importance. Instead, we need to focus our energies on minimizing the total time through this feedback loop. This is the essence of steering a startup and is the subject of Part Two. We will walk through a complete turn of the Build-Measure-Learn feedback loop, discussing each of the components in detail.

The purpose of Part One was to explore the importance of learning as the measure of progress for a startup. As I hope is

evident by now, by focusing our energies on validated learning, we can avoid much of the waste that plagues startups today. As in lean manufacturing, learning where and when to invest energy results in saving time and money.

To apply the scientific method to a startup, we need to identify which hypotheses to test. I call the riskiest elements of a startup's plan, the parts on which everything depends, leap-of-faith assumptions. The two most important assumptions are the value hypothesis and the growth hypothesis. These give rise to tuning variables that control a startup's engine of growth. Each iteration of a startup is an attempt to rev this engine to see if it will turn. Once it is running, the process repeats, shifting into higher and higher gears.

Once clear on these leap-of-faith assumptions, the first step is to enter the Build phase as quickly as possible with a minimum viable product (MVP). The MVP is that version of the product that enables a full turn of the Build-Measure-Learn loop with a minimum amount of effort and the least amount of development time. The minimum viable product lacks many features that may prove essential later on. However, in some ways, creating a MVP requires extra work: we must be able to measure its impact. For example, it is inadequate to build a prototype that is evaluated solely for internal quality by engineers and designers. We also need to get it in front of potential customers to gauge their reactions. We may even need to try selling them the prototype, as we'll soon see.

When we enter the Measure phase, the biggest challenge will be determining whether the product development efforts are leading to real progress. Remember, if we're building something that nobody wants, it doesn't much matter if we're doing it on time and on budget. The method I recommend is called innovation accounting, a quantitative approach that allows us to see whether our engine-tuning efforts are bearing fruit. It also allows us to create learning milestones, which are an alternative to traditional business and product milestones. Learning milestones are useful for entrepreneurs as a way of assessing their progress accurately and objectively; they are also invaluable to managers and investors who

must hold entrepreneurs accountable. However, not all metrics are created equal, and in Chapter 7 I'll clarify the danger of vanity metrics in contrast to the nuts-and-bolts usefulness of actionable metrics, which help to analyze customer behavior in ways that support innovation accounting.

Finally, and most important, there's the pivot. Upon completing the Build-Measure-Learn loop, we confront the most difficult question any entrepreneur faces: whether to pivot the original strategy or persevere. If we've discovered that one of our hypotheses is false, it is time to make a major change to a new strategic hypothesis.

The Lean Startup method builds capital-efficient companies because it allows startups to recognize that it's time to pivot sooner, creating less waste of time and money. Although we write the feedback loop as Build-Measure-Learn because the activities happen in that order, our planning really works in the reverse order: we figure out what we need to learn, use innovation accounting to figure out what we need to measure to know if we are gaining validated learning, and then figure out what product we need to build to run that experiment and get that measurement. All of the techniques in Part Two are designed to minimize the total time through the Build-Measure-Learn feedback loop.

5
LEAP

In 2004, three college sophomores arrived in Silicon Valley with their fledgling college social network. It was live on a handful of college campuses. It was not the market-leading social network or even the first college social network; other companies had launched sooner and with more features. With 150,000 registered users, it made very little revenue, yet that summer they raised their first $500,000 in venture capital. Less than a year later, they raised an additional $12.7 million.

Of course, by now you've guessed that these three college sophomores were Mark Zuckerberg, Dustin Moskovitz, and Chris Hughes of Facebook. Their story is now world famous. Many things about it are remarkable, but I'd like to focus on only one: how Facebook was able to raise so much money when its actual usage was so small.[1]

By all accounts, what impressed investors the most were two facts about Facebook's early growth. The first fact was the raw amount of time Facebook's active users spent on the site. More than half of the users came back to the site every single day.[2] This is an example of how a company can validate its value hypothesis—that customers find the product valuable. The second impressive thing about Facebook's early traction was the rate at which it had taken over its first few college campuses. The rate of growth was staggering: Facebook launched on February 4, 2004, and by the end of that month almost three-quarters of Harvard's undergraduates were using it, without a dollar of marketing or advertising having been

spent. In other words, Facebook also had validated its growth hypothesis. These two hypotheses represent two of the most important leap-of-faith questions any new startup faces.[3]

At the time, I heard many people criticize Facebook's early investors, claiming that Facebook had "no business model" and only modest revenues relative to the valuation offered by its investors. They saw in Facebook a return to the excesses of the dot-com era, when companies with little revenue raised massive amounts of cash to pursue a strategy of "attracting eyeballs" and "getting big fast." Many dot-com-era startups planned to make money later by selling the eyeballs they had bought to other advertisers. In truth, those dot-com failures were little more than middlemen, effectively paying money to acquire customers' attention and then planning to resell it to others. Facebook was different, because it employed a different engine of growth. It paid nothing for customer acquisition, and its high engagement meant that it was accumulating massive amounts of customer attention every day. There was never any question that attention would be valuable to advertisers; the only question was how much they would pay.

Many entrepreneurs are attempting to build the next Facebook, yet when they try to apply the lessons of Facebook and other famous startup success stories, they quickly get confused. Is the lesson of Facebook that startups should not charge customers money in the early days? Or is it that startups should never spend money on marketing? These questions cannot be answered in the abstract; there are an almost infinite number of counterexamples for any technique. Instead, as we saw in Part One, startups need to conduct experiments that help determine what techniques will work in their unique circumstances. For startups, the role of strategy is to help figure out the right questions to ask.

STRATEGY IS BASED ON ASSUMPTIONS

Every business plan begins with a set of assumptions. It lays out a strategy that takes those assumptions as a given and proceeds to

show how to achieve the company's vision. Because the assumptions haven't been proved to be true (they are assumptions, after all) and in fact are often erroneous, the goal of a startup's early efforts should be to test them as quickly as possible.

What traditional business strategy excels at is helping managers identify clearly what assumptions are being made in a particular business. The first challenge for an entrepreneur is to build an organization that can test these assumptions systematically. The second challenge, as in all entrepreneurial situations, is to perform that rigorous testing without losing sight of the company's overall vision.

Many assumptions in a typical business plan are unexceptional. These are well-established facts drawn from past industry experience or straightforward deductions. In Facebook's case, it was clear that advertisers would pay for customers' attention. Hidden among these mundane details are a handful of assumptions that require more courage to state—in the present tense—with a straight face: we assume that customers have a significant desire to use a product like ours, or we assume that supermarkets will carry our product. Acting as if these assumptions are true is a classic entrepreneur superpower. They are called leaps of faith precisely because the success of the entire venture rests on them. If they are true, tremendous opportunity awaits. If they are false, the startup risks total failure.

Most leaps of faith take the form of an argument by analogy. For example, one business plan I remember argued as follows: "Just as the development of progressive image loading allowed the widespread use of the World Wide Web over dial-up, so too our progressive rendering technology will allow our product to run on low-end personal computers." You probably have no idea what progressive image loading or rendering is, and it doesn't much matter. But you know the argument (perhaps you've even used it):

> Previous technology X was used to win market Y because of attribute Z. We have a new technology X2 that will enable us to win market Y2 because we too have attribute Z.

The problem with analogies like this is that they obscure the true leap of faith. That is their goal: to make the business seem less risky. They are used to persuade investors, employees, or partners to sign on. Most entrepreneurs would cringe to see their leap of faith written this way:

> Large numbers of people already wanted access to the World Wide Web. They knew what it was, they could afford it, but they could not get access to it because the time it took to load images was too long. When progressive image loading was introduced, it allowed people to get onto the World Wide Web and tell their friends about it. Thus, company X won market Y.
>
> Similarly, there is already a large number of potential customers who want access to our product right now. They know they want it, they can afford it, but they cannot access it because the rendering is too slow. When we debut our product with progressive rendering technology, they will flock to our software and tell their friends, and we will win market Y2.

There are several things to notice in this revised statement. First, it's important to identify the facts clearly. Is it really true that progressive image loading caused the adoption of the World Wide Web, or was this just one factor among many? More important, is it really true that there are large numbers of potential customers out there who want our solution right now? The earlier analogy was designed to convince stakeholders that a reasonable first step is to build the new startup's technology and see if customers will use it. The restated approach should make clear that what is needed is to do some empirical testing first: let's make sure that there really are hungry customers out there eager to embrace our new technology.

Analogs and Antilogs

There is nothing intrinsically wrong with basing strategy on comparisons to other companies and industries. In fact, that approach can help you discover assumptions that are not really leaps of faith. For example, the venture capitalist Randy Komisar, whose book Getting to Plan B discussed the concept of leaps of faith in great detail, uses a framework of "analogs" and "antilogs" to plot strategy.

He explains the analog-antilog concept by using the iPod as an example. "If you were looking for analogs, you would have to look at the Walkman," he says. "It solved a critical question that Steve Jobs never had to ask himself: Will people listen to music in a public place using earphones? We think of that as a nonsense question today, but it is fundamental. When Sony asked the question, they did not have the answer. Steve Jobs had [the answer] in the analog [version]" Sony's Walkman was the analog. Jobs then had to face the fact that although people were willing to download music, they were not willing to pay for it. "Napster was an antilog. That antilog had to lead him to address his business in a particular way," Komisar says. "Out of these analogs and antilogs come a series of unique, unanswered questions. Those are leaps of faith that I, as an entrepreneur, am taking if I go through with this business venture. They are going to make or break my business. In the iPod business, one of those leaps of faith was that people would pay for music." Of course that leap of faith turned out to be correct.[4]

Beyond "The Right Place at the Right Time"

There are any number of famous entrepreneurs who made millions because they seemed to be in the right place at the right time. However, for every successful entrepreneur who was in the right place in the right time, there are many more who were there, too, in that right place at the right time but still managed to fail. Henry Ford was joined by nearly five hundred other entrepreneurs in the early twentieth century. Imagine being an automobile entrepreneur, trained in state-of-the-art engineering, on the ground floor of one of

the biggest market opportunities in history. Yet the vast majority managed to make no money at all.[5] We saw the same phenomenon with Facebook, which faced early competition from other college-based social networks whose head start proved irrelevant.

What differentiates the success stories from the failures is that the successful entrepreneurs had the foresight, the ability, and the tools to discover which parts of their plans were working brilliantly and which were misguided, and adapt their strategies accordingly.

Value and Growth

As we saw in the Facebook story, two leaps of faith stand above all others: the value creation hypothesis and the growth hypothesis. The first step in understanding a new product or service is to figure out if it is fundamentally value-creating or value-destroying. I use the language of economics in referring to value rather than profit, because entrepreneurs include people who start not-for-profit social ventures, those in public sector startups, and internal change agents who do not judge their success by profit alone. Even more confusing, there are many organizations that are wildly profitable in the short term but ultimately value-destroying, such as the organizers of Ponzi schemes, and fraudulent or misguided companies (e.g., Enron and Lehman Brothers).

A similar thing is true for growth. As with value, it's essential that entrepreneurs understand the reasons behind a startup's growth. There are many value-destroying kinds of growth that should be avoided. An example would be a business that grows through continuous fund-raising from investors and lots of paid advertising but does not develop a value-creating product.

Such businesses are engaged in what I call success theater, using the appearance of growth to make it seem that they are successful. One of the goals of innovation accounting, which is discussed in depth in Chapter 7, is to help differentiate these false startups from true innovators. Traditional accounting judges new ventures by the same standards it uses for established companies, but these

indications are not reliable predictors of a startup's future prospects. Consider companies such as Amazon.com that racked up huge losses on their way to breakthrough success.

Like its traditional counterpart, innovation accounting requires that a startup have and maintain a quantitative financial model that can be used to evaluate progress rigorously. However, in a startup's earliest days, there is not enough data to make an informed guess about what this model might look like. A startup's earliest strategic plans are likely to be hunch- or intuition-guided, and that is a good thing. To translate those instincts into data, entrepreneurs must, in Steve Blank's famous phrase, "get out of the building" and start learning.

GENCHI GEMBUTSU

The importance of basing strategic decisions on firsthand understanding of customers is one of the core principles that underlies the Toyota Production System. At Toyota, this goes by the Japanese term genchi gembutsu, which is one of the most important phrases in the lean manufacturing vocabulary. In English, it is usually translated as a directive to "go and see for yourself" so that business decisions can be based on deep firsthand knowledge. Jeffrey Liker, who has extensively documented the "Toyota Way," explains it this way:

> In my Toyota interviews, when I asked what distinguishes the Toyota Way from other management approaches, the most common first response was genchi gembutsu —whether I was in manufacturing, product development, sales, distribution, or public affairs. You cannot be sure you really understand any part of any business problem unless you go and see for yourself firsthand. It is unacceptable to take anything for granted or to rely on the reports of others.[6]

To demonstrate, take a look at the development of Toyota's Sienna minivan for the 2004 model year. At Toyota, the manager responsible for the design and development of a new model is called the chief engineer, a cross-functional leader who oversees the entire process from concept to production. The 2004 Sienna was assigned to Yuji Yokoya, who had very little experience in North America, which was the Sienna's primary market. To figure out how to improve the minivan, he proposed an audacious entrepreneurial undertaking: a road trip spanning all fifty U.S. states, all thirteen provinces and territories of Canada, and all parts of Mexico. In all, he logged more than 53,000 miles of driving. In small towns and large cities, Yokoya would rent a current-model Sienna, driving it in addition to talking to and observing real customers. From those firsthand observations, Yokoya was able to start testing his critical assumptions about what North American consumers wanted in a minivan.

It is common to think of selling to consumers as easier than selling to enterprises, because customers lack the complexity of multiple departments and different people playing different roles in the purchasing process. Yokoya discovered this was untrue for his customers: "The parents and grandparents may own the minivan. But it's the kids who rule it. It's the kids who occupy the rear two-thirds of the vehicle. And it's the kids who are the most critical—and the most appreciative of their environment. If I learned anything in my travels, it was the new Sienna would need kid appeal."[7] Identifying these assumptions helped guide the car's development. For example, Yokoya spent an unusual amount of the Sienna's development budget on internal comfort features, which are critical to a long-distance family road trip (such trips are much more common in America than in Japan).

The results were impressive, boosting the Sienna's market share dramatically. The 2004 model's sales were 60 percent higher than those in 2003. Of course, a product like the Sienna is a classic sustaining innovation, the kind that the world's best-managed established companies, such as Toyota, excel at. Entrepreneurs face

a different set of challenges because they operate with much higher uncertainty. While a company working on a sustaining innovation knows enough about who and where their customers are to use genchi gembutsu to discover what customers want, startups' early contact with potential customers merely reveals what assumptions require the most urgent testing.

GET OUT OF THE BUILDING

Numbers tell a compelling story, but I always remind entrepreneurs that metrics are people, too. No matter how many intermediaries lie between a company and its customers, at the end of the day, customers are breathing, thinking, buying individuals. Their behavior is measurable and changeable. Even when one is selling to large institutions, as in a business-to-business model, it helps to remember that those businesses are made up of individuals. All successful sales models depend on breaking down the monolithic view of organizations into the disparate people that make them up.

As Steve Blank has been teaching entrepreneurs for years, the facts that we need to gather about customers, markets, suppliers, and channels exist only "outside the building." Startups need extensive contact with potential customers to understand them, so get out of your chair and get to know them.

The first step in this process is to confirm that your leap-of-faith questions are based in reality, that the customer has a significant problem worth solving.[8] When Scott Cook conceived Intuit in 1982, he had a vision—at that time quite radical—that someday consumers would use personal computers to pay bills and keep track of expenses. When Cook left his consulting job to take the entrepreneurial plunge, he didn't start with stacks of market research or in-depth analysis at the whiteboard. Instead, he picked up two phone books: one for Palo Alto, California, where he was living at the time, and the other for Winnetka, Illinois.

Calling people at random, he inquired if he could ask them a few questions about the way they managed their finances. Those early

conversations were designed to answer this leap-of-faith question: do people find it frustrating to pay bills by hand? It turned out that they did, and this early validation gave Cook the confirmation he needed to get started on a solution.[9]

Those early conversations did not delve into the product features of a proposed solution; that attempt would have been foolish. The average consumers at that time were not conversant enough with personal computers to have an opinion about whether they'd want to use them in a new way. Those early conversations were with mainstream customers, not early adopters. Still, the conversations yielded a fundamental insight: if Intuit could find a way to solve this problem, there could be a large mainstream audience on which it could build a significant business.

Design and the Customer Archetype

The goal of such early contact with customers is not to gain definitive answers. Instead, it is to clarify at a basic, coarse level that we understand our potential customer and what problems they have. With that understanding, we can craft a customer archetype, a brief document that seeks to humanize the proposed target customer. This archetype is an essential guide for product development and ensures that the daily prioritization decisions that every product team must make are aligned with the customer to whom the company aims to appeal.

There are many techniques for building an accurate customer archetype that have been developed over long years of practice in the design community. Traditional approaches such as interaction design or design thinking are enormously helpful. To me, it has always seemed ironic that many of these approaches are highly experimental and iterative, using techniques such as rapid prototyping and in-person customer observations to guide designers' work. Yet because of the way design agencies traditionally have been compensated, all this work culminates in a monolithic deliverable to the client. All of a sudden, the rapid

learning and experimentation stops; the assumption is that the designers have learned all there is to know. For startups, this is an unworkable model. No amount of design can anticipate the many complexities of bringing a product to life in the real world.

In fact, a new breed of designers is developing brand-new techniques under the banner of Lean User Experience (Lean UX). They recognize that the customer archetype is a hypothesis, not a fact. The customer profile should be considered provisional until the strategy has shown via validated learning that we can serve this type of customer in a sustainable way.[10]

ANALYSIS PARALYSIS

There are two ever-present dangers when entrepreneurs conduct market research and talk to customers. Followers of the just-do-it school of entrepreneurship are impatient to get started and don't want to spend time analyzing their strategy. They'd rather start building immediately, often after just a few cursory customer conversations. Unfortunately, because customers don't really know what they want, it's easy for these entrepreneurs to delude themselves that they are on the right path.

Other entrepreneurs can fall victim to analysis paralysis, endlessly refining their plans. In this case, talking to customers, reading research reports, and whiteboard strategizing are all equally unhelpful. The problem with most entrepreneurs' plans is generally not that they don't follow sound strategic principles but that the facts upon which they are based are wrong. Unfortunately, most of these errors cannot be detected at the whiteboard because they depend on the subtle interactions between products and customers.

If too much analysis is dangerous but none can lead to failure, how do entrepreneurs know when to stop analyzing and start building? The answer is a concept called the minimum viable product, the subject of Chapter 6.

6
TEST

Groupon is one of the fastest-growing companies of all time. Its name comes from "group coupons," an ingenious idea that has spawned an entire industry of social commerce imitators. However, it didn't start out successful. When customers took Groupon up on its first deal, a whopping twenty people bought two-for-one pizza in a restaurant on the first floor of the company's Chicago offices—hardly a world-changing event.

In fact, Groupon wasn't originally meant to be about commerce at all. The founder, Andrew Mason, intended his company to become a "collective activism platform" called The Point. Its goal was to bring people together to solve problems they couldn't solve on their own, such as fund-raising for a cause or boycotting a certain retailer. The Point's early results were disappointing, however, and at the end of 2008 the founders decided to try something new. Although they still had grand ambitions, they were determined to keep the new product simple. They built a minimum viable product. Does this sound like a billion-dollar company to you? Mason tells the story:

> We took a WordPress Blog and we skinned it to say Groupon and then every day we would do a new post. It was totally ghetto. We would sell T-shirts on the first version of Groupon. We'd say in the write-up, "This T-shirt will come in the color red, size large. If you want a different color or size. e-mail that to us." We didn't have a form to

add that stuff. It was just so cobbled together.

It was enough to prove the concept and show that it was something that people really liked. The actual coupon generation that we were doing was all FileMaker. We would run a script that would e-mail the coupon PDF to people. It got to the point where we'd sell 500 sushi coupons in a day, and we'd send 500 PDFs to people with Apple Mail at the same time. Really until July of the first year it was just a scrambling to grab the tiger by the tail. It was trying to catch up and reasonably piece together a product.[1]

Handmade PDFs, a pizza coupon, and a simple blog were enough to launch Groupon into record-breaking success; it is on pace to become the fastest company in history to achieve $1 billion in sales. It is revolutionizing the way local businesses find new customers, offering special deals to consumers in more than 375 cities worldwide.[2]

A minimum viable product (MVP) helps entrepreneurs start the process of learning as quickly as possible.[3] It is not necessarily the smallest product imaginable, though; it is simply the fastest way to get through the Build-Measure-Learn feedback loop with the minimum amount of effort.

Contrary to traditional product development, which usually involves a long, thoughtful incubation period and strives for product perfection, the goal of the MVP is to begin the process of learning, not end it. Unlike a prototype or concept test, an MVP is designed not just to answer product design or technical questions. Its goal is to test fundamental business hypotheses.

WHY FIRST PRODUCTS AREN'T MEANT TO BE PERFECT

At IMVU, when we were raising money from venture investors, we

were embarrassed. First of all, our product was still buggy and low-quality. Second, although we were proud of our business results, they weren't exactly earth-shattering. The good news was that we were on a hockey-stick-shaped growth curve. The bad news was that the hockey stick went up to only about $8,000 per month of revenue. These numbers were so low that we'd often have investors ask us, "What are the units on these charts? Are those numbers in thousands?" We'd have to reply, "No, sir, those are in ones."

However, those early results were extremely significant in predicting IMVU's future path. As you'll see in Chapter 7, we were able to validate two of our leap-of-faith assumptions: IMVU was providing value for customers, and we had a working engine of growth. The gross numbers were small because we were selling the product to visionary early customers called early adopters. Before new products can be sold successfully to the mass market, they have to be sold to early adopters. These people are a special breed of customer. They accept—in fact prefer—an 80 percent solution; you don't need a perfect solution to capture their interest.[4]

Early technology adopters lined up around the block for Apple's original iPhone even though it lacked basic features such as copy and paste, 3G Internet speed, and support for corporate e-mail. Google's original search engine could answer queries about specialized topics such as Stanford University and the Linux operating system, but it would be years before it could "organize the world's information." However, this did not stop early adopters from singing its praises.

Early adopters use their imagination to fill in what a product is missing. They prefer that state of affairs, because what they care about above all is being the first to use or adopt a new product or technology. In consumer products, it's often the thrill of being the first one on the block to show off a new basketball shoe, music player, or cool phone. In enterprise products, it's often about gaining a competitive advantage by taking a risk with something new that competitors don't have yet. Early adopters are suspicious of something that is too polished: if it's ready for everyone to adopt,

how much advantage can one get by being early? As a result, additional features or polish beyond what early adopters demand is a form of wasted resources and time.

This is a hard truth for many entrepreneurs to accept. After all, the vision entrepreneurs keep in their heads is of a high-quality mainstream product that will change the world, not one used by a small niche of people who are willing to give it a shot before it's ready. That world-changing product is polished, slick, and ready for prime time. It wins awards at trade shows and, most of all, is something you can proudly show Mom and Dad. An early, buggy, incomplete product feels like an unacceptable compromise. How many of us were raised with the expectation that we would put our best work forward? As one manager put it to me recently, "I know for me, the MVP feels a little dangerous—in a good way—since I have always been such a perfectionist."

Minimum viable products range in complexity from extremely simple smoke tests (little more than an advertisement) to actual early prototypes complete with problems and missing features. Deciding exactly how complex an MVP needs to be cannot be done formulaically. It requires judgment. Luckily, this judgment is not difficult to develop: most entrepreneurs and product development people dramatically overestimate how many features are needed in an MVP. When in doubt, simplify.

For example, consider a service sold with a one-month free trial. Before a customer can use the service, he or she has to sign up for the trial. One obvious assumption, then, of the business model is that customers will sign up for a free trial once they have a certain amount of information about the service. A critical question to consider is whether customers will in fact sign up for the free trial given a certain number of promised features (the value hypothesis).

Somewhere in the business model, probably buried in a single cell in a spreadsheet, it specifies the "percentage of customers who see the free trial offer who then sign up." Maybe in our projections we say that this number should be 10 percent. If you think about it, this is a leap-of-faith question. It really should be represented in giant letters in a bold red font: WE ASSUME 10 PERCENT OF CUSTOMERS WILL SIGN UP.

Most entrepreneurs approach a question like this by building the product and then checking to see how customers react to it. I consider this to be exactly backward because it can lead to a lot of waste. First, if it turns out that we're building something nobody wants, the whole exercise will be an avoidable expense of time and money. If customers won't sign up for the free trial, they'll never get to experience the amazing features that await them. Even if they do sign up, there are many other opportunities for waste. For example, how many features do we really need to include to appeal to early adopters? Every extra feature is a form of waste, and if we delay the test for these extra features, it comes with a tremendous potential cost in terms of learning and cycle time.

The lesson of the MVP is that any additional work beyond what was required to start learning is waste, no matter how important it might have seemed at the time.

To demonstrate, I'll share several MVP techniques from actual Lean Startups. In each case, you'll witness entrepreneurs avoiding the temptation to overbuild and overpromise.

THE VIDEO MINIMUM VIABLE PRODUCT

Drew Houston is the CEO of Dropbox, a Silicon Valley company that makes an extremely easy-to-use file-sharing tool. Install its application, and a Dropbox folder appears on your computer desktop. Anything you drag into that folder is uploaded automatically to the Dropbox service and then instantly replicated across all your computers and devices.

The founding team was made up of engineers, as the product demanded significant technical expertise to build. It required, for example, integration with a variety of computer platforms and operating systems: Windows, Macintosh, iPhone, Android, and so on. Each of these implementations happens at a deep level of the system and requires specialized know-how to make the user experience exceptional. In fact, one of Dropbox's biggest competitive advantages is that the product works in such a seamless

way that the competition struggles to emulate it.

These are not the kind of people one would think of as marketing geniuses. In fact, none of them had ever worked in a marketing job. They had prominent venture capital backers and could have been expected to apply the standard engineering thinking to building the business: build it and they will come. But Dropbox did something different.

In parallel with their product development efforts, the founders wanted feedback from customers about what really mattered to them. In particular, Dropbox needed to test its leap-of-faith question: if we can provide a superior customer experience, will people give our product a try? They believed—rightly, as it turned out—that file synchronization was a problem that most people didn't know they had. Once you experience the solution, you can't imagine how you ever lived without it.

This is not the kind of entrepreneurial question you can ask or expect an answer to in a focus group. Customers often don't know what they want, and they often had a hard time understanding Dropbox when the concept was explained. Houston learned this the hard way when he tried to raise venture capital. In meeting after meeting, investors would explain that this "market space" was crowded with existing products, none of them had made very much money, and the problem wasn't a very important one. Drew would ask: "Have you personally tried those other products?" When they would say yes, he'd ask: "Did they work seamlessly for you?" The answer was almost always no. Yet in meeting after meeting, the venture capitalists could not imagine a world in line with Drew's vision. Drew, in contrast, believed that if the software "just worked like magic," customers would flock to it.

The challenge was that it was impossible to demonstrate the working software in a prototype form. The product required that they overcome significant technical hurdles; it also had an online service component that required high reliability and availability. To avoid the risk of waking up after years of development with a product nobody wanted, Drew did something unexpectedly easy: he made a video.

The video is banal, a simple three-minute demonstration of the technology as it is meant to work, but it was targeted at a community of technology early adopters. Drew narrates the video personally, and as he's narrating, the viewer is watching his screen. As he describes the kinds of files he'd like to synchronize, the viewer can watch his mouse manipulate his computer. Of course, if you're paying attention, you start to notice that the files he's moving around are full of in-jokes and humorous references that were appreciated by this community of early adopters. Drew recounted, "It drove hundreds of thousands of people to the website. Our beta waiting list went from 5,000 people to 75,000 people literally overnight. It totally blew us away." Today, Dropbox is one of Silicon Valley's hottest companies, rumored to be worth more than $1 billion.[5]

In this case, the video was the minimum viable product. The MVP validated Drew's leap-of-faith assumption that customers wanted the product he was developing not because they said so in a focus group or because of a hopeful analogy to another business, but because they actually signed up.

THE CONCIERGE MINIMUM VIABLE PRODUCT

Consider another kind of MVP technique: the concierge MVP. To understand how this technique works, meet Manuel Rosso, the CEO of an Austin, Texas–based startup called Food on the Table. Food on the Table creates weekly meal plans and grocery lists that are based on food you and your family enjoy, then hooks into your local grocery stores to find the best deals on the ingredients.

After you sign up for the site, you walk through a little setup in which you identify your main grocery store and check off the foods your family likes. Later, you can pick another nearby store if you want to compare prices. Next, you're presented with a list of items that are based on your preferences and asked: "What are you in the mood for this week?" Make your choices, select the number of meals you're ready to plan, and choose what you care about most

in terms of time, money, health, or variety. At this point, the site searches through recipes that match your needs, prices out the cost of the meal for you, and lets you print out your shopping list.[6]

Clearly, this is an elaborate service. Behind the scenes, a team of professional chefs devise recipes that take advantage of items that are on sale at local grocery stores around the country. Those recipes are matched via computer algorithm to each family's unique needs and preferences. Try to visualize the work involved: databases of almost every grocery store in the country must be maintained, including what's on sale at each one this week. Those groceries have to be matched to appropriate recipes and then appropriately customized, tagged, and sorted. If a recipe calls for broccoli rabe, is that the same ingredient as the broccoli on sale at the local market?

After reading that description, you might be surprised to learn that Food on the Table (FotT) began life with a single customer. Instead of supporting thousands of grocery stores around the country as it does today, FotT supported just one. How did the company choose which store to support? The founders didn't—until they had their first customer. Similarly, they began life with no recipes whatsoever—until their first customer was ready to begin her meal planning. In fact, the company served its first customer without building any software, without signing any business development partnerships, and without hiring any chefs.

Manuel, along with VP of product Steve Sanderson, went to local supermarkets and moms' groups in his hometown of Austin. Part of their mission was the typical observation of customers that is a part of design thinking and other ideation techniques. However, Manuel and his team were also on the hunt for something else: their first customer.

As they met potential customers in those settings, they would interview them the way any good market researcher would, but at the end of each interview they would attempt to make a sale. They'd describe the benefits of FotT, name a weekly subscription fee, and invite the customer to sign up. Most times they were rejected. After all, most people are not early adopters and will not

sign up for a new service sight unseen. But eventually someone did.

That one early adopter got the concierge treatment. Instead of interacting with the FotT product via impersonal software, she got a personal visit each week from the CEO of the company. He and the VP of product would review what was on sale at her preferred grocery store and carefully select recipes on the basis of her preferences, going so far as to learn her favorite recipes for items she regularly cooked for her family. Each week they would hand her—in person—a prepared packet containing a shopping list and relevant recipes, solicit her feedback, and make changes as necessary. Most important, each week they would collect a check for $9.95.

Talk about inefficient! Measured according to traditional criteria, this is a terrible system, entirely nonscalable and a complete waste of time. The CEO and VP of product, instead of building their business, are engaged in the drudgery of solving just one customer's problem. Instead of marketing themselves to millions, they sold themselves to one. Worst of all, their efforts didn't appear to be leading to anything tangible. They had no product, no meaningful revenue, no databases of recipes, not even a lasting organization.

However, viewed through the lens of the Lean Startup, they were making monumental progress. Each week they were learning more and more about what was required to make their product a success. After a few weeks they were ready for another customer. Each customer they brought on made it easier to get the next one, because FotT could focus on the same grocery store, getting to know its products and the kinds of people who shopped there well. Each new customer got the concierge treatment: personal in-home visits, the works. But after a few more customers, the overhead of serving them one-on-one started to increase.

Only at the point where the founders were too busy to bring on additional customers did Manuel and his team start to invest in automation in the form of product development. Each iteration of their minimum viable product allowed them to save a little more time and serve a few more customers: delivering the recipes and shopping list via e-mail instead of via an in-home visit, starting to

parse lists of what was on sale automatically via software instead of by hand, even eventually taking credit card payments online instead of a handwritten check.

Before long, they had built a substantial service offering, first in the Austin area and eventually nationwide. But along the way, their product development team was always focused on scaling something that was working rather than trying to invent something that might work in the future. As a result, their development efforts involved far less waste than is typical for a venture of this kind.

It is important to contrast this with the case of a small business, in which it is routine to see the CEO, founder, president, and owner serving customers directly, one at a time. In a concierge MVP, this personalized service is not the product but a learning activity designed to test the leap-of-faith assumptions in the company's growth model. In fact, a common outcome of a concierge MVP is to invalidate the company's proposed growth model, making it clear that a different approach is needed. This can happen even if the initial MVP is profitable for the company. Without a formal growth model, many companies get caught in the trap of being satisfied with a small profitable business when a pivot (change in course or strategy) might lead to more significant growth. The only way to know is to have tested the growth model systematically with real customers.

PAY NO ATTENTION TO THE EIGHT PEOPLE BEHIND THE CURTAIN

Meet Max Ventilla and Damon Horowitz, technologists with a vision to build a new type of search software designed to answer the kinds of questions that befuddle state-of-the-art companies such as Google. Google befuddled? Think about it. Google and its peers excel at answering factual questions: What is the tallest mountain in the world? Who was the twenty-third president of the United States? But for more subjective questions, Google struggles. Ask, "What's a good place to go out for a drink after the ball game in my

city?" and the technology flails. What's interesting about this class of queries is that they are relatively easy for a person to answer. Imagine being at a cocktail party surrounded by friends. How likely would you be to get a high-quality answer to your subjective question? You almost certainly would get one. Unlike factual queries, because these subjective questions have no single right answer, today's technology struggles to answer them. Such questions depend on the person answering them, his or her personal experience, taste, and assessment of what you're looking for.

To solve this problem, Max and Damon created a product called Aardvark. With their deep technical knowledge and industry experience, it would have been reasonable to expect them to dive in and start programming. Instead, they took six months to figure out what they should be building. But they didn't spend that year at the whiteboard strategizing or engage in a lengthy market research project.

Instead, they built a series of functioning products, each designed to test a way of solving this problem for their customers. Each product was then offered to beta testers, whose behavior was used to validate or refute each specific hypothesis (see examples in sidebar).

The following list of projects are examples from Aardvark's ideation period.[7]

Rekkit. A service to collect your ratings from across the web and give better recommendations to you.

Ninjapa. A way that you could open accounts in various applications through a single website and manage your data across multiple sites.

The Webb. A central number that you could call and talk to a person who could do anything for you that you could do online.

Web Macros. A way to record sequences of steps on websites so that you could repeat common actions, even across sites, and share "recipes" for how you accomplished online tasks.

Internet Button Company. A way to package steps taken on a website and smart form-fill functionality. People could encode buttons and share buttons à la social bookmarking.

Max and Damon had a vision that computers could be used to create a virtual personal assistant to which their customers could ask questions. Because the assistant was designed for subjective questions, the answers required human judgment. Thus, the early Aardvark experiments tried many variations on this theme, building a series of prototypes for ways customers could interact with the virtual assistant and get their questions answered. All the early prototypes failed to engage the customers.

As Max describes it, "We self-funded the company and released very cheap prototypes to test. What became Aardvark was the sixth prototype. Each prototype was a two- to four-week effort. We used humans to replicate the back end as much as possible. We invited one hundred to two hundred friends to try the prototypes and measured how many of them came back. The results were unambiguously negative until Aardvark."

Because of the short time line, none of the prototypes involved advanced technology. Instead, they were MVPs designed to test a more important question: what would be required to get customers to engage with the product and tell their friends about it?

"Once we chose Aardvark," Ventilla says, "we continued to run

with humans replicating pieces of the backend for nine months. We hired eight people to manage queries, classify conversations, etc. We actually raised our seed and series A rounds before the system was automated—the assumption was that the lines between humans and artificial intelligence would cross, and we at least proved that we were building stuff people would respond to.

"As we refined the product, we would bring in six to twelve people weekly to react to mockups, prototypes, or simulations that we were working on. It was a mix of existing users and people who never saw the product before. We had our engineers join for many of these sessions, both so that they could make modifications in real time, but also so we could all experience the pain of a user not knowing what to do."[8]

The Aardvark product they settled on worked via instant messaging (IM). Customers could send Aardvark a question via IM, and Aardvark would get them an answer that was drawn from the customer's social network: the system would seek out the customer's friends and friends of friends and pose the question to them. Once it got a suitable answer, it would report back to the initial customer.

Of course, a product like that requires a very important algorithm: given a question about a certain topic, who is the best person in the customer's social network to answer that question? For example, a question about restaurants in San Francisco shouldn't be routed to someone in Seattle. More challenging still, a question about computer programming probably shouldn't be routed to an art student.

Throughout their testing process, Max and Damon encountered many difficult technological problems like these. Each time, they emphatically refused to solve them at that early stage. Instead, they used Wizard of Oz testing to fake it. In a Wizard of Oz test, customers believe they are interacting with the actual product, but behind the scenes human beings are doing the work. Like the concierge MVP, this approach is incredibly inefficient. Imagine a service that allowed customers to ask questions of human

researchers—for free—and expect a real-time response. Such a service (at scale) would lose money, but it is easy to build on a micro scale. At that scale, it allowed Max and Damon to answer these all-important questions: If we can solve the tough technical problems behind this artificial intelligence product, will people use it? Will their use lead to the creation of a product that has real value?

It was this system that allowed Max and Damon to pivot over and over again, rejecting concepts that seemed promising but that would not have been viable. When they were ready to start scaling, they had a ready-made road map of what to build. The result: Aardvark was acquired for a reported $50 million—by Google.[9]

THE ROLE OF QUALITY AND DESIGN IN AN MVP

One of the most vexing aspects of the minimum viable product is the challenge it poses to traditional notions of quality. The best professionals and craftspersons alike aspire to build quality products; it is a point of pride.

Modern production processes rely on high quality as a way to boost efficiency. They operate using W. Edwards Deming's famous dictum that the customer is the most important part of the production process. This means that we must focus our energies exclusively on producing outcomes that the customer perceives as valuable. Allowing sloppy work into our process inevitably leads to excessive variation. Variation in process yields products of varying quality in the eyes of the customer that at best require rework and at worst lead to a lost customer. Most modern business and engineering philosophies focus on producing high-quality experiences for customers as a primary principle; it is the foundation of Six Sigma, lean manufacturing, design thinking, extreme programming, and the software craftsmanship movement.

These discussions of quality presuppose that the company already knows what attributes of the product the customer will perceive as worthwhile. In a startup, this is a risky assumption to make. Often

we are not even sure who the customer is. Thus, for startups, I believe in the following quality principle:

> If we do not know who the customer is, we do not know what quality is.

Even a "low-quality" MVP can act in service of building a great high-quality product. Yes, MVPs sometimes are perceived as low-quality by customers. If so, we should use this as an opportunity to learn what attributes customers care about. This is infinitely better than mere speculation or whiteboard strategizing, because it provides a solid empirical foundation on which to build future products.

Sometimes, however, customers react quite differently. Many famous products were released in a "low-quality" state, and customers loved them. Imagine if Craig Newmark, in the early days of Craigslist, had refused to publish his humble e-mail newsletter because it lacked sufficient high design. What if the founders of Groupon had felt "two pizzas for the price of one" was beneath them?

I have had many similar experiences. In the early days of IMVU, our avatars were locked in one place, unable to move around the screen. The reason? We were building an MVP and had not yet tackled the difficult task of creating the technology that would allow avatars to walk around the virtual environments they inhabit. In the video game industry, the standard is that 3D avatars should move fluidly as they walk, avoid obstacles in their path, and take an intelligent route toward their destination. Famous best-selling games such as Electronic Arts' The Sims work on this principle. We didn't want to ship a low-quality version of this feature, so we opted instead to ship with stationary avatars.

Feedback from the customers was very consistent: they wanted the ability to move their avatars around the environment. We took this as bad news because it meant we would have to spend considerable amounts of time and money on a high-quality solution similar to The Sims. But before we committed ourselves to that

path, we decided to try another MVP. We used a simple hack, which felt almost like cheating. We changed the product so that customers could click where they wanted their avatar to go, and the avatar would teleport there instantly. No walking, no obstacle avoidance. The avatar disappeared and then reappeared an instant later in the new place. We couldn't even afford fancy teleportation graphics or sound effects. We felt lame shipping this feature, but it was all we could afford.

You can imagine our surprise when we started to get positive customer feedback. We never asked about the movement feature directly (we were too embarrassed). But when asked to name the top things about IMVU they liked best, customers consistently listed avatar "teleportation" among the top three (unbelievably, they often specifically described it as "more advanced than The Sims"). This inexpensive compromise outperformed many features of the product we were most proud of, features that had taken much more time and money to produce.

Customers don't care how much time something takes to build. They care only if it serves their needs. Our customers preferred the quick teleportation feature because it allowed them to get where they wanted to go as fast as possible. In retrospect, this makes sense. Wouldn't we all like to get wherever we're going in an instant? No lines, no hours on a plane or sitting on the tarmac, no connections, no cabs or subways. Beam me up, Scotty. Our expensive "real-world" approach was beaten handily by a cool fantasy-world feature that cost much less but that our customers preferred.

So which version of the product is low-quality, again?

MVPs require the courage to put one's assumptions to the test. If customers react the way we expect, we can take that as confirmation that our assumptions are correct. If we release a poorly designed product and customers (even early adopters) cannot figure out how to use it, that will confirm our need to invest in superior design. But we must always ask: what if they don't care about design in the same way we do?

Thus, the Lean Startup method is not opposed to building high-

quality products, but only in service of the goal of winning over customers. We must be willing to set aside our traditional professional standards to start the process of validated learning as soon as possible. But once again, this does not mean operating in a sloppy or undisciplined way. (This is an important caveat. There is a category of quality problems that have the net effect of slowing down the Build-Measure-Learn feedback loop. Defects make it more difficult to evolve the product. They actually interfere with our ability to learn and so are dangerous to tolerate in any production process. We will consider methods for figuring out when to make investments in preventing these kinds of problems in Part Three.)

As you consider building your own minimum viable product, let this simple rule suffice: remove any feature, process, or effort that does not contribute directly to the learning you seek.

SPEED BUMPS IN BUILDING AN MVP

Building an MVP is not without risks, both real and imagined. Both can derail a startup effort unless they are understood ahead of time. The most common speed bumps are legal issues, fears about competitors, branding risks, and the impact on morale.

For startups that rely on patent protection, there are special challenges with releasing an early product. In some jurisdictions, the window for filing a patent begins when the product is released to the general public, and depending on the way the MVP is structured, releasing it may start this clock. Even if your startup is not in one of those jurisdictions, you may want international patent protection and may wind up having to abide by these more stringent requirements. (In my opinion, issues like this are one of the many ways in which current patent law inhibits innovation and should be remedied as a matter of public policy.)

In many industries, patents are used primarily for defensive purposes, as a deterrent to hold competitors at bay. In such cases, the patent risks of an MVP are minor compared with the learning benefits. However. in industries in which a new scientific

breakthrough is at the heart of a company's competitive advantage, these risks need to be balanced more carefully. In all cases, entrepreneurs should seek legal counsel to ensure that they understand the risks fully.

Legal risks may be daunting, but you may be surprised to learn that the most common objection I have heard over the years to building an MVP is fear of competitors—especially large established companies—stealing a startup's ideas. If only it were so easy to have a good idea stolen! Part of the special challenge of being a startup is the near impossibility of having your idea, company, or product be noticed by anyone, let alone a competitor. In fact, I have often given entrepreneurs fearful of this issue the following assignment: take one of your ideas (one of your lesser insights, perhaps), find the name of the relevant product manager at an established company who has responsibility for that area, and try to get that company to steal your idea. Call them up, write them a memo, send them a press release—go ahead, try it. The truth is that most managers in most companies are already overwhelmed with good ideas. Their challenge lies in prioritization and execution, and it is those challenges that give a startup hope of surviving.[10]

If a competitor can outexecute a startup once the idea is known, the startup is doomed anyway. The reason to build a new team to pursue an idea is that you believe you can accelerate through the Build-Measure-Learn feedback loop faster than anyone else can. If that's true, it makes no difference what the competition knows. If it's not true, a startup has much bigger problems, and secrecy won't fix them. Sooner or later, a successful startup will face competition from fast followers. A head start is rarely large enough to matter, and time spent in stealth mode—away from customers—is unlikely to provide a head start. The only way to win is to learn faster than anyone else.

Many startups plan to invest in building a great brand, and an MVP can seem like a dangerous branding risk. Similarly, entrepreneurs in existing organizations often are constrained by the fear of damaging the parent company's established brand. In either

of these cases, there is an easy solution: launch the MVP under a different brand name. In addition, a long-term reputation is only at risk when companies engage in vocal launch activities such as PR and building hype. When a product fails to live up to those pronouncements, real long-term damage can happen to a corporate brand. But startups have the advantage of being obscure, having a pathetically small number of customers, and not having much exposure. Rather than lamenting them, use these advantages to experiment under the radar and then do a public marketing launch once the product has proved itself with real customers.[11]

Finally, it helps to prepare for the fact that MVPs often result in bad news. Unlike traditional concept tests or prototypes, they are designed to speak to the full range of business questions, not just design or technical ones, and they often provide a needed dose of reality. In fact, piercing the reality distortion field is quite uncomfortable. Visionaries are especially afraid of a false negative: that customers will reject a flawed MVP that is too small or too limited. It is precisely this attitude that one sees when companies launch fully formed products without prior testing. They simply couldn't bear to test them in anything less than their full splendor. Yet there is wisdom in the visionary's fear. Teams steeped in traditional product development methods are trained to make go/kill decisions on a regular basis. That is the essence of the waterfall or stage-gate development model. If an MVP fails, teams are liable to give up hope and abandon the project altogether. But this is a solvable problem.

FROM THE MVP TO INNOVATION ACCOUNTING

The solution to this dilemma is a commitment to iteration. You have to commit to a locked-in agreement—ahead of time—that no matter what comes of testing the MVP, you will not give up hope. Successful entrepreneurs do not give up at the first sign of trouble, nor do they persevere the plane right into the ground. Instead, they possess a unique combination of perseverance and flexibility. The

MVP is just the first step on a journey of learning. Down that road—after many iterations—you may learn that some element of your product or strategy is flawed and decide it is time to make a change, which I call a pivot, to a different method for achieving your vision.

Startups are especially at risk when outside stakeholders and investors (especially corporate CFOs for internal projects) have a crisis of confidence. When the project was authorized or the investment made, the entrepreneur promised that the new product would be world-changing. Customers were supposed to flock to it in record numbers. Why are so few actually doing so?

In traditional management, a manager who promises to deliver something and fails to do so is in trouble. There are only two possible explanations: a failure of execution or a failure to plan appropriately. Both are equally inexcusable. Entrepreneurial managers face a difficult problem: because the plans and projections we make are full of uncertainty, how can we claim success when we inevitably fail to deliver what we promised? Put another way, how does the CFO or VC know that we're failing because we learned something critical and not because we were goofing off or misguided?

The solution to this problem resides at the heart of the Lean Startup model. We all need a disciplined, systematic approach to figuring out if we're making progress and discovering if we're actually achieving validated learning. I call this system innovation accounting, an alternative to traditional accounting designed specifically for startups. It is the subject of Chapter 7.

7
MEASURE

At the beginning, a startup is little more than a model on a piece of paper. The financials in the business plan include projections of how many customers the company expects to attract, how much it will spend, and how much revenue and profit that will lead to. It's an ideal that's usually far from where the startup is in its early days.

A startup's job is to (1) rigorously measure where it is right now, confronting the hard truths that assessment reveals, and then (2) devise experiments to learn how to move the real numbers closer to the ideal reflected in the business plan.

Most products—even the ones that fail—do not have zero traction. Most products have some customers, some growth, and some positive results. One of the most dangerous outcomes for a startup is to bumble along in the land of the living dead. Employees and entrepreneurs tend to be optimistic by nature. We want to keep believing in our ideas even when the writing is on the wall. This is why the myth of perseverance is so dangerous. We all know stories of epic entrepreneurs who managed to pull out a victory when things seemed incredibly bleak. Unfortunately, we don't hear stories about the countless nameless others who persevered too long, leading their companies to failure.

WHY SOMETHING AS SEEMINGLY DULL AS ACCOUNTING WILL CHANGE YOUR LIFE

People are accustomed to thinking of accounting as dry and boring, a necessary evil used primarily to prepare financial reports and survive audits, but that is because accounting is something that has become taken for granted. Historically, under the leadership of people such as Alfred Sloan at General Motors, accounting became an essential part of the method of exerting centralized control over far-flung divisions. Accounting allowed GM to set clear milestones for each of its divisions and then hold each manager accountable for his or her division's success in reaching those goals. All modern corporations use some variation of that approach. Accounting is the key to their success.

Unfortunately, standard accounting is not helpful in evaluating entrepreneurs. Startups are too unpredictable for forecasts and milestones to be accurate.

I recently met with a phenomenal startup team. They are well financed, have significant customer traction, and are growing rapidly. Their product is a leader in an emerging category of enterprise software that uses consumer marketing techniques to sell into large companies. For example, they rely on employee-to-employee viral adoption rather than a traditional sales process, which might target the chief information officer or the head of information technology (IT). As a result, they have the opportunity to use cutting-edge experimental techniques as they constantly revise their product. During the meeting, I asked the team a simple question that I make a habit of asking startups whenever we meet: are you making your product better? They always say yes. Then I ask: how do you know? I invariably get this answer: well, we are in engineering and we made a number of changes last month, and our customers seem to like them, and our overall numbers are higher this month. We must be on the right track.

This is the kind of storytelling that takes place at most startup board meetings. Most milestones are built the same way: hit a certain product milestone, maybe talk to a few customers, and see if the numbers go up. Unfortunately, this is not a good indicator of whether a startup is making progress. How do we know that the changes we've made are related to the results we're seeing? More

important, how do we know that we are drawing the right lessons from those changes?

To answer these kinds of questions, startups have a strong need for a new kind of accounting geared specifically to disruptive innovation. That's what innovation accounting is.

An Accountability Framework That Works Across Industries

Innovation accounting enables startups to prove objectively that they are learning how to grow a sustainable business. Innovation accounting begins by turning the leap-of-faith assumptions discussed in Chapter 5 into a quantitative financial model. Every business plan has some kind of model associated with it, even if it's written on the back of a napkin. That model provides assumptions about what the business will look like at a successful point in the future.

For example, the business plan for an established manufacturing company would show it growing in proportion to its sales volume. As the profits from the sales of goods are reinvested in marketing and promotions, the company gains new customers. The rate of growth depends primarily on three things: the profitability of each customer, the cost of acquiring new customers, and the repeat purchase rate of existing customers. The higher these values are, the faster the company will grow and the more profitable it will be. These are the drivers of the company's growth model.

By contrast, a marketplace company that matches buyers and sellers such as eBay will have a different growth model. Its success depends primarily on the network effects that make it the premier destination for both buyers and sellers to transact business. Sellers want the marketplace with the highest number of potential customers. Buyers want the marketplace with the most competition among sellers, which leads to the greatest availability of products and the lowest prices. (In economics, this sometimes is called supply-side increasing returns and demand-side increasing returns.) For this kind of startup, the important thing to measure is that the network effects are working, as evidenced by the high retention rate

of new buyers and sellers. If people stick with the product with very little attrition, the marketplace will grow no matter how the company acquires new customers. The growth curve will look like a compounding interest table, with the rate of growth depending on the "interest rate" of new customers coming to the product.

Though these two businesses have very different drivers of growth, we can still use a common framework to hold their leaders accountable. This framework supports accountability even when the model changes.

HOW INNOVATION ACCOUNTING WORKS—THREE LEARNING MILESTONES

Innovation accounting works in three steps: first, use a minimum viable product to establish real data on where the company is right now. Without a clear-eyed picture of your current status—no matter how far from the goal you may be—you cannot begin to track your progress.

Second, startups must attempt to tune the engine from the baseline toward the ideal. This may take many attempts. After the startup has made all the micro changes and product optimizations it can to move its baseline toward the ideal, the company reaches a decision point. That is the third step: pivot or persevere.

If the company is making good progress toward the ideal, that means it's learning appropriately and using that learning effectively, in which case it makes sense to continue. If not, the management team eventually must conclude that its current product strategy is flawed and needs a serious change. When a company pivots, it starts the process all over again, reestablishing a new baseline and then tuning the engine from there. The sign of a successful pivot is that these engine-tuning activities are more productive after the pivot than before.

Establish the Baseline

For example, a startup might create a complete prototype of its product and offer to sell it to real customers through its main marketing channel. This single MVP would test most of the startup's assumptions and establish baseline metrics for each assumption simultaneously. Alternatively, a startup might prefer to build separate MVPs that are aimed at getting feedback on one assumption at a time. Before building the prototype, the company might perform a smoke test with its marketing materials. This is an old direct marketing technique in which customers are given the opportunity to preorder a product that has not yet been built. A smoke test measures only one thing: whether customers are interested in trying a product. By itself, this is insufficient to validate an entire growth model. Nonetheless, it can be very useful to get feedback on this assumption before committing more money and other resources to the product.

These MVPs provide the first example of a learning milestone. An MVP allows a startup to fill in real baseline data in its growth model—conversion rates, sign-up and trial rates, customer lifetime value, and so on—and this is valuable as the foundation for learning about customers and their reactions to a product even if that foundation begins with extremely bad news.

When one is choosing among the many assumptions in a business plan, it makes sense to test the riskiest assumptions first. If you can't find a way to mitigate these risks toward the ideal that is required for a sustainable business, there is no point in testing the others. For example, a media business that is selling advertising has two basic assumptions that take the form of questions: Can it capture the attention of a defined customer segment on an ongoing basis? and can it sell that attention to advertisers? In a business in which the advertising rates for a particular customer segment are well known, the far riskier assumption is the ability to capture attention. Therefore, the first experiments should involve content production rather than advertising sales. Perhaps the company will produce a pilot episode or issue to see how customers engage.

Tuning the Engine

Once the baseline has been established, the startup can work toward the second learning milestone: tuning the engine. Every product development, marketing, or other initiative that a startup undertakes should be targeted at improving one of the drivers of its growth model. For example, a company might spend time improving the design of its product to make it easier for new customers to use. This presupposes that the activation rate of new customers is a driver of growth and that its baseline is lower than the company would like. To demonstrate validated learning, the design changes must improve the activation rate of new customers. If they do not, the new design should be judged a failure. This is an important rule: a good design is one that changes customer behavior for the better.

Compare two startups. The first company sets out with a clear baseline metric, a hypothesis about what will improve that metric, and a set of experiments designed to test that hypothesis. The second team sits around debating what would improve the product, implements several of those changes at once, and celebrates if there is any positive increase in any of the numbers. Which startup is more likely to be doing effective work and achieving lasting results?

Pivot or Persevere

Over time, a team that is learning its way toward a sustainable business will see the numbers in its model rise from the horrible baseline established by the MVP and converge to something like the ideal one established in the business plan. A startup that fails to do so will see that ideal recede ever farther into the distance. When this is done right, even the most powerful reality distortion field won't be able to cover up this simple fact: if we're not moving the drivers of our business model, we're not making progress. That becomes a sure sign that it's time to pivot.

INNOVATION ACCOUNTING AT IMVU

Here's what innovation accounting looked like for us in the early days of IMVU. Our minimum viable product had many defects and, when we first released it, extremely low sales. We naturally assumed that the lack of sales was related to the low quality of the product, so week after week we worked on improving the quality of the product, trusting that our efforts were worthwhile. At the end of each month, we would have a board meeting at which we would present the results. The night before the board meeting, we'd run our standard analytics, measuring conversion rates, customer counts, and revenue to show what a good job we had done. For several meetings in a row, this caused a last-minute panic because the quality improvements were not yielding any change in customer behavior. This led to some frustrating board meetings at which we could show great product "progress" but not much in the way of business results. After a while, rather than leave it to the last minute, we began to track our metrics more frequently, tightening the feedback loop with product development. This was even more depressing. Week in, week out, our product changes were having no effect.

Improving a Product on Five Dollars a Day

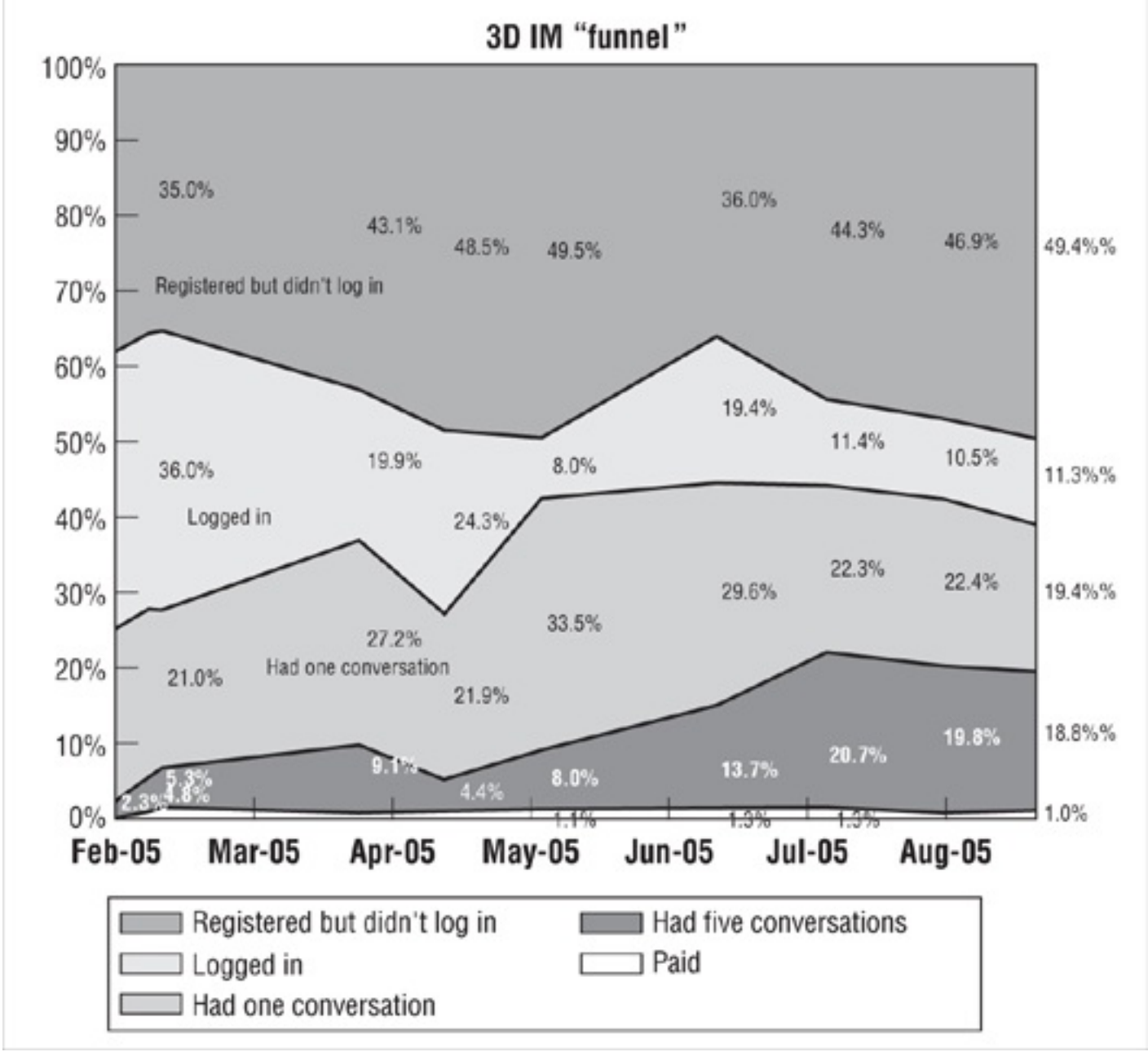
We tracked the "funnel metrics" behaviors that were critical to our engine of growth: customer registration, the download of our application, trial, repeat usage, and purchase. To have enough data to learn, we needed just enough customers using our product to get real numbers for each behavior. We allocated a budget of five dollars per day: enough to buy clicks on the then-new Google AdWords system. In those days, the minimum you could bid for a click was 5 cents, but there was no overall minimum to your spending. Thus, we could afford to open an account and get started even though we had very little money.[1]

Five dollars bought us a hundred clicks—every day. From a marketing point of view this was not very significant, but for learning it was priceless. Every single day we were able to measure our product's performance with a brand new set of customers. Also, each time we revised the product, we got a brand new report card on how we were doing the very next day.

For example, one day we would debut a new marketing message aimed at first-time customers. The next day we might change the way new customers were initiated into the product. Other days, we would add new features, fix bugs, roll out a new visual design, or try a new layout for our website. Every time, we told ourselves we were making the product better, but that subjective confidence was put to the acid test of real numbers.

Day in and day out we were performing random trials. Each day was a new experiment. Each day's customers were independent of those of the day before. Most important, even though our gross numbers were growing, it became clear that our funnel metrics were not changing.

Here is a graph from one of IMVU's early board meetings:



This graph represents approximately seven months of work. Over that period, we were making constant improvements to the IMVU product, releasing new features on a daily basis. We were conducting a lot of in-person customer interviews, and our product development team was working extremely hard.

Cohort Analysis

To read the graph, you need to understand something called cohort analysis. This is one of the most important tools of startup analytics. Although it sounds complex, it is based on a simple premise.

Instead of looking at cumulative totals or gross numbers such as total revenue and total number of customers, one looks at the performance of each group of customers that comes into contact with the product independently. Each group is called a cohort. The graph shows the conversion rates to IMVU of new customers who joined in each indicated month. Each conversion rate shows the percentage of customer who registered in that month who subsequently went on to take the indicated action. Thus, among all the customers who joined IMVU in February 2005, about 60 percent of them logged in to our product at least one time.

Managers with an enterprise sales background will recognize this funnel analysis as the traditional sales funnel that is used to manage prospects on their way to becoming customers. Lean Startups use it in product development, too. This technique is useful in many types of business, because every company depends for its survival on sequences of customer behavior called flows. Customer flows govern the interaction of customers with a company's products. They allow us to understand a business quantitatively and have much more predictive power than do traditional gross metrics.

If you look closely, you'll see that the graph shows some clear trends. Some product improvements are helping—a little. The percentage of new customers who go on to use the product at least five times has grown from less than 5 percent to almost 20 percent. Yet despite this fourfold increase, the percentage of new customers who pay money for IMVU is stuck at around 1 percent. Think about that for a moment. After months and months of work, thousands of individual improvements, focus groups, design sessions, and usability tests, the percentage of new customers who subsequently pay money is exactly the same as it was at the onset even though many more customers are getting a chance to try the product.

Thanks to the power of cohort analysis, we could not blame this failure on the legacy of previous customers who were resistant to change, external market conditions, or any other excuse. Each cohort represented an independent report card, and try as we might, we were getting straight C's. This helped us realize we had a

problem.

I was in charge of the product development team, small though it was in those days, and shared with my cofounders the sense that the problem had to be with my team's efforts. I worked harder, tried to focus on higher- and higher-quality features, and lost a lot of sleep. Our frustration grew. When I could think of nothing else to do, I was finally ready to turn to the last resort: talking to customers. Armed with our failure to make progress tuning our engine of growth, I was ready to ask the right questions.

Before this failure, in the company's earliest days, it was easy to talk to potential customers and come away convinced we were on the right track. In fact, when we would invite customers into the office for in-person interviews and usability tests, it was easy to dismiss negative feedback. If they didn't want to use the product, I assumed they were not in our target market. "Fire that customer," I'd say to the person responsible for recruiting for our tests. "Find me someone in our target demographic." If the next customer was more positive, I would take it as confirmation that I was right in my targeting. If not, I'd fire another customer and try again.

By contrast, once I had data in hand, my interactions with customers changed. Suddenly I had urgent questions that needed answering: Why aren't customers responding to our product "improvements"? Why isn't our hard work paying off? For example, we kept making it easier and easier for customers to use IMVU with their existing friends. Unfortunately, customers didn't want to engage in that behavior. Making it easier to use was totally beside the point. Once we knew what to look for, genuine understanding came much faster. As was described in Chapter 3, this eventually led to a critically important pivot: away from an IM add-on used with existing friends and toward a stand-alone network one can use to make new friends. Suddenly, our worries about productivity vanished. Once our efforts were aligned with what customers really wanted, our experiments were much more likely to change their behavior for the better.

This pattern would repeat time and again, from the days when we were making less than a thousand dollars in revenue per month

all the way up to the time we were making millions. In fact, this is the sign of a successful pivot: the new experiments you run are overall more productive than the experiments you were running before.

This is the pattern: poor quantitative results force us to declare failure and create the motivation, context, and space for more qualitative research. These investigations produce new ideas—new hypotheses—to be tested, leading to a possible pivot. Each pivot unlocks new opportunities for further experimentation, and the cycle repeats. Each time we repeat this simple rhythm: establish the baseline, tune the engine, and make a decision to pivot or persevere.

OPTIMIZATION VERSUS LEARNING

Engineers, designers, and marketers are all skilled at optimization. For example, direct marketers are experienced at split testing value propositions by sending a different offer to two similar groups of customers so that they can measure differences in the response rates of the two groups. Engineers, of course, are skilled at improving a product's performance, just as designers are talented at making products easier to use. All these activities in a well-run traditional organization offer incremental benefit for incremental effort. As long as we are executing the plan well, hard work yields results.

However, these tools for product improvement do not work the same way for startups. If you are building the wrong thing, optimizing the product or its marketing will not yield significant results. A startup has to measure progress against a high bar: evidence that a sustainable business can be built around its products or services. That's a standard that can be assessed only if a startup has made clear, tangible predictions ahead of time.

In the absence of those predictions, product and strategy decisions are far more difficult and time-consuming. I often see this in my consulting practice. I've been called in many times to help a startup that feels that its engineering team "isn't working hard enough."

When I meet with those teams, there are always improvements to be made and I recommend them, but invariably the real problem is not a lack of development talent, energy, or effort. Cycle after cycle, the team is working hard, but the business is not seeing results. Managers trained in a traditional model draw the logical conclusion: our team is not working hard, not working effectively, or not working efficiently.

Thus the downward cycle begins: the product development team valiantly tries to build a product according to the specifications it is receiving from the creative or business leadership. When good results are not forthcoming, business leaders assume that any discrepancy between what was planned and what was built is the cause and try to specify the next iteration in greater detail. As the specifications get more detailed, the planning process slows down, batch size increases, and feedback is delayed. If a board of directors or CFO is involved as a stakeholder, it doesn't take long for personnel changes to follow.

A few years ago, a team that sells products to large media companies invited me to help them as a consultant because they were concerned that their engineers were not working hard enough. However, the fault was not in the engineers; it was in the process the whole company was using to make decisions. They had customers but did not know them very well. They were deluged with feature requests from customers, the internal sales team, and the business leadership. Every new insight became an emergency that had to be addressed immediately. As a result, long-term projects were hampered by constant interruptions. Even worse, the team had no clear sense of whether any of the changes they were making mattered to customers. Despite the constant tuning and tweaking, the business results were consistently mediocre.

Learning milestones prevent this negative spiral by emphasizing a more likely possibility: the company is executing—with discipline!—a plan that does not make sense. The innovation accounting framework makes it clear when the company is stuck and needs to change direction.

In the example above, early in the company's life, the product

development team was incredibly productive because the company's founders had identified a large unmet need in the target market. The initial product, while flawed, was popular with early adopters. Adding the major features that customers asked for seemed to work wonders, as the early adopters spread the word about the innovation far and wide. But unasked and unanswered were other lurking questions: Did the company have a working engine of growth? Was this early success related to the daily work of the product development team? In most cases, the answer was no; success was driven by decisions the team had made in the past. None of its current initiatives were having any impact. But this was obscured because the company's gross metrics were all "up and to the right."

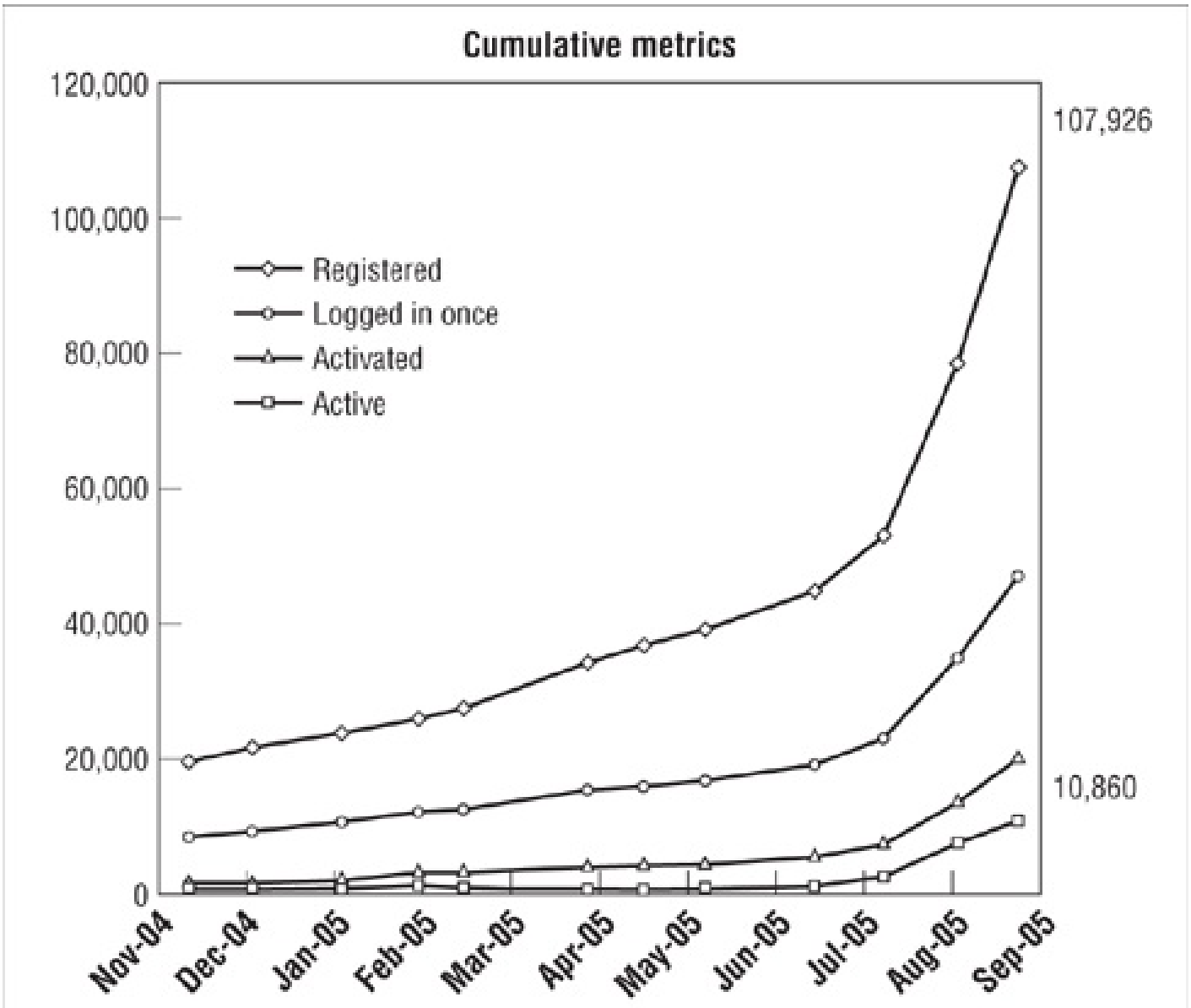
As we'll see in a moment, this is a common danger. Companies of any size that have a working engine of growth can come to rely on the wrong kind of metrics to guide their actions. This is what tempts managers to resort to the usual bag of success theater tricks: last-minute ad buys, channel stuffing, and whiz-bang demos, in a desperate attempt to make the gross numbers look better. Energy invested in success theater is energy that could have been used to help build a sustainable business. I call the traditional numbers used to judge startups "vanity metrics," and innovation accounting requires us to avoid the temptation to use them.

VANITY METRICS: A WORD OF CAUTION

To see the danger of vanity metrics clearly, let's return once more to the early days of IMVU. Take a look at the following graph, which is from the same era in IMVU's history as that shown earlier in this chapter. It covers the same time period as the cohort-style graph on this page; in fact, it is from the same board presentation.

This graph shows the traditional gross metrics for IMVU so far: total registered users and total paying customers (the gross revenue graph looks almost the same). From this viewpoint, things look much more exciting. That's why I call these vanity metrics: they give

the rosiest possible picture. You'll see a traditional hockey stick graph (the ideal in a rapid-growth company). As long as you focus on the top-line numbers (signing up more customers, an increase in overall revenue), you'll be forgiven for thinking this product development team is making great progress. The company's growth engine is working. Each month it is able to acquire customers and has a positive return on investment. The excess revenue from those customers is reinvested the next month in acquiring more. That's where the growth is coming from.



But think back to the same data presented in a cohort style. IMVU is adding new customers, but it is not improving the yield on each new group. The engine is turning, but the efforts to tune the

engine are not bearing much fruit. From the traditional graph alone, you cannot tell whether IMVU is on pace to build a sustainable business; you certainly can't tell anything about the efficacy of the entrepreneurial team behind it.

Innovation accounting will not work if a startup is being misled by these kinds of vanity metrics: gross number of customers and so on. The alternative is the kind of metrics we use to judge our business and our learning milestones, what I call actionable metrics.

ACTIONABLE METRICS VERSUS VANITY METRICS

To get a better sense of the importance of good metrics, let's look at a company called Grockit. Its founder, Farbood Nivi, spent a decade working as a teacher at two large for-profit education companies, Princeton Review and Kaplan, helping students prepare for standardized tests such as the GMAT, LSAT, and SAT. His engaging classroom style won accolades from his students and promotions from his superiors; he was honored with Princeton Review's National Teacher of the Year award. But Farb was frustrated with the traditional teaching methods used by those companies. Teaching six to nine hours per day to thousands of students, he had many opportunities to experiment with new approaches.[2]

Over time, Farb concluded that the traditional lecture model of education, with its one-to-many instructional approach, was inadequate for his students. He set out to develop a superior approach, using a combination of teacher-led lectures, individual homework, and group study. In particular, Farb was fascinated by how effective the student-to-student peer-driven learning method was for his students. When students could help each other, they benefited in two ways. First, they could get customized instruction from a peer who was much less intimidating than a teacher. Second, they could reinforce their learning by teaching it to others. Over time, Farb's classes became increasingly social—and successful.

As this unfolded, Farb felt more and more that his physical presence in the classroom was less important. He made an

important connection: “I have this social learning model in my classroom. There’s all this social stuff going on on the web.” His idea was to bring social peer-to-peer learning to people who could not afford an expensive class from Kaplan or Princeton Review or an even more expensive private tutor. From this insight Grockit was born.

Farb explains, “Whether you’re studying for the SAT or you’re studying for algebra, you study in one of three ways. You spend some time with experts, you spend some time on your own, and you spend some time with your peers. Grockit offers these three same formats of studying. What we do is we apply technology and algorithms to optimize those three forms.”

Farb is the classic entrepreneurial visionary. He recounts his original insight this way: “Let’s forget educational design up until now, let’s forget what’s possible and just redesign learning with today’s students and today’s technology in mind. There were plenty of multi-billion-dollar organizations in the education space, and I don’t think they were innovating in the way that we needed them to and I didn’t think we needed them anymore. To me, it’s really all about the students and I didn’t feel like the students were being served as well as they could.”

Today Grockit offers many different educational products, but in the beginning Farb followed a lean approach. Grockit built a minimum viable product, which was simply Farb teaching test prep via the popular online web conferencing tool WebEx. He built no custom software, no new technology. He simply attempted to bring his new teaching approach to students via the Internet. News about a new kind of private tutoring spread quickly, and within a few months Farb was making a decent living teaching online, with monthly revenues of $10,000 to $15,000. But like many entrepreneurs with ambition, Farb didn’t build his MVP just to make a living. He had a vision of a more collaborative, more effective kind of teaching for students everywhere. With his initial traction, he was able to raise money from some of the most prestigious investors in Silicon Valley.

When I first met Farb, his company was already on the fast track

to success. They had raised venture capital from well-regarded investors, had built an awesome team, and were fresh off an impressive debut at one of Silicon Valley's famous startup competitions.

They were extremely process-oriented and disciplined. Their product development followed a rigorous version of the agile development methodology known as Extreme Programming (described below), thanks to their partnership with a San Francisco–based company called Pivotal Labs. Their early product was hailed by the press as a breakthrough.

There was only one problem: they were not seeing sufficient growth in the use of the product by customers. Grockit is an excellent case study because its problems were not a matter of failure of execution or discipline.

Following standard agile practice, Grockit's work proceeded in a series of sprints, or one-month iteration cycles. For each sprint, Farb would prioritize the work to be done that month by writing a series of user stories, a technique taken from agile development. Instead of writing a specification for a new feature that described it in technical terms, Farb would write a story that described the feature from the point of view of the customer. That story helped keep the engineers focused on the customer's perspective throughout the development process.

Each feature was expressed in plain language in terms everyone could understand whether they had a technical background or not. Again following standard agile practice, Farb was free to reprioritize these stories at any time. As he learned more about what customers wanted, he could move things around in the product backlog, the queue of stories yet to be built. The only limit on this ability to change directions was that he could not interrupt any task that was in progress. Fortunately, the stories were written in such a way that the batch size of work (which I'll discuss in more detail in Chapter 9) was only a day or two.

This system is called agile development for a good reason: teams that employ it are able to change direction quickly, stay light on their feet, and be highly responsive to changes in the business

requirements of the product owner (the manager of the process—in this case Farb—who is responsible for prioritizing the stories).

How did the team feel at the end of each sprint? They consistently delivered new product features. They would collect feedback from customers in the form of anecdotes and interviews that indicated that at least some customers liked the new features. There was always a certain amount of data that showed improvement: perhaps the total number of customers was increasing, the total number of questions answered by students was going up, or the number of returning customers was increasing.

However, I sensed that Farb and his team were left with lingering doubts about the company's overall progress. Was the increase in their numbers actually caused by their development efforts? Or could it be due to other factors, such as mentions of Grockit in the press? When I met the team, I asked them this simple question: How do you know that the prioritization decisions that Farb is making actually make sense?

Their answer: "That's not our department. Farb makes the decisions; we execute them."

At that time Grockit was focused on just one customer segment: prospective business school students who were studying for the GMAT. The product allowed students to engage in online study sessions with fellow students who were studying for the same exam. The product was working: the students who completed their studying via Grockit achieved significantly higher scores than they had before. But the Grockit team was struggling with the age-old startup problems: How do we know which features to prioritize? How can we get more customers to sign up and pay? How can we get out the word about our product?

I put this question to Farb: "How confident are you that you are making the right decisions in terms of establishing priorities?" Like most startup founders, he was looking at the available data and making the best educated guesses he could. But this left a lot of room for ambiguity and doubt.

Farb believed in his vision thoroughly and completely, yet he was starting to question whether his company was on pace to realize

that vision. The product improved every day, but Farb wanted to make sure those improvements mattered to customers. I believe he deserves a lot of credit for realizing this. Unlike many visionaries, who cling to their original vision no matter what, Farb was willing to put his vision to the test.

Farb worked hard to sustain his team's belief that Grockit was destined for success. He was worried that morale would suffer if anyone thought that the person steering the ship was uncertain about which direction to go. Farb himself wasn't sure if his team would embrace a true learning culture. After all, this was part of the grand bargain of agile development: engineers agree to adapt the product to the business's constantly changing requirements but are not responsible for the quality of those business decisions.

Agile is an efficient system of development from the point of view of the developers. It allows them to stay focused on creating features and technical designs. An attempt to introduce the need to learn into that process could undermine productivity.

(Lean manufacturing faced similar problems when it was introduced in factories. Managers were used to focusing on the utilization rate of each machine. Factories were designed to keep machines running at full capacity as much of the time as possible. Viewed from the perspective of the machine, that is efficient, but from the point of view of the productivity of the entire factory, it is wildly inefficient at times. As they say in systems theory, that which optimizes one part of the system necessarily undermines the system as a whole.)

What Farb and his team didn't realize was that Grockit's progress was being measured by vanity metrics: the total number of customers and the total number of questions answered. That was what was causing his team to spin its wheels; those metrics gave the team the sensation of forward motion even though the company was making little progress. What's interesting is how closely Farb's method followed superficial aspects of the Lean Startup learning milestones: they shipped an early product and established some baseline metrics. They had relatively short iterations, each of which was judged by its ability to improve customer metrics.

However, because Grockit was using the wrong kinds of metrics, the startup was not genuinely improving. Farb was frustrated in his efforts to learn from customer feedback. In every cycle, the type of metrics his team was focused on would change: one month they would look at gross usage numbers, another month registration numbers, and so on. Those metrics would go up and down seemingly on their own. He couldn't draw clear cause-and-effect inferences. Prioritizing work correctly in such an environment is extremely challenging.

Farb could have asked his data analyst to investigate a particular question. For example, when we shipped feature X, did it affect customer behavior? But that would have required tremendous time and effort. When, exactly, did feature X ship? Which customers were exposed to it? Was anything else launched around that same time? Were there seasonal factors that might be skewing the data? Finding these answers would have required parsing reams and reams of data. The answer often would come weeks after the question had been asked. In the meantime, the team would have moved on to new priorities and new questions that needed urgent attention.

Compared to a lot of startups, the Grockit team had a huge advantage: they were tremendously disciplined. A disciplined team may apply the wrong methodology but can shift gears quickly once it discovers its error. Most important, a disciplined team can experiment with its own working style and draw meaningful conclusions.

Cohorts and Split-tests

Grockit changed the metrics they used to evaluate success in two ways. Instead of looking at gross metrics, Grockit switched to cohort-based metrics, and instead of looking for cause-and-effect relationships after the fact, Grockit would launch each new feature as a true split-test experiment.

A split-test experiment is one in which different versions of a

product are offered to customers at the same time. By observing the changes in behavior between the two groups, one can make inferences about the impact of the different variations. This technique was pioneered by direct mail advertisers. For example, consider a company that sends customers a catalog of products to buy, such as Lands' End or Crate & Barrel. If you wanted to test a catalog design, you could send a new version of it to 50 percent of the customers and send the old standard catalog to the other 50 percent. To assure a scientific result, both catalogs would contain identical products; the only difference would be the changes to the design. To figure out if the new design was effective, all you would have to do was keep track of the sales figures for both groups of customers. (This technique is sometimes called A/B testing after the practice of assigning letter names to each variation.) Although split testing often is thought of as a marketing-specific (or even a direct marketing–specific) practice, Lean Startups incorporate it directly into product development.

These changes led to an immediate change in Farb's understanding of the business. Split testing often uncovers surprising things. For example, many features that make the product better in the eyes of engineers and designers have no impact on customer behavior. This was the case at Grockit, as it has been in every company I have seen adopt this technique. Although working with split tests seems to be more difficult because it requires extra accounting and metrics to keep track of each variation, it almost always saves tremendous amounts of time in the long run by eliminating work that doesn't matter to customers.

Split testing also helps teams refine their understanding of what customers want and don't want. Grockit's team constantly added new ways for their customers to interact with each other in the hope that those social communication tools would increase the product's value. Inherent in those efforts was the belief that customers desired more communication during their studying. When split testing revealed that the extra features did not change customer behavior, it called that belief into question.

The questioning inspired the team to seek a deeper

understanding of what customers really wanted. They brainstormed new ideas for product experiments that might have more impact. In fact, many of these ideas were not new. They had simply been overlooked because the company was focused on building social tools. As a result, Grockit tested an intensive solo-studying mode, complete with quests and gamelike levels, so that students could have the choice of studying by themselves or with others. Just as in Farb's original classroom, this proved extremely effective. Without the discipline of split testing, the company might not have had this realization. In fact, over time, through dozens of tests, it became clear that the key to student engagement was to offer them a combination of social and solo features. Students preferred having a choice of how to study.

Kanban

Following the lean manufacturing principle of kanban, or capacity constraint, Grockit changed the product prioritization process. Under the new system, user stories were not considered complete until they led to validated learning. Thus, stories could be cataloged as being in one of four states of development: in the product backlog, actively being built, done (feature complete from a technical point of view), or in the process of being validated. Validated was defined as "knowing whether the story was a good idea to have been done in the first place." This validation usually would come in the form of a split test showing a change in customer behavior but also might include customer interviews or surveys.

The kanban rule permitted only so many stories in each of the four states. As stories flow from one state to the other, the buckets fill up. Once a bucket becomes full, it cannot accept more stories. Only when a story has been validated can it be removed from the kanban board. If the validation fails and it turns out the story is a bad idea, the relevant feature is removed from the product (see the chart on this page).

KANBAN DIAGRAM OF WORK AS IT PROGRESSES FROM STAGE TO STAGE

(No bucket can contain more than three projects at a time.)

BACKLOG	IN PROGRESS	BUILT	VALIDATED
A B C	D E	F	

Work on A begins. D and E are in development. F awaits validation.

BACKLOG	IN PROGRESS	BUILT	VALIDATED
G H I	 B C	D E A	F

F is validated. D and E await validation. G, H, I are new tasks to be undertaken. B and C are being built. A completes development.

BACKLOG	IN PROGRESS	BUILT	VALIDATED
 H → I →	G B → C →	D E A	F

B and C have been built, but under kanban, cannot be moved to the next bucket for validation until A, D, E have been validated. Work cannot begin on H and I until space opens up in the buckets ahead.

I have implemented this system with several teams, and the

initial result is always frustrating: each bucket fills up, starting with the "validated" bucket and moving on to the "done" bucket, until it's not possible to start any more work. Teams that are used to measuring their productivity narrowly, by the number of stories they are delivering, feel stuck. The only way to start work on new features is to investigate some of the stories that are done but haven't been validated. That often requires nonengineering efforts: talking to customers, looking at split-test data, and the like.

Pretty soon everyone gets the hang of it. This progress occurs in fits and starts at first. Engineering may finish a big batch of work, followed by extensive testing and validation. As engineers look for ways to increase their productivity, they start to realize that if they include the validation exercise from the beginning, the whole team can be more productive.

For example, why build a new feature that is not part of a split-test experiment? It may save you time in the short run, but it will take more time later to test, during the validation phase. The same logic applies to a story that an engineer doesn't understand. Under the old system, he or she would just build it and find out later what it was for. In the new system, that behavior is clearly counterproductive: without a clear hypothesis, how can a story ever be validated? We saw this behavior at IMVU, too. I once saw a junior engineer face down a senior executive over a relatively minor change. The engineer insisted that the new feature be split-tested, just like any other. His peers backed him up; it was considered absolutely obvious that all features should be routinely tested, no matter who was commissioning them. (Embarrassingly, all too often I was the executive in question.) A solid process lays the foundation for a healthy culture, one where ideas are evaluated by merit and not by job title.

Most important, teams working in this system begin to measure their productivity according to validated learning, not in terms of the production of new features.

Hypothesis Testing at Grockit

When Grockit made this transition, the results were dramatic. In one case, they decided to test one of their major features, called lazy registration, to see if it was worth the heavy investment they were making in ongoing support. They were confident in this feature because lazy registration is considered one of the design best practices for online services. In this system, customers do not have to register for the service up front. Instead, they immediately begin using the service and are asked to register only after they have had a chance to experience the service's benefit.

For a student, lazy registration works like this: when you come to the Grockit website, you're immediately placed in a study session with other students working on the same test. You don't have to give your name, e-mail address, or credit card number. There is nothing to prevent you from jumping in and getting started immediately. For Grockit, this was essential to testing one of its core assumptions: that customers would be willing to adopt this new way of learning only if they could see proof that it was working early on.

As a result of this hypothesis, Grockit's design required that it manage three classes of users: unregistered guests, registered (trial) guests, and customers who had paid for the premium version of the product. This design required significant extra work to build and maintain: the more classes of users there are, the more work is required to keep track of them, and the more marketing effort is required to create the right incentives to entice customers to upgrade to the next class. Grockit had undertaken this extra effort because lazy registration was considered an industry best practice.

I encouraged the team to try a simple split-test. They took one cohort of customers and required that they register immediately, based on nothing more than Grockit's marketing materials. To their surprise, this cohort's behavior was exactly the same as that of the lazy registration group: they had the same rate of registration, activation, and subsequent retention. In other words, the extra effort of lazy registration was a complete waste even though it was considered an industry best practice.

Even more important than reducing waste was the insight that this test suggested: customers were basing their decision about Grockit on something other than their use of the product.

Think about this. Think about the cohort of customers who were required to register for the product before entering a study session with other students. They had very little information about the product, nothing more than was presented on Grockit's home page and registration page. By contrast, the lazy registration group had a tremendous amount of information about the product because they had used it. Yet despite this information disparity, customer behavior was exactly the same.

This suggested that improving Grockit's positioning and marketing might have a more significant impact on attracting new customers than would adding new features. This was just the first of many important experiments Grockit was able to run. Since those early days, they have expanded their customer base dramatically: they now offer test prep for numerous standardized tests, including the GMAT, SAT, ACT, and GRE, as well as online math and English courses for students in grades 7 through 12.

Grockit continues to evolve its process, seeking continuous improvement at every turn. With more than twenty employees in its San Francisco office, Grockit continues to operate with the same deliberate, disciplined approach that has been their hallmark all along. They have helped close to a million students and are sure to help millions more.

THE VALUE OF THE THREE A'S

These examples from Grockit demonstrate each of the three A's of metrics: actionable, accessible, and auditable.

Actionable

For a report to be considered actionable, it must demonstrate clear

cause and effect. Otherwise, it is a vanity metric. The reports that Grockit's team began to use to judge their learning milestones made it extremely clear what actions would be necessary to replicate the results.

By contrast, vanity metrics fail this criterion. Take the number of hits to a company website. Let's say we have 40,000 hits this month—a new record. What do we need to do to get more hits? Well, that depends. Where are the new hits coming from? Is it from 40,000 new customers or from one guy with an extremely active web browser? Are the hits the result of a new marketing campaign or PR push? What is a hit, anyway? Does each page in the browser count as one hit, or do all the embedded images and multimedia content count as well? Those who have sat in a meeting debating the units of measurement in a report will recognize this problem.

Vanity metrics wreak havoc because they prey on a weakness of the human mind. In my experience, when the numbers go up, people think the improvement was caused by their actions, by whatever they were working on at the time. That is why it's so common to have a meeting in which marketing thinks the numbers went up because of a new PR or marketing effort and engineering thinks the better numbers are the result of the new features it added. Finding out what is actually going on is extremely costly, and so most managers simply move on, doing the best they can to form their own judgment on the basis of their experience and the collective intelligence in the room.

Unfortunately, when the numbers go down, it results in a very different reaction: now it's somebody else's fault. Thus, most team members or departments live in a world where their department is constantly making things better, only to have their hard work sabotaged by other departments that just don't get it. Is it any wonder these departments develop their own distinct language, jargon, culture, and defense mechanisms against the bozos working down the hall?

Actionable metrics are the antidote to this problem. When cause and effect is clearly understood, people are better able to learn from their actions. Human beings are innately talented learners

when given a clear and objective assessment.

Accessible

All too many reports are not understood by the employees and managers who are supposed to use them to guide their decision making. Unfortunately, most managers do not respond to this complexity by working hand in hand with the data warehousing team to simplify the reports so that they can understand them better. Departments too often spend their energy learning how to use data to get what they want rather than as genuine feedback to guide their future actions.

There is an antidote to this misuse of data. First, make the reports as simple as possible so that everyone understands them. Remember the saying "Metrics are people, too." The easiest way to make reports comprehensible is to use tangible, concrete units. What is a website hit? Nobody is really sure, but everyone knows what a person visiting the website is: one can practically picture those people sitting at their computers.

This is why cohort-based reports are the gold standard of learning metrics: they turn complex actions into people-based reports. Each cohort analysis says: among the people who used our product in this period, here's how many of them exhibited each of the behaviors we care about. In the IMVU example, we saw four behaviors: downloading the product, logging into the product from one's computer, engaging in a chat with other customers, and upgrading to the paid version of the product. In other words, the report deals with people and their actions, which are far more useful than piles of data points. For example, think about how hard it would have been to tell if IMVU was being successful if we had reported only on the total number of person-to-person conversations. Let's say we have 10,000 conversations in a period. Is that good? Is that one person being very, very social, or is it 10,000 people each trying the product one time and then giving up? There's no way to know without creating a more detailed report.

As the gross numbers get larger, accessibility becomes more and more important. It is hard to visualize what it means if the number of website hits goes down from 250,000 in one month to 200,000 the next month, but most people understand immediately what it means to lose 50,000 customers. That's practically a whole stadium full of people who are abandoning the product.

Accessibility also refers to widespread access to the reports. Grockit did this especially well. Every day their system automatically generated a document containing the latest data for every single one of their split-test experiments and other leap-of-faith metrics. This document was mailed to every employee of the company: they all always had a fresh copy in their e-mail in-boxes. The reports were well laid out and easy to read, with each experiment and its results explained in plain English.

Another way to make reports accessible is to use a technique we developed at IMVU. Instead of housing the analytics or data in a separate system, our reporting data and its infrastructure were considered part of the product itself and were owned by the product development team. The reports were available on our website, accessible to anyone with an employee account.

Each employee could log in to the system at any time, choose from a list of all current and past experiments, and see a simple one-page summary of the results. Over time, those one-page summaries became the de facto standard for settling product arguments throughout the organization. When people needed evidence to support something they had learned, they would bring a printout with them to the relevant meeting, confident that everyone they showed it to would understand its meaning.

Auditable

When informed that their pet project is a failure, most of us are tempted to blame the messenger, the data, the manager, the gods, or anything else we can think of. That's why the third A of good metrics. "auditable." is so essential. We must ensure that the data is

credible to employees.

The employees at IMVU would brandish one-page reports to demonstrate what they had learned to settle arguments, but the process often wasn't so smooth. Most of the time, when a manager, developer, or team was confronted with results that would kill a pet project, the loser of the argument would challenge the veracity of the data.

Such challenges are more common than most managers would admit, and unfortunately, most data reporting systems are not designed to answer them successfully. Sometimes this is the result of a well-intentioned but misplaced desire to protect the privacy of customers. More often, the lack of such supporting documentation is simply a matter of neglect. Most data reporting systems are not built by product development teams, whose job is to prioritize and build product features. They are built by business managers and analysts. Managers who must use these systems can only check to see if the reports are mutually consistent. They all too often lack a way to test if the data is consistent with reality.

The solution? First, remember that "Metrics are people, too." We need to be able to test the data by hand, in the messy real world, by talking to customers. This is the only way to be able to check if the reports contain true facts. Managers need the ability to spot check the data with real customers. It also has a second benefit: systems that provide this level of auditability give managers and entrepreneurs the opportunity to gain insights into why customers are behaving the way the data indicate.

Second, those building reports must make sure the mechanisms that generate the reports are not too complex. Whenever possible, reports should be drawn directly from the master data, rather than from an intermediate system, which reduces opportunities for error. I have noticed that every time a team has one of its judgments or assumptions overturned as a result of a technical problem with the data, its confidence, morale, and discipline are undermined.

When we watch entrepreneurs succeed in the mythmaking world of Hollywood, books, and magazines, the story is always structured the same way. First, we see the plucky protagonist having an epiphany, hatching a great new idea. We learn about his or her character and personality, how he or she came to be in the right place at the right time, and how he or she took the dramatic leap to start a business.

Then the photo montage begins. It's usually short, just a few minutes of time-lapse photography or narrative. We see the protagonist building a team, maybe working in a lab, writing on whiteboards, closing sales, pounding on a few keyboards. At the end of the montage, the founders are successful, and the story can move on to more interesting fare: how to split the spoils of their success, who will appear on magazine covers, who sues whom, and implications for the future.

Unfortunately, the real work that determines the success of startups happens during the photo montage. It doesn't make the cut in terms of the big story because it is too boring. Only 5 percent of entrepreneurship is the big idea, the business model, the whiteboard strategizing, and the splitting up of the spoils. The other 95 percent is the gritty work that is measured by innovation accounting: product prioritization decisions, deciding which customers to target or listen to, and having the courage to subject a grand vision to constant testing and feedback.

One decision stands out above all others as the most difficult, the most time-consuming, and the biggest source of waste for most startups. We all must face this fundamental test: deciding when to pivot and when to persevere. To understand what happens during the photo montage, we have to understand how to pivot, and that is the subject of Chapter 8.

8
PIVOT (OR PERSEVERE)

Every entrepreneur eventually faces an overriding challenge in developing a successful product: deciding when to pivot and when to persevere. Everything that has been discussed so far is a prelude to a seemingly simple question: are we making sufficient progress to believe that our original strategic hypothesis is correct, or do we need to make a major change? That change is called a pivot: a structured course correction designed to test a new fundamental hypothesis about the product, strategy, and engine of growth.

Because of the scientific methodology that underlies the Lean Startup, there is often a misconception that it offers a rigid clinical formula for making pivot or persevere decisions. This is not true. There is no way to remove the human element—vision, intuition, judgment—from the practice of entrepreneurship, nor would that be desirable.

My goal in advocating a scientific approach to the creation of startups is to channel human creativity into its most productive form, and there is no bigger destroyer of creative potential than the misguided decision to persevere. Companies that cannot bring themselves to pivot to a new direction on the basis of feedback from the marketplace can get stuck in the land of the living dead, neither growing enough nor dying, consuming resources and commitment from employees and other stakeholders but not moving ahead.

There is good news about our reliance on judgment, though. We

are able to learn, we are innately creative, and we have a remarkable ability to see the signal in the noise. In fact, we are so good at this that sometimes we see signals that aren't there. The heart of the scientific method is the realization that although human judgment may be faulty, we can improve our judgment by subjecting our theories to repeated testing.

Startup productivity is not about cranking out more widgets or features. It is about aligning our efforts with a business and product that are working to create value and drive growth. In other words, successful pivots put us on a path toward growing a sustainable business.

INNOVATION ACCOUNTING LEADS TO FASTER PIVOTS

To see this process in action, meet David Binetti, the CEO of Votizen. David has had a long career helping to bring the American political process into the twenty-first century. In the early 1990s, he helped build USA.gov, the first portal for the federal government. He's also experienced some classic startup failures. When it came time to build Votizen, David was determined to avoid betting the farm on his vision.

David wanted to tackle the problem of civic participation in the political process. His first product concept was a social network of verified voters, a place where people passionate about civic causes could get together, share ideas, and recruit their friends. David built his first minimum viable product for just over $1,200 in about three months and launched it.

David wasn't building something that nobody wanted. In fact, from its earliest days, Votizen was able to attract early adopters who loved the core concept. Like all entrepreneurs, David had to refine his product and business model. What made David's challenge especially hard was that he had to make those pivots in the face of moderate success.

David's initial concept involved four big leaps of faith:

1. Customers would be interested enough in the social network to sign up. (Registration)
2. Votizen would be able to verify them as registered voters. (Activation)
3. Customers who were verified voters would engage with the site's activism tools over time. (Retention)
4. Engaged customers would tell their friends about the service and recruit them into civic causes. (Referral)

Three months and $1,200 later, David's first MVP was in customers' hands. In the initial cohorts, 5 percent signed up for the service and 17 percent verified their registered voter status (see the chart below). The numbers were so low that there wasn't enough data to tell what sort of engagement or referral would occur. It was time to start iterating.

	INITIAL MVP
Registration	5%
Activation	17%
Retention	Too low
Referral	Too low

David spent the next two months and another $5,000 split testing new product features, messaging, and improving the product's design to make it easier to use. Those tests showed dramatic improvements, going from a 5 percent registration rate to 17 percent and from a 17 percent activation rate to over 90 percent. Such is the power of split testing. This optimization gave David a critical mass of customers with which to measure the next two leaps of faith. However, as shown in the chart below, those numbers proved to be even more discouraging: David achieved a referral rate of only 4 percent and a retention rate of 5 percent.

	INITIAL MVP	AFTER OPTIMIZATION
Registration	5%	17%
Activation	17%	90%
Retention	Too low	5%
Referral	Too low	4%

David knew he had to do more development and testing. For the next three months he continued to optimize, split test, and refine his pitch. He talked to customers, held focus groups, and did countless A/B experiments. As was explained in Chapter 7, in a split test, different versions of a product are offered to different customers at the same time. By observing the changes in behavior between the two groups, one can make inferences about the impact of the different variations. As shown in the chart below, the referral rate nudged up slightly to 6 percent and the retention rate went up to 8 percent. A disappointed David had spent eight months and $20,000 to build a product that wasn't living up to the growth model he'd hoped for.

	BEFORE OPTIMIZATION	AFTER OPTIMIZATION
Registration	17%	17%
Activation	90%	90%
Retention	5%	8%
Referral	4%	6%

David faced the difficult challenge of deciding whether to pivot or persevere. This is one of the hardest decisions entrepreneurs face. The goal of creating learning milestones is not to make the decision

easy; it is to make sure that there is relevant data in the room when it comes time to decide.

Remember, at this point David has had many customer conversations. He has plenty of learning that he can use to rationalize the failure he has experienced with the current product. That's exactly what many entrepreneurs do. In Silicon Valley, we call this experience getting stuck in the land of the living dead. It happens when a company has achieved a modicum of success—just enough to stay alive—but is not living up to the expectations of its founders and investors. Such companies are a terrible drain of human energy. Out of loyalty, the employees and founders don't want to give in; they feel that success might be just around the corner.

David had two advantages that helped him avoid this fate:

1. Despite being committed to a significant vision, he had done his best to launch early and iterate. Thus, he was facing a pivot or persevere moment just eight months into the life of his company. The more money, time, and creative energy that has been sunk into an idea, the harder it is to pivot. David had done well to avoid that trap.
2. David had identified his leap-of-faith questions explicitly at the outset and, more important, had made quantitative predictions about each of them. It would not have been difficult for him to declare success retroactively from that initial venture. After all, some of his metrics, such as activation, were doing quite well. In terms of gross metrics such as total usage, the company had positive growth. It is only because David focused on actionable metrics for each of his leap-of-faith questions that he was able to accept that his company was failing. In addition, because David had not wasted energy on premature PR, he was able to make this determination without public embarrassment or distraction.

Failure is a prerequisite to learning. The problem with the notion of shipping a product and then seeing what happens is that you are

guaranteed to succeed—at seeing what happens. But then what? As soon as you have a handful of customers, you're likely to have five opinions about what to do next. Which should you listen to?

Votizen's results were okay, but they were not good enough. David felt that although his optimization was improving the metrics, they were not trending toward a model that would sustain the business overall. But like all good entrepreneurs, he did not give up prematurely. David decided to pivot and test a new hypothesis. A pivot requires that we keep one foot rooted in what we've learned so far, while making a fundamental change in strategy in order to seek even greater validated learning. In this case, David's direct contact with customers proved essential.

He had heard three recurring bits of feedback in his testing:

1. "I always wanted to get more involved; this makes it so much easier."
2. "The fact that you prove I'm a voter matters."
3. "There's no one here. What's the point of coming back?"[1]

David decided to undertake what I call a zoom-in pivot, refocusing the product on what previously had been considered just one feature of a larger whole. Think of the customer comments above: customers like the concept, they like the voter registration technology, but they aren't getting value out of the social networking part of the product.

David decided to change Votizen into a product called @2gov, a "social lobbying platform." Rather than get customers integrated in a civic social network, @2gov allows them to contact their elected representatives quickly and easily via existing social networks such as Twitter. The customer engages digitally, but @2gov translates that digital contact into paper form. Members of Congress receive old-fashioned printed letters and petitions as a result. In other words, @2gov translates the high-tech world of its customers into the low-tech world of politics.

@2gov had a slightly different set of leap-of-faith questions to

answer. It still depended on customers signing up, verifying their voter status, and referring their friends, but the growth model changed. Instead of relying on an engagement-driven business ("sticky" growth), @2gov was more transactional. David's hypothesis was that passionate activists would be willing to pay money to have @2gov facilitate contacts on behalf of voters who cared about their issues.

David's new MVP took four months and another $30,000. He'd now spent a grand total of $50,000 and worked for twelve months. But the results from his next round of testing were dramatic: registration rate 42 percent, activation 83 percent, retention 21 percent, and referral a whopping 54 percent. However, the number of activists willing to pay was less than 1 percent. The value of each transaction was far too low to sustain a profitable business even after David had done his best to optimize it.

Before we get to David's next pivot, notice how convincingly he was able to demonstrate validated learning. He hoped that with this new product, he would be able to improve his leap-of-faith metrics dramatically, and he did (see the chart below).

	BEFORE PIVOT	AFTER PIVOT
Engine of growth	Sticky	Paid
Registration rate	17%	42%
Activation	90%	83%
Retention	8%	21%
Referral	6%	54%
Revenue	n/a	1%
Lifetime value (LTV)	n/a	Minimal

He did this not by working harder but by working smarter, taking

his product development resources and applying them to a new and different product. Compared with the previous four months of optimization, the new four months of pivoting had resulted in a dramatically higher return on investment, but David was still stuck in an age-old entrepreneurial trap. His metrics and product were improving, but not fast enough.

David pivoted again. This time, rather than rely on activists to pay money to drive contacts, he went to large organizations, professional fund-raisers, and big companies, which all have a professional or business interest in political campaigning. The companies seemed extremely eager to use and pay for David's service, and David quickly signed letters of intent to build the functionality they needed. In this pivot, David did what I call a customer segment pivot, keeping the functionality of the product the same but changing the audience focus. He focused on who pays: from consumers to businesses and nonprofit organizations. In other words, David went from being a business-to-consumer (B2C) company to being a business-to-business (B2B) company. In the process he changed his planned growth model, as well to one where he would be able to fund growth out of the profits generated from each B2B sale.

Three months later, David had built the functionality he had promised, based on those early letters of intent. But when he went back to companies to collect his checks, he discovered more problems. Company after company procrastinated, delayed, and ultimately passed up the opportunity. Although they had been excited enough to sign a letter of intent, closing a real sale was much more difficult. It turned out that those companies were not early adopters.

On the basis of the letters of intent, David had increased his head count, taking on additional sales staff and engineers in anticipation of having to service higher-margin business-to-business accounts. When the sales didn't materialize, the whole team had to work harder to try to find revenue elsewhere. Yet no matter how many sales calls they went on and no matter how much optimization they did to the product, the model wasn't working. Returning to his

leap-of-faith questions, David concluded that the results refuted his business-to-business hypothesis, and so he decided to pivot once again.

All this time, David was learning and gaining feedback from his potential customers, but he was in an unsustainable situation. You can't pay staff with what you've learned, and raising money at that juncture would have escalated the problem. Raising money without early traction is not a certain thing. If he had been able to raise money, he could have kept the company going but would have been pouring money into a value-destroying engine of growth. He would be in a high-pressure situation: use investor's cash to make the engine of growth work or risk having to shut down the company (or be replaced).

David decided to reduce staff and pivot again, this time attempting what I call a platform pivot. Instead of selling an application to one customer at a time, David envisioned a new growth model inspired by Google's AdWords platform. He built a self-serve sales platform where anyone could become a customer with just a credit card. Thus, no matter what cause you were passionate about, you could go to @2gov's website and @2gov would help you find new people to get involved. As always, the new people were verified registered voters, and so their opinions carried weight with elected officials.

The new product took only one additional month to build and immediately showed results: 51 percent sign-up rate, 92 percent activation rate, 28 percent retention rate, 64 percent referral rate (see the chart below). Most important, 11 percent of these customers were willing to pay 20 cents per message. Most important, this was the beginning of an actual growth model that could work. Receiving 20 cents per message might not sound like much, but the high referral rate meant that @2gov could grow its traffic without spending significant marketing money (this is the viral engine of growth).

	BEFORE PIVOT	AFTER PIVOT

Engine of growth	Paid	Viral
Registration rate	42%	51%
Activation	83%	92%
Retention	21%	28%
Referral	54%	64%
Revenue	1%	11%
Lifetime value (LTV)	Minimal	$0.20 per message

Votizen's story exhibits some common patterns. One of the most important to note is the acceleration of MVPs. The first MVP took eight months, the next four months, then three, then one. Each time David was able to validate or refute his next hypothesis faster than before.

How can one explain this acceleration? It is tempting to credit it to the product development work that had been going on. Many features had been created, and with them a fair amount of infrastructure. Therefore, each time the company pivoted, it didn't have to start from scratch. But this is not the whole story. For one thing, much of the product had to be discarded between pivots. Worse, the product that remained was classified as a legacy product, one that was no longer suited to the goals of the company. As is usually the case, the effort required to reform a legacy product took extra work. Counteracting these forces were the hard-won lessons David had learned through each milestone. Votizen accelerated its MVP process because it was learning critical things about its customers, market, and strategy.

Today, two years after its inception, Votizen is doing well. They recently raised $1.5 million from Facebook's initial investor Peter Thiel, one of the very few consumer Internet investments he has made in recent years. Votizen's system now can process voter identity in real time for forty-seven states representing 94 percent of

the U.S. population and has delivered tens of thousands of messages to Congress. The Startup Visa campaign used Votizen's tools to introduce the Startup Visa Act (S.565), which is the first legislation introduced into the Senate solely as a result of social lobbying. These activities have attracted the attention of established Washington consultants who are seeking to employ Votizen's tools in future political campaigns.

David Binetti sums up his experience building a Lean Startup:

> In 2003 I started a company in roughly the same space as I'm in today. I had roughly the same domain expertise and industry credibility, fresh off the USA.gov success. But back then my company was a total failure (despite consuming significantly greater investment), while now I have a business making money and closing deals. Back then I did the traditional linear product development model, releasing an amazing product (it really was) after 12 months of development, only to find that no one would buy it. This time I produced four versions in twelve weeks and generated my first sale relatively soon after that. And it isn't just market timing—two other companies that launched in a similar space in 2003 subsequently sold for tens of millions of dollars, and others in 2010 followed a linear model straight to the dead pool.

A STARTUP'S RUNWAY IS THE NUMBER OF PIVOTS IT CAN STILL MAKE

Seasoned entrepreneurs often speak of the runway that their startup has left: the amount of time remaining in which a startup must either achieve lift-off or fail. This usually is defined as the remaining cash in the bank divided by the monthly burn rate, or net drain on that account balance. For example, a startup with $1 million in the bank that is spending $100,000 per month has a projected runway of ten months.

When startups start to run low on cash, they can extend the runway two ways: by cutting costs or by raising additional funds. But when entrepreneurs cut costs indiscriminately, they are as liable to cut the costs that are allowing the company to get through its Build-Measure-Learn feedback loop as they are to cut waste. If the cuts result in a slowdown to this feedback loop, all they have accomplished is to help the startup go out of business more slowly.

The true measure of runway is how many pivots a startup has left: the number of opportunities it has to make a fundamental change to its business strategy. Measuring runway through the lens of pivots rather than that of time suggests another way to extend that runway: get to each pivot faster. In other words, the startup has to find ways to achieve the same amount of validated learning at lower cost or in a shorter time. All the techniques in the Lean Startup model that have been discussed so far have this as their overarching goal.

PIVOTS REQUIRE COURAGE

Ask most entrepreneurs who have decided to pivot and they will tell you that they wish they had made the decision sooner. I believe there are three reasons why this happens.

First, vanity metrics can allow entrepreneurs to form false conclusions and live in their own private reality. This is particularly damaging to the decision to pivot because it robs teams of the belief that it is necessary to change. When people are forced to change against their better judgment, the process is harder, takes longer, and leads to a less decisive outcome.

Second, when an entrepreneur has an unclear hypothesis, it's almost impossible to experience complete failure, and without failure there is usually no impetus to embark on the radical change a pivot requires. As I mentioned earlier, the failure of the "launch it and see what happens" approach should now be evident: you will always succeed—in seeing what happens. Except in rare cases, the early results will be ambiguous, and you won't know whether to

pivot or persevere, whether to change direction or stay the course.

Third, many entrepreneurs are afraid. Acknowledging failure can lead to dangerously low morale. Most entrepreneurs' biggest fear is not that their vision will prove to be wrong. More terrifying is the thought that the vision might be deemed wrong without having been given a real chance to prove itself. This fear drives much of the resistance to the minimum viable product, split testing, and other techniques to test hypotheses. Ironically, this fear drives up the risk because testing doesn't occur until the vision is fully represented. However, by that time it is often too late to pivot because funding is running out. To avoid this fate, entrepreneurs need to face their fears and be willing to fail, often in a public way. In fact, entrepreneurs who have a high profile, either because of personal fame or because they are operating as part of a famous brand, face an extreme version of this problem.

A new startup in Silicon Valley called Path was started by experienced entrepreneurs: Dave Morin, who previously had overseen Facebook's platform initiative; Dustin Mierau, product designer and cocreator of Macster; and Shawn Fanning of Napster fame. They decided to release a minimum viable product in 2010. Because of the high-profile nature of its founders, the MVP attracted significant press attention, especially from technology and startup blogs. Unfortunately, their product was not targeted at technology early adopters, and as a result, the early blogger reaction was quite negative. (Many entrepreneurs fail to launch because they are afraid of this kind of reaction, worrying that it will harm the morale of the entire company. The allure of positive press, especially in our "home" industry, is quite strong.)

Luckily, the Path team had the courage to ignore this fear and focus on what their customers said. As a result, they were able to get essential early feedback from actual customers. Path's goal is to create a more personal social network that maintains its quality over time. Many people have had the experience of being overconnected on existing social networks, sharing with past coworkers, high school friends, relatives, and colleagues. Such broad groups make it hard to share intimate moments. Path took an

unusual approach. For example, it limited the number of connections to fifty, based on brain research by the anthropologist Robin Dunbar at Oxford. His research suggests that fifty is roughly the number of personal relationships in any person's life at any given time.

For members of the tech press (and many tech early adopters) this "artificial" constraint on the number of connections was anathema. They routinely use new social networking products with thousands of connections. Fifty seemed way too small. As a result, Path endured a lot of public criticism, which was hard to ignore. But customers flocked to the platform, and their feedback was decidedly different from the negativity in the press. Customers liked the intimate moments and consistently wanted features that were not on the original product road map, such as the ability to share how friends' pictures made them feel and the ability to share "video moments."

Dave Morin summed up his experience this way:

> The reality of our team and our backgrounds built up a massive wall of expectations. I don't think it would have mattered what we would have released; we would have been met with expectations that are hard to live up to. But to us it just meant we needed to get our product and our vision out into the market broadly in order to get feedback and to begin iteration. We humbly test our theories and our approach to see what the market thinks. Listen to feedback honestly. And continue to innovate in the directions we think will create meaning in the world.

Path's story is just beginning, but already their courage in facing down critics is paying off. If and when they need to pivot, they won't be hampered by fear. They recently raised $8.5 million in venture capital in a round led by Kleiner Perkins Caufield & Byers. In doing so, Path reportedly turned down an acquisition offer for $100 million from Google.[2]

THE PIVOT OR PERSEVERE MEETING

The decision to pivot requires a clear-eyed and objective mind-set. We've discussed the telltale signs of the need to pivot: the decreasing effectiveness of product experiments and the general feeling that product development should be more productive. Whenever you see those symptoms, consider a pivot.

The decision to pivot is emotionally charged for any startup and has to be addressed in a structured way. One way to mitigate this challenge is to schedule the meeting in advance. I recommend that every startup have a regular "pivot or persevere" meeting. In my experience, less than a few weeks between meetings is too often and more than a few months is too infrequent. However, each startup needs to find its own pace.

Each pivot or persevere meeting requires the participation of both the product development and business leadership teams. At IMVU, we also added the perspectives of outside advisers who could help us see past our preconceptions and interpret data in new ways. The product development team must bring a complete report of the results of its product optimization efforts over time (not just the past period) as well as a comparison of how those results stack up against expectations (again, over time). The business leadership should bring detailed accounts of their conversations with current and potential customers.

Let's take a look at this process in action in a dramatic pivot done by a company called Wealthfront. That company was founded in 2007 by Dan Carroll and added Andy Rachleff as CEO shortly thereafter. Andy is a well-known figure in Silicon Valley: he is a cofounder and former general partner of the venture capital firm Benchmark Capital and is on the faculty of the Stanford Graduate School of Business, where he teaches a variety of courses on technology entrepreneurship. I first met Andy when he commissioned a case study on IMVU to teach his students about the process we had used to build the company.

Wealthfront's mission is to disrupt the mutual fund industry by bringing greater transparency, access, and value to retail investors.

What makes Wealthfront's story unusual, however, is not where it is today but how it began: as an online game.

In Wealthfront's original incarnation it was called kaChing and was conceived as a kind of fantasy league for amateur investors. It allowed anyone to open a virtual trading account and build a portfolio that was based on real market data without having to invest real money. The idea was to identify diamonds in the rough: amateur traders who lacked the resources to become fund managers but who possessed market insight. Wealthfront's founders did not want to be in the online gaming business per se; kaChing was part of a sophisticated strategy in the service of their larger vision. Any student of disruptive innovation would have looked on approvingly: they were following that system perfectly by initially serving customers who were unable to participate in the mainstream market. Over time, they believed, the product would become more and more sophisticated, eventually allowing users to serve (and disrupt) existing professional fund managers.

To identify the best amateur trading savants, Wealthfront built sophisticated technology to rate the skill of each fund manager, using techniques employed by the most sophisticated evaluators of money managers, the premier U.S. university endowments. Those methods allowed them to evaluate not just the returns the managers generated but also the amount of risk they had taken along with how consistent they performed relative to their declared investment strategy. Thus, fund managers who achieved great returns through reckless gambles (i.e., investments outside their area of expertise) would be ranked lower than those who had figured out how to beat the market through skill.

With its kaChing game, Wealthfront hoped to test two leap-of-faith assumptions:

1. A significant percentage of the game players would demonstrate enough talent as virtual fund managers to prove themselves suitable to become managers of real assets (the value hypothesis).

2. The game would grow using the viral engine of growth and generate value using a freemium business model. The game was free to play, but the team hoped that a percentage of the players would realize that they were lousy traders and therefore want to convert to paying customers once Wealthfront started offering real asset management services (the growth hypothesis).

kaChing was a huge early success, attracting more than 450,000 gamers in its initial launch. By now, you should be suspicious of this kind of vanity metric. Many less disciplined companies would have celebrated that success and felt their future was secure, but Wealthfront had identified its assumptions clearly and was able to think more rigorously. By the time Wealthfront was ready to launch its paid financial product, only seven amateur managers had qualified as worthy of managing other people's money, far less than the ideal model had anticipated. After the paid product launched, they were able to measure the conversion rate of gamers into paying customers. Here too the numbers were discouraging: the conversion rate was close to zero. Their model had predicted that hundreds of customers would sign up, but only fourteen did.

The team worked valiantly to find ways to improve the product, but none showed any particular promise. It was time for a pivot or persevere meeting.

If the data we have discussed so far was all that was available at that critical meeting, Wealthfront would have been in trouble. They would have known that their current strategy wasn't working but not what to do to fix it. That is why it was critical that they followed the recommendation earlier in this chapter to investigate alternative possibilities. In this case, Wealthfront had pursued two important lines of inquiry.

The first was a series of conversations with professional money managers, beginning with John Powers, the head of Stanford University's endowment, who reacted surprisingly positively. Wealthfront's strategy was premised on the assumption that professional money managers would be reluctant to join the system

because the increased transparency would threaten their sense of authority. Powers had no such concerns. CEO Andy Rachleff then began a series of conversations with other professional investment managers and brought the results back to the company. His insights were as follows:

1. Successful professional money managers felt they had nothing to fear from transparency, since they believed it would validate their skills.
2. Money managers faced significant challenges in managing and scaling their own businesses. They were hampered by the difficulty of servicing their own accounts and therefore had to require high minimum investments as a way to screen new clients.

The second problem was so severe that Wealthfront was fielding cold calls from professional managers asking out of the blue to join the platform. These were classic early adopters who had the vision to see past the current product to something they could use to achieve a competitive advantage.

The second critical qualitative information came out of conversations with consumers. It turned out that they found the blending of virtual and real portfolio management on the kaChing website confusing. Far from being a clever way of acquiring customers, the freemium strategy was getting in the way by promoting confusion about the company's positioning.

This data informed the pivot or persevere meeting. With everyone present, the team debated what to do with its future. The current strategy wasn't working, but many employees were nervous about abandoning the online game. After all, it was an important part of what they had signed on to build. They had invested significant time and energy building and supporting those customers. It was painful—as it always is—to realize that that energy had been wasted.

Wealthfront decided it could not persevere as it existed. The company chose instead to celebrate what it had learned. If it had

not launched its current product, the team never would have learned what it needed to know to pivot. In fact, the experience taught them something essential about their vision. As Andy says, "What we really wanted to change was not who manages the money but who has access to the best possible talent. We'd originally thought we'd need to build a significant business with amateur managers to get professionals to come on board, but fortunately it turns out that wasn't necessary."

The company pivoted, abandoning the gaming customers altogether and focusing on providing a service that allowed customers to invest with professional managers. On the surface, the pivot seems quite dramatic in that the company changed its positioning, its name, and its partner strategy. It even jettisoned a large proportion of the features it had built. But at its core, a surprising amount stayed the same. The most valuable work the company had done was building technology to evaluate managers' effectiveness, and this became the kernel around which the new business was built. This is also common with pivots; it is not necessary to throw out everything that came before and start over. Instead, it's about repurposing what has been built and what has been learned to find a more positive direction.

Today, Wealthfront is prospering as a result of its pivot, with over $180 million invested on the platform and more than forty professional managers.[3] It recently was named one of Fast Company's ten most innovative companies in finance.[4] The company continues to operate with agility, scaling in line with the growth principles outlined in Chapter 12. Wealthfront is also a leading advocate of the development technique known as continuous deployment, which we'll discuss in Chapter 9.

FAILURE TO PIVOT

The decision to pivot is so difficult that many companies fail to make it. I wish I could say that every time I was confronted with the need to pivot, I handled it well, but this is far from true. I

remember one failure to pivot especially well.

A few years after IMVU's founding, the company was having tremendous success. The business had grown to over $1 million per month in revenue; we had created more than twenty million avatars for our customers. We managed to raise significant new rounds of financing, and like the global economy, we were riding high. But danger lurked around the corner.

Unknowingly, we had fallen into a classic startup trap. We had been so successful with our early efforts that we were ignoring the principles behind them. As a result, we missed the need to pivot even as it stared us in the face.

We had built an organization that excelled at the kinds of activities described in earlier chapters: creating minimum viable products to test new ideas and running experiments to tune the engine of growth. Before we had begun to enjoy success, many people had advised against our "low-quality" minimum viable product and experimental approach, urging us to slow down. They wanted us to do things right and focus on quality instead of speed. We ignored that advice, mostly because we wanted to claim the advantages of speed. After our approach was vindicated, the advice we received changed. Now most of the advice we heard was that "you can't argue with success," urging us to stay the course. We liked this advice better, but it was equally wrong.

Remember that the rationale for building low-quality MVPs is that developing any features beyond what early adopters require is a form of waste. However, the logic of this takes you only so far. Once you have found success with early adopters, you want to sell to mainstream customers. Mainstream customers have different requirements and are much more demanding.

The kind of pivot we needed is called a customer segment pivot. In this pivot, the company realizes that the product it's building solves a real problem for real customers but that they are not the customers it originally planned to serve. In other words, the product hypothesis is confirmed only partially. (This chapter described such a pivot in the Votizen story, above.)

A customer segment pivot is an especially tricky pivot to execute

because, as we learned the hard way at IMVU, the very actions that made us successful with early adopters were diametrically opposed to the actions we'd have to master to be successful with mainstream customers. We lacked a clear understanding of how our engine of growth operated. We had begun to trust our vanity metrics. We had stopped using learning milestones to hold ourselves accountable. Instead, it was much more convenient to focus on the ever-larger gross metrics that were so exciting: breaking new records in signing up paying customers and active users, monitoring our customer retention rate—you name it. Under the surface, it should have been clear that our efforts at tuning the engine were reaching diminishing returns, the classic sign of the need to pivot.

For example, we spent months trying to improve the product's activation rate (the rate at which new customers become active consumers of the product), which remained stubbornly low. We did countless experiments: usability improvements, new persuasion techniques, incentive programs, customer quests, and other game-like features. Individually, many of these new features and new marketing tools were successful. We measured them rigorously, using A/B experimentation. But taken in aggregate, over the course of many months, we were seeing negligible changes in the overall drivers of our engine of growth. Even our activation rate, which had been the center of our focus, edged up only a few percentage points.

We ignored the signs because the company was still growing, delivering month after month of "up and to the right" results. But we were quickly exhausting our early adopter market. It was getting harder and harder to find customers we could acquire at the prices we were accustomed to paying. As we drove our marketing team to find more customers, they were forced to reach out more to mainstream customers, but mainstream customers are less forgiving of an early product. The activation and monetization rates of new customers started to go down, driving up the cost of acquiring new customers. Pretty soon, our growth was flatlining and our engine sputtered and stalled.

It took us far too long to make the changes necessary to fix this

situation. As with all pivots, we had to get back to basics and start the innovation accounting cycle over. It felt like the company's second founding. We had gotten really good at optimizing, tuning, and iterating, but in the process we had lost sight of the purpose of those activities: testing a clear hypothesis in the service of the company's vision. Instead, we were chasing growth, revenue, and profits wherever we could find them.

We needed to reacquaint ourselves with our new mainstream customers. Our interaction designers led the way by developing a clear customer archetype that was based on extensive in-person conversations and observation. Next, we needed to invest heavily in a major product overhaul designed to make the product dramatically easier to use. Because of our overfocus on fine-tuning, we had stopped making large investments like these, preferring to invest in lower-risk and lower-yield testing experiments.

However, investing in quality, design, and larger projects did not require that we abandon our experimental roots. On the contrary, once we realized our mistake and executed the pivot, those skills served us well. We created a sandbox for experimentation like the one described in Chapter 12 and had a cross-functional team work exclusively on this major redesign. As they built, they continuously tested their new design head to head against the old one. Initially, the new design performed worse than the old one, as is usually the case. It lacked the features and functionality of the old design and had many new mistakes as well. But the team relentlessly improved the design until, months later, it performed better. This new design laid the foundation for our future growth.

This foundation has paid off handsomely. By 2009, revenue had more than doubled to over $25 million annually. But we might have enjoyed that success earlier if we had pivoted sooner.[5]

A CATALOG OF PIVOTS

Pivots come in different flavors. The word pivot sometimes is used incorrectly as a synonym for change. A pivot is a special kind of

change designed to test a new fundamental hypothesis about the product, business model, and engine of growth.

Zoom-in Pivot

In this case, what previously was considered a single feature in a product becomes the whole product. This is the type of pivot Votizen made when it pivoted away from a full social network and toward a simple voter contact product.

Zoom-out Pivot

In the reverse situation, sometimes a single feature is insufficient to support a whole product. In this type of pivot, what was considered the whole product becomes a single feature of a much larger product.

Customer Segment Pivot

In this pivot, the company realizes that the product it is building solves a real problem for real customers but that they are not the type of customers it originally planned to serve. In other words, the product hypothesis is partially confirmed, solving the right problem, but for a different customer than originally anticipated.

Customer Need Pivot

As a result of getting to know customers extremely well, it sometimes becomes clear that the problem we're trying to solve for them is not very important. However, because of this customer intimacy, we often discover other related problems that are important and can be solved by our team. In many cases, these related problems may require little more than repositioning the

existing product. In other cases, it may require a completely new product. Again, this a case where the product hypothesis is partially confirmed; the target customer has a problem worth solving, just not the one that was originally anticipated.

A famous example is the chain Potbelly Sandwich Shop, which today has over two hundred stores. It began as an antique store in 1977; the owners started to sell sandwiches as a way to bolster traffic to their stores. Pretty soon they had pivoted their way into an entirely different line of business.

Platform Pivot

A platform pivot refers to a change from an application to a platform or vice versa. Most commonly, startups that aspire to create a new platform begin life by selling a single application, the so-called killer app, for their platform. Only later does the platform emerge as a vehicle for third parties to leverage as a way to create their own related products. However, this order is not always set in stone, and some companies have to execute this pivot multiple times.

Business Architecture Pivot

This pivot borrows a concept from Geoffrey Moore, who observed that companies generally follow one of two major business architectures: high margin, low volume (complex systems model) or low margin, high volume (volume operations model).[6] The former commonly is associated with business to business (B2B) or enterprise sales cycles, and the latter with consumer products (there are notable exceptions). In a business architecture pivot, a startup switches architectures. Some companies change from high margin, low volume by going mass market (e.g., Google's search "appliance"); others, originally designed for the mass market, turned out to require long and expensive sales cycles.

Value Capture Pivot

There are many ways to capture the value a company creates. These methods are referred to commonly as monetization or revenue models. These terms are much too limiting. Implicit in the idea of monetization is that it is a separate "feature" of a product that can be added or removed at will. In reality, capturing value is an intrinsic part of the product hypothesis. Often, changes to the way a company captures value can have far-reaching consequences for the rest of the business, product, and marketing strategies.

Engine of Growth Pivot

As we'll see in Chapter 10, there are three primary engines of growth that power startups: the viral, sticky, and paid growth models. In this type of pivot, a company changes its growth strategy to seek faster or more profitable growth. Commonly but not always, the engine of growth also requires a change in the way value is captured.

Channel Pivot

In traditional sales terminology, the mechanism by which a company delivers its product to customers is called the sales channel or distribution channel. For example, consumer packaged goods are sold in a grocery store, cars are sold in dealerships, and much enterprise software is sold (with extensive customization) by consulting and professional services firms. Often, the requirements of the channel determine the price, features, and competitive landscape of a product. A channel pivot is a recognition that the same basic solution could be delivered through a different channel with greater effectiveness. Whenever a company abandons a previously complex sales process to "sell direct" to its end users, a channel pivot is in progress.

It is precisely because of its destructive effect on sales channels that the Internet has had such a disruptive influence in industries that previously required complex sales and distribution channels, such as newspaper, magazine, and book publishing.

Technology Pivot

Occasionally, a company discovers a way to achieve the same solution by using a completely different technology. Technology pivots are much more common in established businesses. In other words, they are a sustaining innovation, an incremental improvement designed to appeal to and retain an existing customer base. Established companies excel at this kind of pivot because so much is not changing. The customer segment is the same, the customer's problem is the same, the value-capture model is the same, and the channel partners are the same. The only question is whether the new technology can provide superior price and/or performance compared with the existing technology.

A PIVOT IS A STRATEGIC HYPOTHESIS

Although the pivots identified above will be familiar to students of business strategy, the ability to pivot is no substitute for sound strategic thinking. The problem with providing famous examples of pivots is that most people are familiar only with the successful end strategies of famous companies. Most readers know that Southwest or Walmart is an example of a low-cost disruption in their markets, that Microsoft an example of a platform monopoly, and that Starbucks has leveraged a powerful premium brand. What is generally less well known are the pivots that were required to discover those strategies. Companies have a strong incentive to align their PR stories around the heroic founder and make it seem that their success was the inevitable result of a good idea.

Thus, although startups often pivot into a strategy that seems

similar to that of a successful company, it is important not to put too much stock in these analogies. It's extremely difficult to know if the analogy has been drawn properly. Have we copied the essential features or just superficial ones? Will what worked in that industry work in ours? Will what has worked in the past work today? A pivot is better understood as a new strategic hypothesis that will require a new minimum viable product to test.

Pivots are a permanent fact of life for any growing business. Even after a company achieves initial success, it must continue to pivot. Those familiar with the technology life cycle ideas of theorists such as Geoffrey Moore know certain later-stage pivots by the names he has given them: the Chasm, the Tornado, the Bowling Alley. Readers of the disruptive innovation literature spearheaded by Harvard's Clayton Christensen will be familiar with established companies that fail to pivot when they should. The critical skill for managers today is to match those theories to their present situation so that they apply the right advice at the right time.

Modern managers cannot have escaped the deluge of recent books calling on them to adapt, change, reinvent, or upend their existing businesses. Many of the works in this category are long on exhortations and short on specifics.

A pivot is not just an exhortation to change. Remember, it is a special kind of structured change designed to test a new fundamental hypothesis about the product, business model, and engine of growth. It is the heart of the Lean Startup method. It is what makes the companies that follow Lean Startup resilient in the face of mistakes: if we take a wrong turn, we have the tools we need to realize it and the agility to find another path.

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In Part Two, we have looked at a startup idea from its initial leaps of faith, tested it with a minimum viable product, used innovation accounting and actionable metrics to evaluate the results, and made the decision to pivot or persevere.

I have treated these subjects in great detail to prepare for what

comes next. On the page, these processes may seem clinical, slow, and simple. In the real world, something different is needed. We have learned to steer when moving slowly. Now we must learn to race. Laying a solid foundation is only the first step toward our real destination: acceleration.