

The Right It

Why So Many Ideas Fail
and How to Make Sure
Yours Succeed

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*To the Beast of Failure.
You taught me a few lessons, now it's my turn.*

**It waits. Patient.
Confident that it will soon get its prey—it always does.
Few escape its bite, none its tentacles.
One way or another, the Beast of Failure gets us all.**

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Prelude

It was 3 a.m., but I was too agitated to fall asleep. In six hours I would be sitting in the final board meeting for the company I had cofounded. After five years of hard work and several attempts to make our business work, we had no options but to accept a fire-sale offer—pennies on the dollar—for our award-winning technology and assets. Dozens of people I had hired, people who had put their trust in me and my vision, would be out of a job. The three world-class venture capitalists who had entrusted us with not only a \$25 million investment but also their time, connections, and advice would be staring angrily at me, my cofounder, and our executive team. I had been bitten by the Beast of Failure, and it hurt like hell.

What hurt the most was that I could not figure out where I had gone wrong. Failure was something that only affected other people: less experienced, less competent, and less well-prepared people. Until then I had a perfect record with startups and entrepreneurship. I was an early employee and had a great career at two fledgling companies that eventually became industry giants (Sun Microsystems and Google). I had also cofounded a venture capital-backed company that managed to turn a \$3 million investment into a \$100 million acquisition in eighteen months. My record was a perfect 3 and 0, and I was sure that win number 4 was just a matter of time. The formula was easy: identify an idea for a new product or service that solves a big problem, assemble a strong team, get venture funding, build the idea, launch it, and then go public or—worst case—get acquired for a pile of money.

And we had done all that. We had an ambitious idea for a product that addressed a major software engineering problem in a very innovative way. All our due diligence and market research confirmed that companies needed, wanted, and would buy our product for their developers. We had assembled a team of amazing people who worked incredibly hard for five years. We had a great business plan and plenty of funding from the best venture capitalists (VCs) in the world to execute it—and we executed it, just as planned.

So, WTF? Why The Failure? Where did we go wrong?

Unable to sleep, I got out of bed and stared out the window thinking of all the other people who were, had been, or would soon be in a similar predicament. It was a sobering thought.

* * *

At this very moment, millions of people around the world are working hard to bring to life new ideas that, when launched, will be successful. Some of these ideas will turn out to be stunning successes that will have a major impact on our world and our culture: the next Google, the next polio vaccine, the next Harry Potter series, the next Red Cross, the next Ford Mustang. Others will be smaller, more personal, but no less meaningful successes: a little restaurant that becomes a neighborhood favorite, a biography that does not make the bestseller list but tells an important story, a local nonprofit that cares for abandoned pets.

At this very same moment, many other people are working equally hard to develop new ideas that, when launched, will fail. Some of them will fail spectacularly and publicly: like New Coke, the movie *John Carter*, or the Ford Edsel. Others will be smaller, more private, but no less painful failures: a home-based business that never takes off, a children's book that neither publishers nor children have any interest in, a charity for a cause that too few people care enough about.

If you are currently working to develop a new idea, whether on your own or as part of a team, which group are you in? Or if at this moment you are still only *thinking* about investing to develop a new idea, which group will you be in?

Most people believe that they either are or will be in the first group—the group whose ideas will be successful. All they have to do is work hard and execute well. Unfortunately, we know that this cannot be the case. Most new products, services, businesses, and initiatives will fail soon after they are launched—regardless of how promising they sound, how much their developers commit to them, or how well they execute them.

This is a hard fact to accept. We believe that other people fail, because they don't know what they are doing. They are losers who have no business being in that business. Somehow, we believe that this does

not apply to us and to our idea—especially if we've experienced victories in the past: "I am a winner. I was successful before. I will be successful again—just watch me!"

That is how I used to think. And I believed I had good reasons for my smugness, because I had experienced a string of successes with only a few relatively minor setbacks. Failure was something that only affected other people.

And then, just as I had reached new heights of confidence and hubris, the Beast of Failure wrapped its tentacles around me and bit me in the ass. A good, hard-to-ignore, and impossible-to-forget bite on that smart, competent, and well-prepared ass of mine.

I could lick my wounds or bite back. I decided to bite back.

Failure became my nemesis. Defeating it, my obsession. Teaching others how to defeat it, my mission.

This book is part of that mission.

Book Overview

I am a practical guy, and this is a practical book. I believe that the best way to accomplish goals, deal with challenges, and solve problems is to approach them with the proper combination of facts, tools, and tactics. And that's exactly how this book is organized.

Part I: Hard Facts

The initial set of chapters is my version of “the birds and the bees” talk you may have had to endure when you were a child. But instead of explaining how babies are born (and other facts of life), I will explain how new product ideas are hatched—and what fate awaits them—using my own set of metaphors.

To begin with, we take an unflinching look at the Law of Market Failure in [Chapter 1](#). It won’t be a pretty sight. The gauntlet that awaits most new ideas might remind you of one of those hard-to-watch scenes in nature documentaries, like the one where they show hundreds of newly hatched sea turtles race from the beach to the surf, while all sorts of predators pick them off, one by one, like mini-quiches at a buffet. Only a few lucky baby turtles make it to the water, and even fewer reach maturity. Mother Nature is brutal—and so is the market. Before we can consistently beat failure, we need to study it and understand it.

In [Chapter 2](#), you will learn that our only chance to beat the Law of Market Failure is to have an idea that is The Right It. I introduce The Right It and its evil twin, The Wrong It (an idea destined to fail in the market), and explain how and why we so often end up investing in ideas that are The Wrong It. You will also learn why I have a low opinion of opinions.

Data! How do I love thee, let me count the ways. [Chapter 3](#) is dedicated to my passion for data—especially market data. But you will also learn that I don’t love all data equally. In fact, I am very, very picky about my data. The goal of this chapter is to make you love (or at least appreciate) data as much as I do and to be as choosy with it as I am.

Part II: Sharp Tools

You've probably heard the expressions "You need the right tool for the job" and "You are only as good as your tools." Well, we have one heck of a job to do, and we have no chance of succeeding without the right tools. Part II: Sharp Tools is our toolbox. In it, I introduce you to a set of powerful tools that will give you a significant advantage in fighting failure.

The root cause of most failures can be traced back to muddy, fuzzy, undisciplined thinking. Unless and until we can articulate our new product idea clearly and thoroughly, our odds of market success will not improve. In [Chapter 4](#) you will learn a surprisingly simple but incredibly effective set of tools designed to turn your thinking from fuzzy to razor sharp.

In [Chapter 5](#), I introduce the concept of *pretotyping*, point out how pretotyping differs from prototyping, and explain why that single vowel makes all the difference in our quest for The Right It. Then I share with you a powerful collection of pretotyping techniques along with many examples of how to use prototypes to test how the market responds to your new product idea.

Pretotyping experiments give you a lot of great fresh firsthand market data. But having great data is not enough. In order to make the right decisions, the data has to be analyzed and interpreted rigorously and objectively. In [Chapter 6](#), you will get the tools to do just that.

Part III: Plastic Tactics

In the final part of the book, I share with you my thoughts on the best ways to put your newfound knowledge and tools into action. [Chapter 7](#) is where you will learn four powerful tactics to help you organize and execute your market validation tests in the most efficient way possible. In [Chapter 8](#), I'll walk you through a realistic scenario of what happens to an idea for a new business when we apply the tools and tactics that we've learned.

The first eight chapters of the book are fact-oriented and practical—mostly nuts-and-bolts and how-to stuff. In the final chapter, [Chapter 9](#),

I raise the level of our discourse and get a bit more abstract and philosophical. The tools and tactics you've learned will give you new power and a significant advantage against the Beast of Failure. What will you use that newfound power and advantage for? What kind of new products, services, or businesses would you tackle if you knew that the odds for success were greatly in your favor? I will encourage you to think bigger and aim higher.

Tall Tales and Short Stories

I've included many real-world stories in this book. I chose each story based on how well it illustrates a point, technique, or moral. But as you read them, keep in mind that they are just that, *stories*. And, as with all stories, different people see things differently, remember things differently, and draw different conclusions. On top of that, many of these stories have enough complexity and interesting lessons that each could be the worthy subject for an entire book, but here I may summarize them in a paragraph or two.

For example, you will read about Webvan, a company that failed spectacularly after investing nearly \$1 billion based on a vision for a grocery home-delivery company. It's a huge story with lots of interesting subplots and lessons and a cast of thousands from venture capitalists, to the CEO, to hundreds of employees in all sorts of functions (managers, warehouse workers, accountants, truck drivers, etc.). Each of them was involved with, was affected by, and saw the company's failure in different and often contradictory ways. The Webvan investors' story is going to be different from the CEO's story, which is going to be different from that of one of the company's truck drivers or accountants.

In Akira Kurosawa's classic movie *Rashomon*, four witnesses describe the same murder in four mutually contradictory ways. When a new product or company fails, there are as many stories as there are witnesses. The Rashomon Effect is in full swing. Even my own stories and examples—the ones I witnessed firsthand and participated in—are bound to be distorted by my unique perspective, my own biases, and my imperfect memory.

Aiming for 100% accuracy and thoroughness is an impossible task, given the myriad of complex and hard-to-quantify factors that can cause a new product or company to succeed or fail. But we can still extract valuable lessons and rules of thumb that might work for us.

Bottom line: I have selected and summarized examples with the goal of maximum usefulness, impact, and memorability. I took a lot of “to the best of my recollection” stories and case studies and translated them into a form optimized for *your* recollection and benefit. Besides, even if I were somehow able to give a perfect account of each example, I would still warn you to consider it nothing more than anecdotal evidence from other people, at other times, and in different circumstances—what later in the book I introduce as OPD, Other People’s Data.

So take each story you read with several grains of salt.

Terminology

The tools and tactics described in this book apply to all sorts of ideas: ideas for new products (or for new features to be added to existing products); ideas for new services (or for new offerings to be added to existing services); and ideas for new businesses or organizations of all types (for profit, not for profit, commercial, political, philosophical). However, since I don’t care to continually write, and I’m sure you don’t care to endlessly read, “idea for a new product, service, business, or organization,” most of the time I will simply use the word *product* (or *It*, capitalized, as in *The Right It*). In the context of this book, the terms *product* and *It* can refer to a new musical instrument, a mobile app, an innovative diaper-delivery service, a social-media startup (as if we need another one of those), a video game, a genetically engineered hypoallergenic hamster, a new college course or academic discipline (*Failure Science?*), a charitable organization, a new religion or cult (no, please!), and so on.

Similarly, the term *market* as used in the book doesn’t necessarily mean people with wallets, but any group you think will want, use, adopt, or somehow engage with your idea. If you are planning to design and launch a new high-school class, your market is the students. If you are

thinking of enacting a new law to regulate, say, speeding bicyclists in your town, your market is pedaling citizens.

In other words, the tools and tactics covered in the book apply to any undertaking in any market that involves:

1. a nontrivial investment,
2. a high probability of failure, and
3. the desire to avoid that failure.

And that accounts for pretty much all major human endeavors.

Credit Where Credit Is Due

I want to be up front about the fact that my role, and the purpose of this book, is not to present previously unknown facts and completely original ideas. Some of the tools and techniques I share with you have been around for centuries—but they never got the visibility and attention they deserved, nor were they ever given a name. I dug them out, dusted them off, polished them a bit, and put them on display. Think of me as a collector, curator, and guide to these ideas.

Many of the inspirations, examples, and scenarios are source materials I've collected over the past few years as I dedicated myself to developing, practicing, and teaching these tools and techniques to anyone who'd listen. Thanks to Google and Stanford University, I have had the opportunity to give hundreds of talks, seminars, and workshops and to participate in countless hands-on coaching sessions in which I've applied these techniques to real-world projects side by side with students, entrepreneurs, and even Fortune 500 CEOs. These people and organizations not only put these ideas into practice, but suggested new tools and many improvements. They also provided me with relevant stories and accounts from many different industries. So whatever product, service, or business you are planning to pursue, you will find help here.

I Practice What I Teach

Do I practice what I teach? You bet! I practice it not because it would

look bad if I didn't, but because what I teach works and has been working amazingly well for me and for my students and clients. In fact, the tools and techniques you'll learn in this book *cannot not work* because, as you will discover, they are grounded in proven facts and based on simple logic. Master them, put them to use, and you will flip the odds for market success in your favor and win most battles with the Beast of Failure.

This book is a good example of my practicing what I teach. Let me explain. Writing a book is a major undertaking, and I was well aware that most authors never find a publisher or a significant audience for their work. In other words, most books fail in the market.

So, before deciding to invest a year or more in writing a proper book and taking the risk that nobody would read it or find it useful, I did an experiment. I gave myself five days to write a short booklet, which I titled: *Pretotype It: Make Sure You Are Building The Right It Before You Build It Right*.^{*} The booklet introduced the concept of The Right It along with a rough version of some of the tools and techniques you will read about in this book. I printed and manually stapled a few dozen copies of the booklet and handed them out to colleagues and friends.

A couple of days later, I began to receive requests for more copies. "Hey, Alberto, loved the book. Can you get me ten more copies? I'd like to share this with my team. I am happy to pay you for them."

Soon the requests for additional copies outpaced my ability to produce them by hand. So I ordered a big batch from a printing shop, and then another batch, and another. Eventually, tired of trips to the printer and schlepping heavy boxes around, I decided to put a PDF of the booklet online and to make it available for free, so people could print it themselves if they wanted. By request, I also created a Kindle version and put it on sale on Amazon for 99¢ (the lowest price allowed).

A few days later, I began to receive emails from people I did not know telling me how much they liked the booklet and thanking me for writing it. A lot of them encouraged me to write a proper book with more techniques and examples. I also started to receive invitations to give talks

* If you want to check out the original booklet, just search online for "Pretotype It Alberto Savoia."

and workshops on The Right It and pretotyping at major companies and universities. Several people from different parts of the world volunteered to translate the booklet into their language; and I gave them all my permission with the condition that a PDF of their translation must also be available for free. Thanks to their efforts, today there are about a dozen translations of *Pretotype It* available.

The first version of that booklet came out in 2011. Since then, the original PDF and eBook versions (and their translations) have been copied and posted on countless websites, so I have no way of knowing how many people have actually downloaded and read it, but I believe it's in the tens of thousands. And all of this with practically no marketing or advertising, simply by word of mouth.

In life and business, including the book business, there are no guarantees. But the strong initial and ongoing interest in my experimental booklet and the many success stories from the people who applied the lessons and techniques in it were enough to convince me to invest the time and effort required to write a proper book—the book you are currently reading.

Be Daring, but Be Careful

Please make sure to use your best judgment and respect all applicable laws and industry standards in following the examples or applying the tools that I describe in this book. Every organization is different, and the advice and tactics contained herein may not be suitable for your situation or product idea. In fact, some of the techniques may be illegal or unethical for certain types of products (e.g., in medicine) or in certain highly regulated industries (e.g., aviation). If you wish to apply ideas contained in this book, you are taking full responsibility for your actions. Neither the author nor the publisher shall be held liable or responsible to any person or entity with respect to any loss or incidental or consequential damages caused, or alleged to have been caused, directly or indirectly by the information contained herein.

Part I

Hard Facts

1 The Law of Market Failure

I like hard facts. The harder, the better. I like them even when they go against my desires and preferences. I like them because they are grounded in reality, and that grounding gives me a solid foundation—a bedrock I can build upon. Facing and accepting hard facts may feel uncomfortable at first, but that initial distress is nothing compared to the problems and pain that await those who choose to ignore them.

The hard facts we will explore share three dimensions of hardness:

1. They are *hard* to take. They may be difficult to accept, at least at first.
2. They are based on *hard*, objective data, not on tenuous hopes, wobbly beliefs, or volatile opinions.
3. They are *hard* in the sense of being solid, unyielding, and permanent. They are unlikely to change—at least in your lifetime.

The last two characteristics of hard facts, objectivity and permanence, make them universal and timeless, so you better get used to them.

My goal for the first part of the book is to expose you to key hard facts about failure and success. If I do my job well, and if you keep an open mind and a courageous attitude, you will find yourself not only accepting and respecting hard facts, as I do, but also depending on them, valuing them, and seeking them out, as I do. A hard fact is good to find.

Failure Is Not an Option—Not!

“Failure is not an option!” I am sure you’ve heard that phrase before in action movies, motivational speeches, and desperate staff meetings. It’s a great quote. So optimistic. So inspiring. So confident. And yet so wrong.

When it comes to bringing new product ideas to market, failure is always an option. In fact, it is—by far—the most likely outcome anytime anyone tries to do something new or different. This is true in art, in science, in relationships—everywhere. And it’s most definitely true in the world of business. Most new businesses fail, and most new products launched by existing businesses fail.

Telling yourself that failure is not an option may work if you are a hero in a Hollywood action movie or if you are stuck in a jam with only one way out. But if you are planning to launch a new product into the market, to say, believe, or act according to “failure is not an option” is to go against reality—always a bad idea. Those five words may give you an initial boost of confidence or motivation, but that boost will be short-lived and, more often than not, you will be boosted in the wrong direction—straight into the jaws of the Beast of Failure.

I offer you a more realistic formulation:

Failure is the most likely outcome.

Consider this your first hard fact, and remember that hard facts are your friends even if they may not appear so at first glance. As you go through this book, you will see why treating failure as the most likely outcome for any new idea is a much more effective way to think, because it aligns with the reality of the market, and that alignment will ultimately and consistently lead you to more successes in the market.

This first hard fact about failure is so important and so deserving of respect that I believe it's wise to treat it as a *law*.

The Law of Market Failure

I've already characterized failure as a beast, and I will use the metaphor of the Beast of Failure throughout the book because, psychologically, that image conjures up the kind of fears and emotions most people feel when faced with failure. We must not ignore those psychological effects, because they do impact how we think and act. But I also want to present and deal with failure in a more objective and analytical way. When new products are brought to market, failure is the rule, not the exception. Failure is so consistent, persistent, and ubiquitous that it will serve us well to treat it as a law and give it the respect it deserves.* The Law of Market Failure is this:

* I am aware that, technically, calling this a law is a stretch. It's clearly not in the same category as, say, Newton's First Law of Motion. So why do I call it a law? Because I want

**Most new products will fail in the market,
even if competently executed.**

I wrote the law on two lines because it packs a one-two punch:

Punch 1: Most new products will fail in the market.

Ouch!

Punch 2: They will fail even if competently executed.

Double ouch!

I will soon give you hard evidence to back up the hard fact that failure is the most likely outcome for any new product. But before we go there, it's a good idea to clarify what we mean by market failure—and the more elusive market success.

Market Failure and Success Defined

For our purposes, *market failure* means any actual market result from an investment in a new product that is *less than or the opposite of the expected result*.

Let me explain. When you bring a new product to market, you make an investment—an investment of money, time, resources, and reputation. You make that investment with the expectation that you'll achieve specific desirable results: more revenue, bigger profits, a larger market share, new customers, more publicity, and so on. For example:

- Two employees quit their secure jobs and invest their savings to start their own company, *expecting* that they will be happier and/or make more money being their own bosses.
- A company invests in developing a new and improved version of an existing product, *expecting* that it will sell more and be more profitable than the previous one.

you to pay attention to it, respect it, take it into account. I could have called it “a rule of thumb” or “an observation,” but those and other such terms lack the necessary gravitas.

- An expert in a field takes an unpaid sabbatical to write a book in which he shares his expertise, *expecting* that the book will be published and that it will add to his reputation. A publisher decides to publish that same book *expecting* that it will sell well.
- A restaurateur operating a popular and profitable restaurant opens a second location, *expecting* that it will also be popular and profitable.
- A highway department invests in adding new toll-road lanes to a busy freeway, *expecting* that this will ease traffic congestion and generate enough revenue to pay for itself.

If you launch your product into the market and the actual results are less than or the opposite of what you expected, that's what we call a market failure.

Our definition of market success is the flip side of the one for market failure. *Market success* means any actual market result from an investment in a new product that *matches or surpasses the expected result*.

It's important to note that some products can be a market failure but still be considered successful based on other criteria. A movie that garners critical praise but flops at the box office is still a market failure—at least for those who invested in it hoping to make a profit. A new product that does exactly what it's supposed to do, does it better than anything else, but does not sell enough to enable a profitable business may be a marvel of engineering, but it's still a market failure. A new toll road that eases traffic congestion but does not generate enough revenue might be considered a success by commuters, but it's a market failure as far as the taxpayers are concerned. It's important to be explicit about your success criteria before you get started.

Now that we have clear and specific definitions of what we mean by market failure and market success, let's explore in more detail how often, how, and why most new products do fail.

Market Failure Statistics

The first line in the Law of Market Failure states: "Most new products will

fail in the market.” By using the word *most*, I am implying that more than 50% of all new products fail. That’s a safe position to take. I have yet to find an industry in which the majority of new products consistently succeed. This makes perfect sense, because that would indicate an industry or market with infinite demand and resources to absorb a huge number of new products—and there’s no such thing.

But what is the actual rate of failure? Is it 51%, 70%, 95%, 99%? The answer depends on a number of factors, among them the type of business or industry, how many companies and products are included in the study, and the definition of failure.

In the broader consumer-products market, some of the best data on new-product failure comes from the venerable firm Nielsen Research. For decades, Nielsen has been tracking tens of thousands of new product launches. Every year it produces a report on how those products have fared in the market. The results are remarkably consistent: approximately 80% of new products fall short of their original expectations and are categorized as “failed,” “disappointing,” or “canceled”—year after year, with no exceptions.

If you research or talk to people like authors or publishers, mobile app developers, venture capitalists, and restaurateurs, you will hear the same story and get roughly the same 80% number over and over. So if you are looking for a number to replace “most” in the Law of Market Failure, anything in the range of 70% to 90% will do. I suggest you err on the side of caution and begin by assuming a 90% chance of failure for any new product idea.

The statistics on new-product failure are clear, consistent, and convincing. But what’s behind those numbers? Why do most new products fail? Hard facts are easier to accept and deal with if we understand their causes. So let me begin to answer those questions by exposing the logic that explains why failure is a much more likely outcome than success.

The Success Equation

Success depends on a number of key factors. A *factor* is a circumstance, fact, or event that contributes to a result or outcome. And a *key factor* is one that *must* be done right or turn out right for the idea to succeed. Most

results and outcomes depend on the interaction of multiple key factors, and to have a successful outcome all key factors must be done right or turn out right.

To help visualize and analyze this concept we can use a formula that I call the Success Equation:

$$\text{Right A} \times \text{Right B} \times \text{Right C} \times \text{Right D} \times \text{Right E, etc.} = \text{Success}$$

where A, B, C, D, E, etc., are key factors for success.

A competent and well-trained kitchen staff is a key success factor for a new restaurant; let's call it key success factor A. But success also requires finding a suitable space in the right neighborhood (B), good suppliers (C), capable servers (D), sound financial management (E), good marketing and a sufficient marketing budget (F), and competent operations (G). In addition, you need the cooperation of factors that are mostly outside your control, like the overall economic climate, competitors, and reviewers. All of these are key factors. All of these must turn out right for the restaurant to succeed—and that's a lot of *rights* in a row.

On the other hand, all it takes for something to fail is for a single key factor to go wrong—just one!

$$\text{Right A} \times \text{Right B} \times \text{Right C} \times \underline{\text{Wrong D}} \times \text{Right E, etc.} = \text{Failure}$$

$$\text{Right A} \times \underline{\text{Wrong B}} \times \text{Right C} \times \text{Right D} \times \text{Right E, etc.} = \text{Failure}$$

Remember when you first learned in math class that any number, no matter how large and impressive, when multiplied by zero results in zero? The same general idea applies to the Success Equation.

A single bad review from an influential critic in a bad mood can doom a restaurant—whether you've spent \$1,000 or \$1 million in marketing. Any number of success factors done right or gone right can be undone by a single thing done wrong or gone wrong. (For those of you who are mathematically inclined, if a successful outcome depends on n key factors being right, there are $2^n - 1$ ways to fail—and only 1 way to succeed.) With the odds so heavily stacked against them, it's not surprising that most new

products fail. The real mystery—or surprise—is that some products beat those odds and succeed.

This brutal logic explains the statistics behind the Law of Market Failure—but it's also the logic we'll use to beat it.

Too Smart to Fail?

Most people don't argue too much with the first part of the Law of Market Failure. After I provide them with the statistical evidence and the logic behind it, they eventually accept the hard fact that most new products will fail in the market. But I get a lot more resistance to the second part of the law, the part that states that most new products will fail “even if competently executed.”

Very few people are ready to accept that, and I understand their reluctance. That's exactly what I used to think, that failure is the result of incompetence or inexperience at one or more points during the execution of a new product. Unfortunately, competence in execution is not an antidote to failure. That's a hard pill to swallow, but swallow it you must. If you operate under the delusion that experience and competence in a given field or market insulate you from the Law of Market Failure, not only will you still fail, but your hubris will be punished by your failing in an even bigger and more painful way.

In my workshops and classes, I prove my point by showing case after case of dramatic failures by people and companies widely considered the best in their respective markets. Here are some examples.

Coca-Cola and PepsiCo are arguably the two most successful companies in the soft-drink industry. Both companies are world-class experts at making, marketing, and distributing anything bottled and bubbly; they have decades of experience and unparalleled know-how and resources. None of that impresses the Law of Market Failure. Coca-Cola's New Coke—a mid-1980s reformulation of one of the most iconic products in history—involved a massive market-research and publicity effort. But, despite all that work, research, and preparation, New Coke was met with a response that ranged from negative to downright hostile. The company had to rush back to offering the original formulation. PepsiCo experienced

a similar failure with Crystal Pepsi, a coloring-free, caffeine-free alternative to regular Pepsi.

Did you see the movie *John Carter*? If you answered no, you are in good company. The movie was a flop—even though it was a Disney movie with a \$250 million production budget and a \$100 million marketing budget. Disney is no Dumbo when it comes to making and marketing movies; on the contrary, it's one of the most savvy, experienced, and successful movie studios in history. But its competence does not make it immune to the Law of Market Failure. The same applies to filmmaker George Lucas, who followed the hugely successful *Star Wars* with *Howard the Duck*—a disastrous flop.

Google is one of the most experienced and competent companies in the world when it comes to developing and launching internet products. And yet, in 2010, the same company that brought you Google Search, Google Maps, and Gmail, launched Google Wave—a new way for teams to collaborate online. Despite an impressive amount of press, hype, and enthusiasm, Google Wave ran smack into the Law of Market Failure, and the company began to phase it out in a matter of months. The same fate awaited many other high-profile and well-publicized Google products such as Google Buzz and Google Glass. Dozens of lesser known Google products also failed or fizzled out—ever heard of or used Jaiku or Google Answers?

In my classes I like to ask students to name all the Google and Microsoft products they use or know about. Then I show them a list of Google and Microsoft products that failed. My list is always at least five times longer than theirs. If you search online for “Google failures” or “Microsoft failures,” you will find several compilations of the companies’ many unsuccessful products—including a Pinterest page called the Google Graveyard and one called the Microsoft Morgue.

Of course, most companies don’t advertise their failures—they bury their dead quietly and move on. But if you dig a little, you will find that you can pick any successful company and create the equivalent of the Google Graveyard or Microsoft Morgue for it. For example, the McDonald’s Mortuary might include the ill-fated McLobster, the Hula Burger (with a slice of pineapple instead of meat), and McSpaghetti—which, as an Italian, I find particularly disturbing.

Competent companies that are more aggressive and more innovative will easily have a ratio of failed to successful products of 5 to 1, if not more. But even more conservative and competent companies will fail far more often than not. The most striking thing about these failed products is that the people, resources, and companies behind them are not only competent, but often the best in their field—with decades of experience. The Law of Market Failure is blind.

The hard fact to take away is that, although experience and competence are necessary to achieve lasting success with a product that the market wants, they are powerless when applied to a product that the market is not interested in. In fact, experience and competence often result in an even bigger and more public failure, because we tend to make a bigger investment and set unrealistically high expectations.

Failophobia

To summarize, most new products will fail, failure can happen in many ways, and competent execution is not enough to defeat the Law of Market Failure. But what is the primary reason for all these failures? Can we identify a major culprit—enemy number one—so that we can maneuver to avoid it?

I had such a culprit in mind, but I did not want to draw general conclusions based only on my experience. So after my first major experience with failure I decided to return to Google, get my old job back, and, while working on new projects, learn more about failure. Fortunately, Google not only took me back, but welcomed me with open arms and gave me a great opportunity to study failure. The company had become very interested in how failure affected its ability to innovate and launch new products and asked me and a few others to investigate the problem and propose some solutions. I could not have asked for a better setup.

As a company, Google has a relatively high tolerance for failure. But although the Beast of Failure did not intimidate the company as a whole, it scared the heck out of most individual employees. Google, the company, understood and accepted that failure is an inevitable by-product of innovation. But most Google employees preferred to work on already success-

ful and well-known products; they did not want to risk failure by joining a group working on a new and unproven idea. They wanted to tell their friends and family, “You know Gmail? I work on that!” as opposed to, “I am an engineer on project SnarfBlatt.”

This deeply human desire for being associated with success and avoiding failure—*failophobia*—is why many already successful companies have trouble staying innovative even when they, as organizations, are prepared to accept some failures. At the company level, one really successful new product, like Gmail, can easily erase dozens of failures, like Google Wave, but any individual employee will have two or three years of work on a failed product on his or her own résumé. While working at Google as engineering director, I witnessed this behavior every time I tried to recruit engineers or product managers for key roles in new and exciting but risky projects. Because of failophobia, most candidates chose to accept lesser roles working on already established successes.

But although many Google employees wanted to avoid new failures, they had no problem talking about their *past* failures. In fact, they loved to talk about them. Many of these discussions reminded me of the scene from the movie *Jaws* where the characters played by Robert Shaw and Richard Dreyfuss share their shark-attack stories, taking pride in their battle scars and trying to one-up each other: “If you think your failure was bad, wait until you hear my story!”

It was fascinating to hear these smart and competent people recount their failures. But I cared mostly about the answer to one question: *Why* do you think your product failed?

FLOP

After interviewing scores of people about their experiences with failure, a clear pattern emerged. Most projects failed for three reasons:

Failure due to Launch, Operations, or Premise

Which gives us the appropriate (and easy to remember) acronym FLOP.

Failure due to Launch happens when the sales, marketing, or distribution efforts accompanying your new product fail to reach the intended market with the necessary visibility or availability. In other words, too many of the people who are supposed to need or want your product or service (i.e., your intended target market) don't know it exists, don't know enough about it, or can't get their hands on it. It may be the greatest idea ever, beautifully executed, and a perfect solution to a major problem, but if you can't get the word or your product out to your target market, it will fail.

Failure due to Operations occurs when your new product's design, functionality, or reliability fails to meet a minimum threshold of user expectations. For example, a beautiful-looking but uncomfortable chair, a restaurant with great food but lousy service, or a mobile app that constantly crashes. You may be able to get a few early adopters to buy or use your product or service, but inevitably, if your implementation of the idea is not good enough, word will get out and it will fail.

Failure due to Premise happens when people are simply not interested in your idea. They know about it, they understand it, and they believe that it does what it promises reliably and efficiently. They can also easily find it, try it, or buy it, but they just don't care.

Through my interviews, I identified these as the three main reasons most products fail in the market, but something about these answers bugged me. People's accounts of the failures involved an initial pattern of blaming, which muddied the results. When a project fails, people point fingers. In a high-tech project, for example, engineering blames marketing, marketing blames engineering, everyone blames sales, and sales blames everyone. Similarly, if a restaurant fails, people point the finger at the chef, or the service staff, or the marketing team, or even the decorator. But when I pressed my interviewees to get past the blame game, when I asked them to seek a more fundamental root cause for the failure of their project, they realized that most of the people involved were competent—often even exceptional—in their respective designing, building, marketing, and selling

responsibilities. There may have been some launch and operation issues, but those were not the root cause for the failure.

Once they got past the finger-pointing, most people had a similar epiphany: “I guess that, after all is said and done, we did a pretty darn good job building and marketing our product, but it was a product that not enough people really wanted or needed. I’ll be damned!” After the fog of blame lifted, one root cause emerged above all others: Premise. A small percentage of products fail in the market because they are poorly launched or built; the majority fail because they are the wrong product idea to start with. I had found my enemy number one, the most common reason behind the market failure of new products.

This result came as a surprise to most people—including me. I had always spent most of my time and energy on a new product making sure that we were building it right (robust, high-quality, scalable, with lots of features) and then on marketing and selling it right. But because we had assumed from the start that we were building the right product, more often than not all that time and effort and competence went into a product that in fact turned out to be wrong for the market.

Most new products fail not because of incompetence in designing, building, or marketing, but simply because they are not what the market wants. We build *It* right but we don’t build *The Right It*—a product that enough people want or need to justify developing it.

I summarized that realization in the following sentence, which has become my mantra and the reason and motivation for this book:

**Make sure you are building The Right It
before you build It right.**

2 The Right It

The Right It is not just the title of this book. It's the star of this book. It's what it takes to beat the Law of Market Failure—in fact, it's the *only* thing that can beat it. So let me take some time to clarify and expand on what I mean by The Right It. I begin by defining it. *The Right It is an idea for a new product that, if competently executed, will succeed in the market.*

In business, there are no good ideas or bad ideas; there are only ideas that succeed in the market and ideas that fail in the market. As we have seen, most ideas will fail—even if competently executed. The minority of ideas that become a market success have one thing in common: they are *The Right It*. In other words, take an idea that is The Right It, add competent execution, and the idea will succeed in the market.

Does that mean that if *you* combine an idea that is The Right It with *your* competent execution, *you* are guaranteed success? Sorry, it doesn't work that way. First of all, there are no guarantees in business. Second, the definition says that the *idea* will succeed, not *you*. It's always possible that someone else will execute the same idea better or faster than you. This happens all the time. In fact, once the market for an idea is proven, other people will realize that the idea is The Right It and jump in and try to do it better—or just differently. Pizza is a great example of an idea that is The Right It (possibly my favorite example); all it takes is a ten-minute drive around town to see how many companies, both large and small, have jumped in to grab a slice—ahem—of the pizza market.

Even though success in business cannot be guaranteed, if you are working with an idea that is The Right It, your odds for success will increase dramatically. And if you apply the tools and tactics you will learn in this book, you will be able to determine if an idea is likely to be The Right It quickly and reliably. You might still fail in the end if you don't execute well or competitors execute better, but when compared with the odds you have when dealing with The Wrong It, this is a huge advantage. Speaking of The Wrong It . . .

The Wrong It

The Wrong It is the evil twin of The Right It. I define it as follows: *The Wrong It is an idea for a new product that, even if competently executed, will fail in the market.*

Every time a team of experienced and competent people work hard to develop and launch a high-quality new product and that new product fails in the market, they've fallen victim to The Wrong It. The basic Premise for their idea (remember the P from FLOP) is out of sync with the reality (the actual needs and wants) of the market. No amount of competent execution (brilliant design, clever engineering, impeccable quality, masterful marketing, or superior salesmanship) can save a product that is based on the wrong Premise. In fact, the more time and effort you invest in The Wrong It, the longer, bigger, and more painful your failure will be. Going to market with an idea that is The Wrong It is a hopeless task. Even if you manage to create some initial buzz and interest for the product (perhaps with some marketing fireworks), your odds of *long-term* success are still 0.0%.

Now that we've defined and explained both The Right It and The Wrong It, we should answer two key questions:

1. Why do so many presumably experienced people fall in the trap and waste their experience and competence executing The Wrong It?
2. How can we know if an idea is The Right It before we invest too much in developing it?

I will answer the first question in the next few pages; the second question is the focus of **Parts II** and **III** of this book.

Thoughtland

How is it possible that so many competent and successful people and organizations invest so much time and energy in developing products that, more often than not, fail in the market? Shouldn't these experts know better? Don't they know that they need to do some market research before

pulling the trigger on a new product? Why do they keep falling into The Wrong It trap?

Once again, I set out to answer these questions by interviewing dozens of people from different industries and asking them a series of pesky questions:

How do you know if the product you are currently building is something that the market wants? . . . I see, you do market research. And what market research techniques do you use? . . . Interesting. And how well have those market research techniques worked for you in the past? . . . Ouch, that doesn't sound like a winning record—more misses than hits. By the way, how much did you spend on that research? . . . Wow, that's a bunch of time and money to get such unreliable results. . . . You agree? Then why do you keep using those techniques?

What I learned from these interviews is that most successful people and organizations are fully aware that having the right product premise is critical for market success (duh!). To make sure that they have the right product, they invest significant time and money on market research. And yet most of those products still fail. What's going on?

After digging up for autopsy a lot of product corpses that failed despite extensive market research, I identified a recurring—and problematic—pattern: most of the so-called market research for those products was not done in the actual market, but in a fictional environment I call Thoughtland. Thoughtland is an imaginary place where every potential new product begins life as a simple, pure, abstract idea. Think of it as an idea hatchery. So far, so good.

The problem arises when an idea spends too much time in Thoughtland after it's hatched. When that happens, the idea starts to attract opinions the way a ship's hull attracts barnacles. Some people think the idea is great. Others think the idea is lame. Even so-called experts disagree. Opinions about an idea are not data—not even close. Opinions are subjective, biased judgments. They are guesses thrown out without much thought, evidence, and—critically—with no skin in the game (if you

are not sure about what I mean by “skin in the game,” stay tuned, as I will discuss this important concept in detail later). If an idea spends too much time in Thoughtland, it gets wrapped in a fuzzy ball of unsubstantiated off-the-cuff judgments, beliefs, preferences, and predictions.

You cannot determine if an idea is The Right It just through thinking. Not through your thoughts. Not through the thoughts and opinions of others. Not through the thoughts of “experts.”

You are no Nostradamus. Nor am I. Nor is anyone else. At best, our predictions turn out to be right *some* of the time—but that’s mostly luck.

The Right It cannot be deduced or induced while you stay in Thoughtland. It has to be discovered through experimentation in the real world. And yet most market research is Thoughtland-based—and that’s bad news. To better explain what I am talking about, let me illustrate why Thoughtland-based market research is dangerous using one of the most common market research tools: the focus group.

Hocus-Pocus Focus Groups

In case you are not familiar with marketing focus groups, let me give you an example that illustrates why and how they are used. I like beer, and it goes well with another one of my favorite examples, pizza, so I will use it for this example, but the process is the same for all products and services.

Let’s pretend that Alberto’s Beer Company (ABC)—a successful brewing company run by highly experienced beverage executives—wants to capture a greater share of the female drinkers market. To better understand this gender-specific market, ABC decides to use focus groups. It begins by bringing groups of women drinkers into a room (often equipped with a one-way mirror) and asking them a series of questions, such as:

When you choose a drink, how often do you choose beer?

When you choose a drink other than beer, what are the reasons for it?

What would it take for you to choose beer more often?

Then it compiles the results and comes up with a list of “insights” that looks something like this:

- 55% of focus-group participants said they prefer white wine to beer, because they think it’s a more appropriate drink for a lady. Sample quote: “Telling the bartender ‘Bring me a Bud’ does not sound very ladylike.”
- 31% agreed with the comment that light beer tastes “too light” and “flavorless” and nonlight beers are usually “too heavy” or “too bitter.”
- 38% said that they would be more likely to order beer if there was a brand that looked and tasted more “feminine.”

Armed with this “data,” ABC comes up with a new product idea, LadyLike, a light yet flavorful beer packaged in a slim bottle. The executives love the idea and give the go-ahead to brew a few batches of the beer (perhaps with a few flavor variations), design a fancy new bottle and logo, and run a second focus group to see if ABC is on the right track.

In this second focus group, the participants are introduced to the potential new brand, they get a chance to taste it, and then they are asked another series of questions:

If LadyLike beer were available, how likely would you be to choose it over white wine?

If you could choose between LadyLike and a regular light beer, which one would you choose?

Did you prefer the peach-flavored or the cantaloupe-flavored LadyLike?

Once again the results of the focus group are compiled:

- 47% of participants who normally choose white wine said that they would order LadyLike if it were available.

- 54% said that they'd choose LadyLike over regular light-beer brands.
- 82% preferred the peach flavor to the cantaloupe flavor.

Compelling results. The ABC executives' eyes light up as they imagine a double-digit growth in market share. They give LadyLike the go-ahead with a multimillion-dollar marketing budget to launch the product—and start thinking about how they are going to use their bonus money.

Nine months later, accompanied by a multimillion-dollar multimedia marketing blitz, LadyLike beer hits bars and store shelves. But a few months after that, the majority of that initial shipment is still on those shelves. The few six-packs that managed to make it into household refrigerators are still there—with just one bottle missing. Despite all that market research and publicity, few women tried LadyLike, and even fewer came back for seconds. LadyLike's slogan should have been: "One sip and that's it." The Law of Market Failure strikes again.

The market focus group turned out to be a hocus-pocus group—a magic trick that made months of work and millions of dollars disappear in a puff of smoke.

If I seem particularly harsh in my depiction of focus groups and similar Thoughtland-based market research and market-survey methods, it's only because I've heard too many stories from people who have been badly burned by such studies. They hired the best research firms, spent huge sums of money with them, and a few months later got impressive-looking reports—which pointed them in the wrong direction. I have fallen into the Thoughtland-based market-research trap a couple of times myself, and each time it cost me and my investors years of wasted effort and millions of dollars.

Even if Thoughtland-based market research could produce more dependable results—and that's a gargantuan *if*—it still would not be my first choice, because there are, as you shall see, faster, cheaper, and far better ways to get the data we need. You can use the heel of an expensive Italian shoe to drive in a nail, but why abuse a Ferragamo when you have a proper hammer at your disposal?

If your personal experience using this kind of market research (or

using shoes as hammers) is more positive than mine, good for you—don’t stop on my account. But augment it with the tools and techniques I am about to show you. In other words, run two parallel market-research efforts using both methods. If the results from the two approaches don’t agree, it means that one of them is *lying* to you. When people need to make a major medical decision, they usually seek a second and even a third or fourth opinion. I suggest you do the same with your product decisions.

As you might have guessed by now, I am rarely satisfied by just knowing facts; I want to understand the reasons and the mechanics behind those facts—the root causes. Especially when those facts run counter to common wisdom and practices. In this case, I wanted to understand why this Thoughtland-based market research—which is so common and seems so sensible on the surface—can produce such unreliable and untrustworthy results. So, once again, I went in search of answers.

The Four Trolls of Thoughtland

Thoughtland-based market-research tools such as focus groups and market surveys are a big business. It’s not unheard of for companies to spend hundreds of thousands of dollars—even millions—on this type of research for a single new product. And, to be fair, well-planned and -executed market research of this kind can occasionally provide you with *some* interesting insights. But be very careful of how much weight you put on those insights, because these are Thoughtland-based tools and, because of that, they are subject to a variety of mental traps.

The Beast of Failure does not work alone; it benefits from the mischievous doings of some devoted little helpers. These impish troll-like creatures dwell in Thoughtland where, through their trickery, they create all sorts of havoc with our ideas. Four of the most common problems caused by these mind-dwelling mini-monsters are:

1. The Lost-in-Translation Problem
2. The Prediction Problem
3. The No-Skin-in-the-Game Problem
4. The Confirmation-Bias Problem

Let's look at each of them in some detail.

The Lost-in-Translation Problem

The first problem we face in Thoughtland is one of communication. Until it's made concrete or tangible in some form, your idea for a new product or service is just an abstraction. It's something that you imagine or picture in your head in your own unique way. The moment you try to communicate what you see in your mind's eye to someone else, you run into a challenging translation problem—especially if your idea is new and different from anything else they've seen.

This problem stems from the fact that the way you imagine the new product and its uses may be completely different from the way other people will imagine it after you describe it to them. Their interpretation of your idea is going to be distorted by their own mental trolls: their personal beliefs, preferences, and prejudices. Not only is their understanding of the idea itself likely to be different from yours, but they will *judge* the idea within the context of their unique mental model of the world.

When I first heard about the car service Uber, for example, I was seriously skeptical about its chances for success. This is how I saw and judged the idea in my head:

You mean, strangers get into another stranger's car? Not a taxi with a licensed and professional driver, but just any car with any driver? Who's going to go for that? "Don't get into a stranger's car" is the first thing that my mother taught me! This is a crazy idea. It'll never work, and I'd never use it.

In my own mind, Uber was about as unsafe as hitchhiking (for both the drivers and the passengers). Even as it gained in popularity, I thought that it would become at best a niche option for a small market—definitely not a threat to taxis, limos, or public transportation. A few months later, a friend convinced me to give Uber a try on my way back from the airport: “I bet you that you’ll never set foot in a taxi again!”

I downloaded the Uber app, and a few minutes later I was sitting in a black Toyota Prius driven by a friendly, chatty twentysomething who

offered me candy and bottled water and got me home safely for half the cost of a taxi. Since then, Uber has become my default choice when I need a ride. A few years after I first taught my daughter never to get into a stranger's car, I told her that she should try Uber. Her reply? "Dad, I've been using Uber for months." So much for parental advice and preconceived notions.

The Prediction Problem

Even if you manage to successfully communicate your idea without major distortions caused by the translation troll, you will run into another serious issue. People are notoriously bad at predicting whether they would actually want or like something they have not yet experienced—as well as how or how often they would actually use it.

I was a teenager living in Italy when I first heard about sushi. A friend had returned from a trip to Japan and described dishes of raw tuna, salmon, eel, and even shrimp. I thought he was pulling my leg; uncooked fish sounded like a disgusting idea. But now I love sushi, and I try to eat it at least once a week.

Let's go back to Uber for another example of the Prediction Problem. Even though I had come to terms with the whole idea of jumping in a car driven by a stranger who is neither a taxi nor a limo driver, I initially thought I'd be using the service exactly the same way I was using taxis and limos—once or twice every few weeks. Wrong prediction—way wrong. I find myself using Uber three to four times more often than I've ever used taxis.

In another outcome I had not predicted, my daughter thought that using Uber all the time might prove easier and be more cost-effective than having her own car with all the traffic and parking hassles of San Francisco. To validate this Thoughtland-based scenario, she decided to run a six-month test to see if and how much she would miss having her own car and how the cost of using Uber would compare to the cost (insurance, maintenance, gas) of having her own car. She parked her Toyota in our driveway, dropped off her keys, and took an Uber back to her apartment. Six months later, she had the data she needed to make an informed deci-

sion. Much to her surprise and ours, she sold the car and, to this day, she has no plans to buy another one.

Bottom line: as a species, we suck at predicting if, how, and how often we might use a new product or service.

The Skin-in-the-Game Problem

The concept of “skin in the game” is central to this book; you will see it come up several times. In case you are not familiar with the expression, *skin in the game* refers to the idea of having a vested interest in an outcome—something to lose or gain. For example, it’s one thing to encourage your entrepreneur friend to quit her cushy job to start a new company because you think she will be hugely successful; it’s a very different thing if you back your encouragement by offering to invest \$10,000 of your own money in her new company. With \$10,000 of skin in the game, you will lose your investment if your friend’s business fails.

People love to give their opinions and advice; most of us do that without much thought if we have no skin in the game—because we’ve got nothing to lose or gain either way. Going back to the ABC LadyLike beer focus-group example, one of the main problems with this type of market research is that the participants don’t have any stake in the outcome. If a focus-group participant gives enthusiastic responses to the survey questions and ABC fails with LadyLike beer, it’s no foam off her nose. I’ll have a lot more to say about skin in the game later on in the book.

The Confirmation-Bias Problem

The first three problems challenge the validity of the information we collect. This last one is a problem with how we interpret that information.

The term *confirmation bias* refers to our (very human) tendency to seek evidence that confirms our existing beliefs or theories and to avoid or dismiss anything that runs counter to them. In other words, not only do we fail to look for objective ways to gather information, but we also fail to look objectively at the information we do get. We pick and choose and give more weight to bits of data that confirm our beliefs and dismiss those

that run against them. That's why in the US, for example, most conservatives follow conservative news channels and most liberals follow liberal news channels.

Most people don't like to have their beliefs challenged, let alone be proven totally wrong. Confirmation bias can and does affect the way we design our experiments, interpret the results, and come to conclusions. As cognitive and mathematical psychologist Amos Tverski put it: "Once we have adopted a particular hypothesis or interpretation, we grossly exaggerate the likelihood of that hypothesis, and find it very difficult to see things any other way."^{*}

When the Trolls Team Up

Every one of these four basic problems can by itself lead us to reach a wrong conclusion, and here's what happens when they are combined:

First, the original idea is distorted in translation.

Then the distorted version of the idea is viewed and judged through each person's unique experiences and biases.

Next, a no-skin-in-the-game opinion is issued.

Finally, those no-risk opinions based on biased judgment of a distorted version of the idea are carefully selected and interpreted to confirm what we wanted to believe all along.

Bottom line: instead of dependable, objective, and actionable data, Thoughtland coughs up fur balls of subjective, biased, misguided, and misleading opinions.

* The problems that make Thoughtland-based market research so undependable for predicting market interest in a product belong to a family of cognitive errors and biases that are themselves the subject of much research. The book *Thinking, Fast and Slow* by Daniel Kahneman (New York: Farrar, Straus and Giroux, 2011) is a great resource if you need more evidence and examples of how our seemingly rational brain can fool most of us most of the time.

It's true that sometime these fur balls of opinion align and reflect the reality of the market—even a broken watch displays the correct time twice a day. But, more often than not, Thoughtland produces false positives and false negatives, that is, results suggesting the presence of a market that is *not* actually there or results suggesting the absence of a market that *is* actually there.

Thoughtland and False Positives

Thoughtland produces a false positive when your idea for a new product collects enough positive opinions and predictions to convince you that the idea is worth pursuing—perhaps even worth pursuing quickly and going all-in before someone else beats you to it. Armed with enthusiasm and a can't-fail attitude, you make a big investment to develop the idea, and a few months (or years) later you launch a beautiful and competently executed new product. And then . . . nothing. All those glowing opinions and golden predictions, all those promises of “If you build it, I will come (or I will buy it, use it, adopt it)” never materialize—at least not in the numbers you were expecting.

How common are false positives caused by Thoughtland? Based on the hard fact that most new products fail in the market, I'd say they are as common as roaches in New York City. Whenever you read about the surprising failure of an exciting and promising new product, chances are that you are reading about such a scenario. And, like roaches, for every such story that you read about, you can bet that there are hundreds more that you never see.

Dramatic examples of false positives can be found in every industry and business, but I've chosen a particularly juicy one, an oldie but goodie, to illustrate my point: a not so little startup called Webvan.

In the late 1990s, around the time when Amazon (a great example of The Right It built right) was beginning to seriously disrupt book and CD retailers with its new online store, a group of smart, successful, and experienced people were convinced that the grocery business was ripe for an Amazon-like disruption. It sounded like a sure bet. After all, most households spend considerably more on groceries than they do on books

or CDs. Furthermore, since shopping for cauliflower and cheddar cheese at a supermarket is far less fun and exciting than shopping for books or CDs, it was logical to expect an even faster and deeper market adoption.

It makes perfect sense. I love hanging around and browsing at bookstores, and I look forward to the experience—not so with supermarkets. Overall, the online grocery business appeared to be a once-in-a-lifetime opportunity, with a market that was considerably bigger and even more compelling than the market Amazon was in. Or so Webvan thought . . . in Thoughtland.

Based on that, Webvan's founders decided to create a new company that would make it easy for people to order groceries online and have them delivered to their home at a specified time by a fleet of vans. Almost everyone who heard about the idea—business analysts, grocery-business consultants, internet pundits—agreed that this was a huge market opportunity. More important, most of the potential customers interviewed had the same enthusiastic response: “That would be awesome. I hate shopping for groceries, waiting in line, schlepping them to my car, and so on.”

In other words, lots of excitement, promises, and hot air, but no skin in the game. By the way, as a potential customer, I had exactly the same reaction as most other people. I was already addicted to Amazon, and I couldn't wait for Webvan to launch. I had predicted not only that Webvan would be a huge success, but that our household would make the shift and do most of our grocery shopping online.

As far as Thoughtland was concerned, there wasn't a single cloud on the horizon. All Webvan had to do was execute well—but also fast, before someone else jumped on this can't-lose opportunity. After securing an initial round of funding of over \$100 million from some of the best VCs in the industry, Webvan went on a turbocharged hiring, shopping, and building spree. It recruited hundreds of people, signed up dozens of partners, bought or built huge refrigerated warehouses, and of course purchased an impressive fleet of vans and trucks, which were painted beige with a huge company logo on the side. Eventually Webvan raised—and spent—over \$800 million.

You can guess where this is going. Webvan launched with great fanfare and publicity. But somehow all those Thoughtland promises of

masses of consumers forgoing checkout lines to do their regular grocery shopping online never materialized—at least in nowhere near the numbers predicted by their Thoughtland-based research. For whatever reason, even though the internet proved great for selling books and CDs, it did not, at least at the time, prove to be a great channel for grocery shopping.

In 2001, roughly two years after it started operations, Webvan filed for bankruptcy. Some of the company's beige vans, which were sold at bankruptcy auctions, can still be seen driving around Silicon Valley. You can even make out a faint outline of the Webvan logo—a ghostly reminder of what can happen when you put too much trust and spend too much time in Thoughtland.

Thoughtland and False Negatives

We have seen how false positives can convince you to overinvest in a new product idea that turns out to be The Wrong It. False negatives have the opposite result; they convince you to abandon an idea that would have turned out to be The Right It.

Here's how the false-negative scenario typically unfolds. You have what you think is a great idea: a new way to solve a common problem, a new market opportunity, a great plot for a thriller. Barely able to contain your excitement, you take your idea on a road trip through Thoughtland. You share it with family, friends, possible partners or investors, potential users—anyone who will listen. You explain your idea with the enthusiasm of a freshman cheerleader who has had one Frappuccino too many. But instead of sharing your vision and excitement, most people don't get your idea: "Why would anyone want to use that?" "That's a stupid idea." "Keep your day job." And so on.

At first, you manage to absorb a few blows. You remember that line from Rudyard Kipling's poem "If": "If you can trust yourself when all men doubt you." So you pick yourself up and keep going. But after a few more poundings, you remember the next line from the poem: "But make allowance for their doubting too." You allow some doubt to creep in and, soon after, you decide to drop your idea and wonder what on earth made you think that something like that would even work.

A year or so later, you read about someone else succeeding with that very same idea. Thoughtland has produced another false negative and claimed another victim.

If you are reading this book, you are probably the type of person who regularly comes up with ideas for new products and businesses, and I bet that false-negative story I just described sounds familiar to you. I've been through this scenario myself too many times to count. On some occasions, I was the one with the idea being dismissed and ridiculed; on other occasions, I was the one doing the dismissing. Let me share more examples with you.

When I was offered the opportunity to join a little-known startup called Google as the company's first engineering director, almost all my friends and former colleagues thought it would be a bad move. In their opinion, there were already several well-established search engines (remember Alta Vista?) and other, even better portals to the web such as Yahoo! In other words, that market was already spoken for and saturated.

I ignored their opinions and joined Google anyway. A few months later, I tried to get a good friend to join me and work in the Google ads team. I told him that we were building one of the most amazing money-making machines ever developed and that in a few years it would generate billions of dollars. His response was: "It may make a few bucks, but I don't think it will be big at all. I never click on online ads."

Another friend I tried to recruit chose a lesser position at Yahoo! In his opinion, Google was a minor internet player and would remain one. He thought that Google's minimalistic home page was a stupid waste of valuable screen real estate compared to Yahoo's everything-but-the-kitchen-sink landing page and that its PageRank algorithm was inferior to Yahoo's manually curated results.

In Google's case, I made the right bet, and those two friends are still licking their wounds. But I've been on the wrong side of the fence many times myself. Here's a short list of relatively recent ideas I snickered at when I first heard them, along with my initial reaction:

Twitter: Who'd want to follow people like me? Heck, I don't even want to be followed. And what's with that 140-character

limit anyway?

Uber: No thanks. If I can't find a taxi and can't afford a limo, I'd rather take the bus than hop in a car driven by an unlicensed stranger. What's next, go spend the night in a stranger's bedroom?

Airbnb: You mean that people rent rooms to virtual strangers for the night? I can't imagine enough people willing to open their home like that or enough people willing to go to sleep in a stranger's home. Have you ever watched a horror movie?

Tesla's original Roadster: \$120,000 for an all-electric two-seater that runs on batteries? I can buy a Porsche or a used Ferrari for that kind of cash.

I could go on for pages, but you get the point. I should add that I was far from alone with my negative initial opinion about the aforementioned ideas and companies; most of the people I talked to had similar reactions. It happens all the time, to most new ideas.

Just this morning, I read that Amazon agreed to buy Ring,^{*} a company that makes wi-fi-enabled video doorbells and other home security products. The acquisition price has not been disclosed yet, but it's rumored to be in the neighborhood of a billion dollars. Why do I mention Ring? Because just few years earlier Ring's founder and CEO, Jamie Siminoff, couldn't convince anyone to invest in his video doorbell idea. He couldn't even get a deal on the popular TV show *Shark Tank*. In fact, one of the show's savviest investors and an expert in television infomercials, Lori Greiner, the "Queen of QVC TV," made the comment: "You'll never be able to sell this on QVC."

In a recent QVC appearance, Siminoff said that he sold 140,000 units of his video doorbell (\$22.5 million worth) in twenty-four hours—one of

* Ali Montag, "Amazon Is Buying Ring, a Business That Was Once Rejected on 'Shark Tank,'" CNBC, February 27, 2018, <https://www.cnbc.com/2018/02/27/amazon-buys-ring-a-former-shark-tank-reject.html>.

the most successful QVC sales of the year. Oops. Add to that a couple of other million units sold through various outlets, and what we have is another spectacular false negative courtesy of Thoughtland. If a tank full of sharks can get it so wrong, what chance do we little guppies have?

Escape from Thoughtland

When you combine the Law of Market Failure and Thoughtland, chances are that you will be a victim of one of two possible scenarios:

- Disregard for the Law of Market Failure combined with false-positive responses from Thoughtland leads you to overinvest in The Wrong It—an idea that is destined to fail.
- Fear of failure combined with false-negative feedback from Thoughtland stops you from pursuing an idea that has the potential to be The Right It—an idea that, if competently executed, is destined to succeed.

As I've already mentioned, opinions from Thoughtland and the real world do align sometimes. False positives and false negatives are the norm, but *true* positives and *true* negatives do happen.

Sometimes, an idea gets an enthusiastic response in Thoughtland, is launched based on that, and succeeds. "I knew that this was going to be big!" And sometimes an idea gets slammed in Thoughtland, is executed despite the negative reactions, and flops miserably. "Told you so. Do you think you can get your old job back?"

How can you know if the negative or positive responses you are getting from Thoughtland are true or false? My conclusion is that you can't. Given the Lost-in-Translation Problem, the Prediction Problem, the No-Skin-in-the-Game Problem, and the Confirmation-Bias Problem, there are too many ways to misjudge an idea's likelihood for success.

But if you can't trust your opinions, other people's opinions, or even the opinions of experts, how can you decide if the idea you have and would like to develop is likely to succeed?

You need data!

3 Data Beats Opinions

The title for this chapter, “Data Beats Opinions,” comes from one of Google’s key operational principles. I had always believed that, being a rational guy, I had been making most of my work-related decisions based on data and hard facts. It wasn’t until I started working at Google in 2001 that I realized how much my own opinions, preferences, and biases affected my decision process. I won’t go as far as saying that opinions carried zero weight at Google, but after a few meetings I learned that if I could not support my opinions with sufficient objective data, I had little chance of winning arguments or convincing my colleagues.

Not only that, but I also learned that the company’s data-driven decision process was very exacting. What most people would consider data did not pass muster at Google. In order to get serious consideration in the decision process, the data had to satisfy a number of key criteria:

Freshness: The data has to be fresh—the fresher the better. That’s because what was true a few years (or months or weeks) ago may not be true today. This is particularly important in high-tech businesses and the online world, where people’s attitudes and expectations change the most rapidly. In the late 1990s, for example, one of the performance rules of thumb for websites was that a page had to load in eight seconds or less. Data from a widely publicized study showed that if a web page took longer than eight seconds to load, at least 50% of the website visitors would lose patience and leave the site.

These days, those eight seconds would feel like an eternity to 90% of users. We expect web pages to load instantly, and if they take more than a couple of seconds, we are gone. The “eight-second rule” became the “two-second rule,” and in a few years it will probably be the “half-second rule.” Some types of data spoil faster than a banana left in the back seat of a hot car; others remain valid longer. Unfortunately, unlike ripe bananas, aging data does not develop brown spots and go mushy to alert you, nor does it come with a helpful expiration

date. So it's up to you to be careful with the data you choose to use. If in doubt about its freshness, throw it out.

Strong relevance: The data must be directly applicable to the specific product or decision being evaluated. This may sound like an obvious criterion, but you'd be surprised how often data with weak relevance can trickle into the decision process. The fact that, say, most McDonald's customers would not order onion rings with their burgers even if they were offered does not mean that you should not include them on the menu for *your* idea for a burger food truck.

Known provenance: You should not rely on data collected from other people, in other organizations, or for other projects to make your decisions. Who knows what methods those people used to collect and filter the data? And who knows what biases, influences, and motivations may have affected them when they compiled and summarized the data? The "eight-second rule" and other similar studies mentioned before, for example, were sponsored and publicized by companies that sold products and services to accelerate website performance; so they had a vested interest in showing data that supported their business offering. Make sure you know where your data comes from and how it was collected and filtered.

Statistical significance: The data must be statistically significant. It must be based on a sufficiently large sample to ensure that the result cannot be attributed to chance. And unless you want to be humiliated in front of your colleagues, don't try to present personal experiences or one-off stories as data. I made that mistake twice in my early tenure at Google, and both times I was quickly reprimanded with a chorus of "Anecdotes are not data."

To be clear, nobody at Google sat me down and formally walked me through such a list of criteria. But after a few meetings, I learned that the term *data* in "data beats opinions" meant *fresh, relevant, trustworthy, and statistically significant data*. I also learned that the quickest and most re-

liable way to get that kind of data was to collect that data myself. As a result, I developed a deep-seated distrust of Other People's Data.

Other People's Data

You should not rely on Other People's Data (OPD) to determine whether your idea is likely to succeed in the market. It's a tempting, but lazy and dangerous shortcut.

First, let me define what I mean by Other People's Data. OPD is *any market data collected and compiled by other people, for other projects, at other times, in other places, with other methods, and for other purposes*. OPD violates one or more of the criteria of freshness, relevance, trustworthiness, and significance that we've just outlined. The data derived from the experiments, actions, and decisions of other people working on ideas similar to yours can be used to *supplement* and *inform* your own actions and decisions. But it's *not sufficient* and it should *not be a substitute* for collecting your own data. Let me explain why.

Whenever you think you have a new idea for a business, product, or service, there are five possible scenarios:

1. You are the first person in history to come up with that idea. There's nothing in the world like it.
2. Other people have had the same or a similar idea and
 - a. they chose not to pursue it.
 - b. they are actively pursuing it, but have not launched it yet.
 - c. they pursued it, launched it, and failed.
 - d. they pursued it, launched it, and succeeded.

Let's examine each of these scenarios more closely:

Scenario 1: You are the first to come up with that idea. This scenario is extremely unlikely. I know that because I am constantly trying to come up with new and unique, but somewhat plausible product ideas to use as examples in my prototyping classes and workshops, and it's a nearly impossible task. Even when I push the limits of plausibility,

tastefulness, or ethics (e.g., beer for dogs, squirrel burgers, cat cloning), I find that someone has beaten me to it. Furthermore, even with all the information now available on the internet, there's no way of knowing for sure that nobody else is thinking or secretly working on a similar idea somewhere in the world. In the extremely unlikely event that you are indeed the first person in the world to think of a totally new idea, you don't need to worry about OPD anyway—because there is none. You are in virgin territory, and you will have to collect every bit of data yourself.

Scenario 2a: Others have had a similar idea and chose not to pursue it. This scenario provides us with zero applicable data. The fact that nobody else decided to develop an idea similar to yours does not mean that that idea cannot succeed in the market—if competently executed. Coming up with new ideas is easy; pursuing them requires effort, sacrifices, skin in the game, and more. Most people have lots of ideas but don't do anything about them; that may tell you something about those people, but it tells you nothing about the idea's potential for success.

Scenario 2b: Others are actively pursuing a similar idea, but they haven't launched it yet. This scenario also fails to provide us with any meaningful market data because, unless you spy on them, you can't know how similar their idea is to yours, what market tests and experiments (if any) they ran to inform their decision, what their risk tolerance is, and so on.

Scenario 2c: Others pursued a similar idea, launched it, and failed. This scenario provides us with *some* data, but not enough to make a decision based on it. They may have botched one or more aspects of their execution, or their product may have differed from your idea in a small but meaningful way. Not to mention the fact that if that idea was tried at some other time, in another location, or on a different population, their results may not apply to your idea and target market. The history of business is full of ideas that failed at some time or in some place, but flourished at other times and in other places. Mc-

Donald's McSpaghetti, for example, flopped in most countries, but is, believe it or not, popular in the Philippines.

Scenario 2d: Others pursued a similar idea, launched it, and succeeded. This case provides us with potentially relevant market data, but not enough to make a decision based on it. Just because someone succeeds with an idea does not mean that *you* will succeed with a similar idea. The fact that, in 1983, Stephen King's book about a homicidal car, *Christine*, sold well and was even made into a movie does not mean that *Madeline*, Alberto Savoia's idea for book about a murderous motorcycle, will succeed.

The bottom line is this. You should not make a decision about your idea based solely on what other people did or did not do with an idea similar to yours. Their experience, results, and data are not necessarily applicable to your idea.

Am I telling you to ignore any and all data from other people who have pursued ideas and markets similar to yours? Not exactly. I am not telling you to completely ignore it, because there may be something, perhaps even a lot, that you can learn from OPD. But I am telling you not to depend on it, because OPD is not sufficient. When it comes to determining the market potential of a new idea, OPD is simply not enough, and it's no substitute for *your own data*.

You Must Get Your Own DAta

Your Own DAta (YODA) is market data collected firsthand by *your own* team to validate *your own* idea. To qualify as YODA, the data must satisfy the criteria of freshness, relevance, trustworthiness, and significance. YODA is the opposite of OPD—and heaps more valuable. OPD may seem easier to collect, especially with all the data available online these days. But don't be lured by the ease with which you can obtain it, because an ounce of YODA is worth a ton of OPD. And the best news is that collecting YODA is neither difficult nor time consuming nor expensive. In fact, getting fresh YODA is often easier, faster, and more fun than digging out

and dusting off some stale OPD—especially if you use the tools and tactics you will learn in **Parts II** and **III**.

Quick Recap

In **Part I**, we spent a lot of time learning about failure. This may not have been the most encouraging way to start, but understanding how and why most new products fail in the market is a key prerequisite for appreciating and understanding the tools, techniques, and tactics you will learn in **Parts II** and **III**. Before we proceed, however, let me take a moment to summarize what we've learned so far.

Here are the main takeaways from **Part I**, worth repeating, memorizing, and perhaps tattooing somewhere on your body as a reminder:

- The Law of Market Failure: Most new ideas will fail in the market—even if competently executed.
- Most new ideas fail in the market because they are **The Wrong It**—ideas that the market is not interested in regardless of how well they are executed.
- Your best chance for succeeding in the market is to combine an idea that is **The Right It** with competent execution.
- You cannot depend on your intuition, other people's opinions, or other people's data to determine if a new idea is **The Right It**.
- The most reliable way to determine if a new idea is likely to be **The Right It** is to collect **Your Own DAta (YODA)**.

In **Part II** of this book, I will introduce you to three categories of tools and techniques to help you collect, analyze, and interpret your own data: *thinking tools*, to help clarify your idea and identify the data you need to collect; *prototyping tools*, to help you test your idea in the market so you can collect YODA efficiently; and *analysis tools*, to help you interpret the data you collect with objectivity and help you translate that data into decisions.

Part II

Sharp Tools

4 Thinking Tools

We've seen what can happen in Thoughtland when you combine fuzzy thinking with a lack of reality checks. We will address reality checks a bit later; first we need to fix the way we think about our ideas for new products.

Clarity of thought is paramount. If your new product idea is vague, imprecise, ambiguous, or open to multiple interpretations, then you don't have a solid foundation for going forward. Before you can put an idea to the test, you must be able to articulate it with enough clarity and precision to guide the design of meaningful and revealing tests, tests whose results you can trust.

In the next few sections, you will learn a set of proven concepts and tools designed to de-fuzz, clarify, and sharpen your thinking. Let's begin with the all-important and all-powerful Market Engagement Hypothesis.

Market Engagement Hypothesis

Remember the Success Equation from Part I?

$$\text{Right A} \times \text{Right B} \times \text{Right C} \times \text{Right D} \times \text{Right E, etc.} = \text{Success}$$

For an idea to succeed in the market, a number of key factors must line up just right. It's not enough to have what you think is a great concept for a new restaurant; you must also hire competent kitchen staff and servers, run an effective marketing campaign to build some buzz, get an initial set of positive reviews, and hope that Gordon Ramsay does not open a competing restaurant across the street. But, as we've seen, competent execution, experience, and even good luck can't help you if your idea is not The Right It. If your chef or staff turn out to be unreliable, you can replace them. If your first marketing campaign fizzles, you can run another campaign. But if your restaurant premise—the concept itself—is The Wrong It, what are you going to do? Change people's minds? Good luck with that!

When the market believes, rightly or wrongly, that a restaurant called Cheapo Sushi sounds like an open invitation for food poisoning and decides

to not even give it a try, you are done. If the market does not want to engage with your idea, you can't force it to. Let me put it as succinctly as I can: *If there's no market, there's no way.*

But what is your market? And how do you define and determine engagement? You need to be 100 percent clear about these questions. Enter the Market Engagement Hypothesis, which I will abbreviate as MEH (a suitable acronym since, thanks to *The Simpsons* TV show, the exclamation "meh" has entered our lexicon as a way to express a lack of interest or enthusiasm—which, more often than not, is exactly how the market responds to most new ideas).

The Market Engagement Hypothesis identifies your key belief or assumption about how the market will engage with your idea. Will they want to learn more about it, explore it, try it, adopt it, buy it? And if they adopt it, try it, or buy it, how are they going to use it and how often? Will they buy it again or recommend it to friends? In other words, the MEH articulates your vision of how the market will respond to and use your idea.

If your MEH turns out to be wrong, then it's possible that your vision is just a hallucination or wishful thinking. In that case, you better revisit it, tweak it, or move on to a different idea. But if your MEH is right, you have a fighting chance against the Law of Market Failure.

Because the MEH is so important, it should not only be clear, but it should also be testable and expressed using numbers wherever possible. But let's not get too ahead of ourselves. Let me begin by showing you what typical Market Engagement Hypotheses look like, and then I'll show you how to make them better. Here are some examples that we can use as a starting point:

Idea: Cheapo Sushi, a cheap sushi food truck, home of the 99¢ tuna roll

MEH: If we make sushi as fast and as inexpensive as other fast food, many sushi lovers will choose it over burgers, tacos, or less healthy options

Idea: Webvan, online ordering and home delivery of groceries

MEH: Given the option, lots of households will regularly choose to buy groceries online instead of going to the supermarket

Idea: A movie based on the Marvel cartoon character Howard the Duck

MEH: People love fictional ducks (Donald Duck, Daffy Duck), so they will flock (sorry!) to see a live-action movie featuring Howard the Duck

Idea: Netflix (the company's original DVD-based business model—before streaming)

MEH: If we combine mail-based delivery of DVDs with a flat monthly rate and no late-return fees, a lot of people will sign up with us instead of renting from video stores

You get the idea. The Market Engagement Hypothesis is a short sentence that encapsulates the basic premise of your idea along with how you expect the market to engage with it.

I have to confess that while I was developing the MEH concept, I was terribly tempted to use the word *hope* or *hallucination* instead of *hypothesis* (as in Market Engagement Hope or Market Engagement Hallucination). Most of the time either one of those terms would be a more accurate description of how people ruminating on their idea in Thoughtland imagine the market will engage with their idea—a blend of hopeful thinking and hallucination:

Everyone at the office loves my low-fat vegan kale cookies. They all tell me I should go into business to sell them. Oh, and a neighbor of mine who works at Whole Foods said that this is exactly the kind of baked goods their customers are asking for.

She said I could easily sell them for \$3 each! So I am going to quit my job, take a second mortgage on our house, invest in commercial-grade baking equipment, hire a few people to help me, and three months from now I'll be rolling in kale-cookie dough!

Add tangerine trees, marmalade skies, a girl with kaleidoscope eyes, and, well, let's just say that when it comes to ideas for new products or businesses, most of us don't need LSD to have hallucinations. Over the years, I've had several such hallucinations myself.

The problem is that sometimes those hallucinations become reality. Sometimes your imagined market materializes just as you expected. And once in a while the market turns out to be much bigger and much hungrier for your new product than you or anyone else could have imagined. In other words, sometimes your Market Engagement Hypothesis turns out to be correct—your idea is *The Right It*. Of course, you will still have to execute competently, and many obstacles still stand between you and market success, but over the years I've learned to count on the following fact: *If there's a market, there's a way*.

If there's real demand for inexpensive sushi, one way or another Cheapo Sushi will find a supply of low-cost fish, so it can offer a tuna roll for 99¢. People can be very creative and will usually find a solution once they have hard evidence (not just belief or hope) of market demand for their product. Formulating the precise definition and finding proof of existence of that market demand are exactly what the Market Engagement Hypothesis is designed to help us do. It's an important tool and a nonnegotiable first step in beating the Law of Market Failure.

Now that you have a general idea of what a Market Engagement Hypothesis looks like, let's learn how to turn it into one of those sharp tools I promised you. It all begins with numbers.

Say It with Numbers

Are you familiar with the saying “Not everything that counts can be counted, and not everything that can be counted counts”? It's a good

quote—words to live by. But this is also true: “Some things that count *can* be counted—and *should* be counted.”

One of the most valuable habits I acquired by working at Google is to avoid vague terms and to use numbers whenever possible. If “data beats opinions,” then the best way to express that data is to *say it with numbers*. For example, instead of blurting out, “I believe that if we make the ‘Subscribe’ buttons a little wider, we’ll get a few more clicks on them,” a well-trained Google employee would transform “a little wider” and “a few more clicks” into specific quantities, turning that fuzzy opinion into a testable hypothesis:

Fuzzy opinion: I believe that if we make our “Subscribe” buttons a little wider, we’ll get a few more clicks on them.

Testable hypothesis: If we make our “Subscribe” buttons 20% wider, we will get at least 10% more subscribers.

By saying it with numbers, a fuzzy belief becomes a clearly stated hypothesis that can be tested. In this case, an obvious experiment would be to split users into two groups: group A (original button size) and group B (a 20% wider button), and to compare the clicks between the two groups.

Test results: Using a sample of 1,000 page views, we conducted an A/B test. The results indicate that when we increased the width of the “Subscribe” button by 20% (from 100 to 120 pixels), we got 14% more subscribers.

If these initial results hold up after a few more tests, the team will have compelling evidence—YODA, not opinions or speculations—that a bigger button will result in more subscriptions.

Fuzzy thinking, along with opinions, is like catnip for the Beast of Failure—an open and irresistible invitation for trouble. Nothing removes fuzziness from your thinking like numbers; and the best part is that those numbers can be just rough estimates at first. In fact, it’s a mistake to use more precision than the situation allows. That’s why in the example above we used round numbers (e.g., 20%, 10%) in the initial hypothesis.

At that point we were just making an educated guess, and it would have been premature and presumptuous (not to mention ridiculous) to use more precise numbers—that’s what experiments are for. For example, after several more rounds of testing we might learn that the optimal width for “Subscribe” buttons is 124 pixels (24% wider than the current size) and that such an increase in size will generate an average of 13.8% more clicks.

Now that you know what I mean by “say it with numbers,” let’s apply this approach to our Market Engagement Hypothesis.

XYZ Hypothesis

The Market Engagement Hypothesis is a critical first step and an essential tool. But, like a pair of scissors or a knife, if our tool is not sharp enough, it won’t be very useful. The way we sharpen a Market Engagement Hypothesis is by rewriting it using another tool: the XYZ Hypothesis. I developed this tool out of frustration during one of my office-hours sessions at Stanford. Here’s what happened.

I was trying to get a small team of engineering students to apply “say it with numbers” to the Market Engagement Hypothesis for their personal air-pollution monitor idea. The students kept coming up with fuzzy descriptions of what they thought their market was and how potential customers would engage with their product. Here’s an example of what the students were coming up with:

**Some people who live in very polluted cities would
be interested in a reasonably priced device to
help them monitor and avoid air pollution.**

How many people is “some people”? What cities qualify as “very polluted”? What does “would be interested” imply? What does “reasonably priced” mean?

We were meeting on campus in a whiteboard-lined room, where a previous group of students had left a bunch of mathematical equations on the board. I looked at those equations and had an idea. I jumped off my chair, grabbed a marker, went to the whiteboard, and wrote out the following:

At least X% of Y will Z.

Then I explained: “X% is a specific percentage of your target market. Y is a clear description of your target market. Z is how you expect the market will engage with your idea. As you may recall from your high-school algebra, X, Y, and Z are the letters we use to represent unknown variables. And at this point that’s exactly where your idea stands—you are dealing with many unknown variables. But you can begin by making educated guesses about those unknown variables, running some simple experiments to test your initial hypothesis, and making adjustments as necessary.”

Finally, the students smiled and nodded—I was speaking their language. After a few iterations the fuzziness was eliminated, and they had a respectable, testable, say-it-with-numbers hypothesis:

At least 10% of people who live in cities with an AQI level greater than 100 will buy a \$120 portable pollution sensor (where AQI stands for Air Quality Index, an objective measure of air pollution).

Note that the initial values for X, Y, and Z were just starting points—best guesses based on the minimum market size the students believed they needed for their idea to be viable. Is 10% a good estimate of the market? Is greater than 100 the right AQI? Is \$120 the right price? Probably not. These initial numbers may prove to be way off, but at least the students defined what “some people,” “very polluted,” “would be interested,” and “reasonably priced” meant to them, and they could test to see if the market agreed.

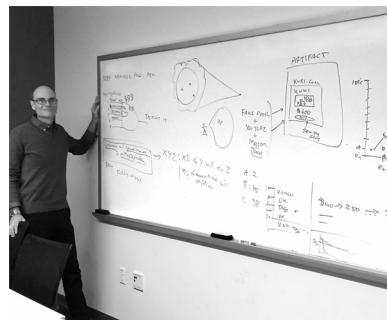
In addition to having the virtue of being testable, the XYZ Hypothesis is a great tool for getting teams to make their implicit assumptions explicit. One student’s version of reasonably priced was \$200, while another one thought that they could not possibly reach 10% of the market at that price and that the device would have to cost \$80 to \$100. The two students didn’t know that they had different ideas about pricing, but when forced to put a number to “reasonably priced,” the disagreement was unmasked. Which student’s price is right? We don’t know—perhaps neither.

It's quite possible that no significant market exists at either price point or at any price point. It's quite possible that people are simply not interested—for whatever set of reasons—in a portable pollution detection device. Ultimately, the market will decide what “reasonably priced” means; but for the time being the students met halfway and compromised on an initial price of \$120.

The XYZ Hypothesis format proved to be a wonderful “de-fuzzifier.” It replaced broad and imprecise terms (“some,” “very,” “reasonable”) with precise counterparts and the vague notion of “interested” with the specific action “buy” at the specific price of “\$120.”

Before XYZ Hypothesis	After XYZ Hypothesis
Some people	At least 10% of people
Very polluted cities	Cities with AQI index > 100
Interested	Buy
Reasonably priced	\$120

After that first success, I suspected that the XYZ Hypothesis format would prove to be a valuable tool, so I asked one of the students to take a photo of the whiteboard to commemorate that *Eureka!* moment.



And I am really glad to have that memento now, because my suspicion proved correct. The XYZ Hypothesis has become a permanent and valuable part of my toolkit and one of the first things I teach. In fact, if I only have a few minutes to help aspiring entrepreneurs or product man-

agers, I use that time to explain XYZ Hypothesis and help them express their idea using it—and it always succeeds in clarifying their thinking and bringing to the surface any misunderstandings or disagreements among team members.

Venture into the Unknown

As I've already briefly mentioned, X, Y, and Z are the letters conventionally used in science and mathematics to represent unknown variables. The letter X, in particular, is often used (even in popular culture) to represent mysteries, elements that we don't yet fully understand or whose existence we cannot prove: *The X Factor*, *The X Files*, *Planet X*.

That which we don't know or fully understand represents both a danger and an opportunity. This makes these three letters very appropriate for our task, because bringing a new product to market is akin to a journey into the unknown—a journey that could bring us great rewards or result in failure. It's like entering a colossal dark cave that is both full of treasures and full of traps—home to the Beast of Failure and the trolls of Thoughtland.

X, Y, and Z are also used to describe, measure, and graph things in three-dimensional space. In our case, the unknown three-dimensional XYZ space we are exploring and want to map and understand consists of:

X: How *big* a slice, what percent, of our target market can we capture?

Y: What *is* our target market?

Z: *How* and exactly *to what extent* will the target market engage with our product?

No sane explorer would venture into the unknown without the basic tools (compass, sextant, charting kit, etc.) needed to track his or her position and measure progress. The XYZ Hypothesis is the first tool in our market exploration toolkit. And it's an essential one, because it gives us an objective way to measure and map our steps into that dark unknown that is the market.

XYZ Hypothesis Examples

Market Engagement Hypothesis and the XYZ Hypothesis are so important that I want to make sure you understand them well. So here are a few more examples of how to use the XYZ Hypothesis format to clarify a fuzzy Market Engagement Hypothesis. All examples are based on ideas that have come up during my lectures, coaching sessions, or brainstorming exercises. They are meant for illustrative purposes only. Don't focus on the actual ideas or how silly they might appear to you, but rather on how they are expressed in XYZ format. And, yes, I know that some of these ideas have actually been tried.

Idea: An Uber-like service that picks up and delivers laundry

Fuzzy MEH: Most people who use coin laundries dread the experience. Many of them would pay a few extra dollars to have their laundry picked up, washed, dried, and returned in a reasonable amount of time.

XYZ Hypothesis: At least 10% of coin laundry users will pay \$5 extra to have their laundry picked up and returned within twenty-four hours.

Idea: An ice cube-based air cooler for cars that don't have air conditioning

Fuzzy MEH: Drivers short on cash with broken or no air conditioning in their cars would buy a cheap ice cube-powered gizmo to help them cool their car.

XYZ Hypothesis: At least 5% of people without air conditioning will buy a \$20 air-cooling gizmo when the average temperature gets over 100 degrees.

Idea: Beer for dogs

Fuzzy MEH: Many dog owners don't like to drink alone; quite a few of them would buy a dog-safe beer, so their best friend can drink with them.

XYZ Hypothesis: At least 15% of dog owners will add a six-pack of beer for dogs for \$4 when they buy dog food.

Idea: *Super Squirrel* Collectors' Edition book

Fuzzy MEH: Fans of the *Super Squirrel* comic book series will go nuts (apologies) for a limited-edition, high-quality collection featuring their favorite rodent superhero.

XYZ Hypothesis: At least 50% of the 220,000 subscribers to the *Super Squirrel* comic will buy the \$100 Collectors' Edition.

These examples may be silly (I find that makes them more memorable), but the XYZ Hypothesis is a serious and powerful tool; and it becomes even more powerful when we combine it with our next thinking tool, hypozooming.

Hypozooming

The goal of *hypozooming* is to take a specific but broad hypothesis and zoom in until you have a version of that hypothesis that is *actionable and testable right now*. It's a way to go from XYZ to xyz, a smaller, simpler, more immediately verifiable version of your Market Engagement Hypothesis. The idea is that if XYZ is true, then xyz must also be true—but xyz is much easier to test and verify.

Let me explain using the XYZ Hypothesis from the pollution sensor example:

At least 10% of people who live in cities with an AQI level greater than 100 will buy a \$120 portable pollution sensor.

This is a well-crafted, say-it-with-numbers Market Engagement Hypothesis, but it describes a huge potential market: millions of people living in all the polluted cities in the world. As it currently stands, this hypothesis is too big a starting point. It's not *testable*—at least not anytime soon, and definitely not by a handful of undergraduate students with midterms looming and no real budget. Time to hypozoom.

Target Market Zooming: $Y \rightarrow y$

The uppercase Y from your Market Engagement Hypothesis represents your ultimate target market—all the people in the world who you think will want to buy your product once it is available. To hypozoom, take that huge market and zoom in until you have a small, local, but representative subset of it. I want you to go from Y, the entire market, to y, a small, manageable, easy-to-reach initial test market. Imagine one of those videos where the camera zooms from a view of our planet in space down to a continent, then to a country, and then all the way to a city or even a specific building. That's exactly what I want you to do in your mind's eye with your hypothesis.

Be aggressive with hypozooming, but make sure that you don't zoom in so close that you are left with a sample size too small to be statistically significant (e.g., your two roommates, your mom, and that weird guy across the street). So what's a good sample size? Something in the neighborhood of 100 to 1,000 people would get a nod of approval from most statisticians. But you have to make sure that the population you sample is representative of your target market (the type of people who might actually buy your product or service).

For example, 100 randomly chosen people from the 328 million US population is a good sample size to test a hypothesis related to a new type of pizza, since most people like and can afford pizza. But if you are planning to sell a \$120,000 all-electric two-seater car (like Tesla's original Roadster), you cannot use 100 random people to get your data, because most people cannot afford or would not choose such a car. In this case, the testing should focus on the target market of, say, young, "geeky" millionaires.

Let's return to our pollution-monitor example and apply hypozooming to it. Many cities have poor air quality. But the students in the team who came up with the idea for the portable pollution sensor are from Beijing, China, one of the cities with the most dangerous levels of air pollution in the world. So the first level of zooming for them is obvious: zoom from the entire world to Beijing.

**Y: all the cities in the world with an AQI greater than 100
y: Beijing, China**

That's a good first step, but Beijing is a huge city with a population of over 10 million people. They need to zoom in further. After a few minutes of discussion, the team determines that parents of young children might be an ideal target market. One of the team members mentions that his sister's children attend Beijing Tot Academy, a private bilingual preschool in Beijing with about 300 students. He thinks that his sister would be willing to present the product at the next parents' meeting to see how many of the parents would be interested. Fantastic. They zoomed in from the entire planet to a specific preschool with 300 children:

**Y: all the cities in the world with an AQI greater than 100
y: Beijing Tot Academy parents**

Since they've zoomed into China, they also change the currency from US dollars to Chinese yuan and now have the following xyz hypothesis:

At least 10% of Beijing Tot Academy parents will buy an 800-yuan portable pollution sensor.

This is great progress. They now have a say-it-with-numbers hypothesis based on a subset of the target market that they can easily reach. But they still have a big problem: they don't have the product yet! At this point their portable pollution sensor is just an unrealized idea stuck in Thoughtland. Designing, developing, and manufacturing such a product will take at least one year and a lot of money. Even a one-off prototype will take a nontrivial investment and months to develop. They are stuck in a sort of chicken-and-egg problem. They need to validate their Market

Engagement Hypothesis to make sure their product idea is The Right It before investing a lot of time and money to build it. But to validate their MEH they need to have their product already built. Or do they?

The only way to know—with 100 percent certainty—if our idea for a new product will be successful in the market is to develop it, manufacture it in quantity, run a proper marketing campaign, and see what happens. In other words, we go forward on the hope, “If we build it, they will come” (another inspiring line from Hollywood movies that, like “Failure is not an option,” sounds great but usually works against you).

This approach, however, is a very expensive and risky way to find out if an idea is The Right It—especially since we know that most ideas will fail in the market. But what alternatives do we have? After all, we’ve learned that we can’t depend on Thoughtland-based, opinion-driven market research—too many false positives and false negatives. We know that we need data and not opinions. But we’ve also learned that we cannot depend on OPD; we need to collect YODA. That makes sense, but how can we collect YODA until we have built a product to collect it with?

That’s where prototyping comes in.

5 Pretotyping Tools

Pretotyping is a word I made up. Why would I want to do that? The best way to explain why we need a new word and why it's relevant to our quest for The Right It is by sharing with you the example that led me to it.

The IBM Speech-to-Text Example

I first heard this story at a software conference a few years ago. I am not sure how accurate my description of the events is and I may get some details wrong, but in this case the gist of the story is much more important than the details. With that caveat, here's the story as I remember hearing it.

A few decades ago, well before the age of the internet and before the dawn of ubiquitous personal computing, IBM was best known for its mainframe computers and typewriters. In those days, typing was something that only a few people were good at—mostly secretaries, writers, and some computer programmers. Most people typed with one finger, slowly and inefficiently, so companies depended on professional typists, who were relatively expensive and required things like bathroom breaks and once in a while free bagels and coffee to help keep up the morale.

IBM was ideally positioned to leverage its leading position in the computer technology and typewriter market to develop a speech-to-text computer. This technology would allow people to speak into a microphone and see their words and commands magically appear on the screen with no need for typing. By reducing the need for professional typists and eventually replacing them, the technology had the potential for making a lot of money for IBM—if the company could make it work and if the intended users were comfortable using it.

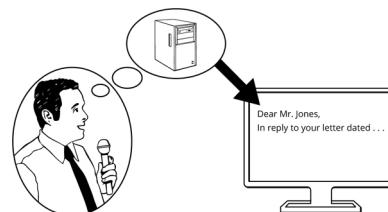
In Thoughtland everyone, with the possible exception of professional typists, loved the idea. Many people wanted to use computers, but none of them wanted to learn to type. Besides, along with flying cars, computers that understood human speech were what most people expected to see in the future. But before making what would have been a decades-long and

very expensive R&D commitment, the company wanted to ensure that its target market, businesspeople, would respond positively to this technology, not just in Thoughtland but in the real world, after experiencing first-hand how it would work and what it could do for them. The best way to do that was to expose them to a prototype version of the technology. But one major problem stood in the way.

In those days, computers were much less powerful and considerably more expensive than they are today, and a speech-to-text function requires a lot of computing power—more than what computers could provide at that time. Furthermore, even if adequate processing power were available, accurate speech-to-text translation is a very difficult computer science problem, which we are only now beginning to tackle successfully. In other words, IBM was decades away from being able to put together a proper prototype, but it needed something in order to validate a key hypothesis about its target market. IBM researchers came up with a brilliant solution.

They set up a mock workstation with a computer box, a monitor, and a microphone—but no keyboard. They told several potential customers that they had a prototype of a revolutionary speech-to-text computer. They then gave the would-be clients some basic usage instructions and invited them to try out the new invention. Skeptical but excited, people took the microphone and spoke into it: “Dictate new letter. Dear Mr. Jones, In reply to your letter dated . . .” After just a couple of seconds’ delay, the text of their letter appeared on the monitor.

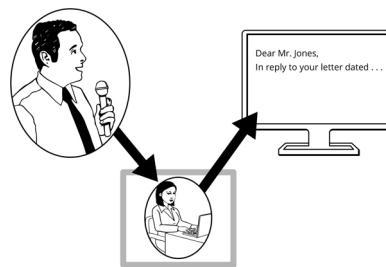
All users were impressed. This was too good to be true, which—as it turns out—it was.



What users thought was going on.

What was actually happening, and what makes this such a clever experiment, is that no speech-to-text machine existed, not even a pro-

totype. The computer box in the room was a dummy. In the room next door, a skilled typist was listening to the user's voice from the microphone and typing the spoken words and commands into a computer using a keyboard—the old-fashioned way. Whatever the typist entered on the keyboard showed up on the user screen, leading the user to believe that what appeared was the output of an actual speech-to-text machine.



What was actually going on.

IBM learned quite a bit from this experiment. After being initially impressed by the “technology,” most of the people who (in Thoughtland) were convinced that they would buy and use a speech-to-text computer changed their minds after using the system for a few hours. Even with the fast, near-perfect translation simulated by the human typist, using speech to enter more than a few lines of text into a computer proved too clumsy and problematic in several ways. People’s throats would get sore after a couple of hours, so much talking created a noisy work environment, and it was not suitable for confidential material. Imagine dictating a letter saying, “We need to fire Bob from accounting,” as Bob is passing by.

IBM’s approach was ingenious, but what would you call it? The speech-to-text setup with the typist was not a *proper prototype*—not unless one were planning to create a breed of miniature typists, stuff them into computer boxes, and feed them cheese and crackers through a floppy-drive slot. IBM did not have a prototype speech-to-text system; it only *pretended* to have such a prototype. And it needed to pretend, because if the test users knew of, or even suspected, the presence of a person instead of a computer at the receiving end of the microphone, they would have acted very differently, and the results would have reflected that.

The first time I heard this story I was left—er—speechless. My first thought was: “Why didn’t anyone tell me about this before!?” Like most people, I had spent years working on projects and products that turned out to be The Wrong It. We had built some prototypes first, of course, but the primary goal of those prototypes was to see if and how we could build the product; we were working on the assumption “If we build it, they will come.” We often spent months and millions just on those prototypes. And once you’ve invested that much time and money to develop something, it becomes really painful to call it quits—even when the actual market response is negative. So you tend to keep going, adding new features and making tweaks, hoping that somehow the situation will turn around. It’s an expensive and dangerous spiral.

Pretotyping

As an engineer, when I think of a prototype for a new technology I imagine a clumsy, hacked-together, wires-sticking-out, not-ready-for-prime-time version of that technology. What IBM did, however, struck me as something different enough from our common notion of a prototype to deserve its own word. The first word I came up with was *pretendotyping*. I thought it was an appropriate term because the IBM team, years away from being able to put together a proper prototype, *pretended* to have one.

But, although evocative, *pretendotyping* proved awkward to say or write, so I simplified it to *pretotyping*. The word worked well for me because the prefix *pre-* suggests something that comes before something else. In this case, pretotyping comes before prototyping, and the noun *pretotype* describes an artifact that precedes a prototype. So *pretotyping* combines the critical elements of both “comes before” and “pretending.”

Over the years, I’ve learned (primarily through hostile tweets and snarky comments on various social-media platforms) that a few people really dislike the word *pretotype*. They think that the word *prototype* already covers all the bases, so there’s no need to craft a new word. I agree with their premise, but disagree with their conclusion. *Prototype* does cover all the bases—but that’s exactly the problem! The term is too generic; it can mean anything. As it’s currently used, a prototype can refer

to anything from a 5¢ paper-clips-and-rubber-band contraption to a \$5 million one-off working version of an idea. I've seen the term *prototype* used to describe five-minute experiments as well as five-year projects involving hundreds of people.

Furthermore, prototypes and pretotypes serve different functions. Prototypes are primarily designed to test if an idea for a product or service can be built, how it should be built, how (and if) it will work, what's the best size or shape for it, and so on. Pretotypes, on the other hand, are designed primarily to validate, quickly and cheaply, if an idea is worth pursuing and building in the first place—a different objective that is best accomplished with a different set of techniques and best served by having its own vocabulary. In fact, in the rest of the book, I will not only use the word *pretotype*, but assign unique names to the different types of pretotypes. Is all this new nomenclature necessary? I believe so. Let's see if I can get you to agree with me.

Is it necessary to have names for different types of insects? A bug is a bug after all.

In our pantry we have boxes of spaghetti, spaghettini, linguine, fettuccine, bucatini, ditalini, and tortellini. What's the difference? Pasta is pasta. Someone should tell those pasta companies to stop confusing us with all those varieties.

What about karate, judo, jujitsu, kung fu, aikido, and tae kwon do. How many exotic ways can there be to kick someone's butt?

And do we really need all those names for the countless ways we can fall ill? Do you really need to differentiate a cold from the flu? One type of infection from another? A stomachache from appendicitis? Food poisoning from radiation poisoning? One prescription pill from another? After all, sick is sick, and medicine is medicine. I will stop here, but you can see where I am going.

In many fields, including ours, the right words help us to work and communicate more efficiently and with greater precision. More important, the right terminology sets the proper expectations and influences how we approach a situation. I would prepare for and approach crossing a river very differently than I would a creek—even though, technically, a river and a creek are both flowing bodies of water. Similarly, a six-month schedule

and \$300,000 budget would not be out of the norm for a prototype, but it would be totally out of line for a pretotype because, as we shall see, the word *pretotype* implies schedules that are measured by hours (or days at the most) and budgets that rarely go above a few hundred dollars.

If you are still unconvinced about the need for new terminology, I hope you will not let that stop you from reading on and applying the tools and tactics I am about to introduce. I'd hate to lose you over a minor lexical matter. Feel free to replace *pretotype* with *prototype*, if you must. But please keep an open mind, because I am confident that, if you give it a chance, you will see that benefits of our more precise terminology are well worth the minor cost.

* * *

In 2009, while I was at Google, I started explaining the terms *prototyping* and *prototype* to my colleagues in engineering and product management. Much to my surprise, almost all of them found the approach not only interesting, but applicable to most projects and potentially very useful in preventing investment in The Wrong It. In fact, many of them, after hearing the IBM speech-to-text example, said something along the lines of: "I wish we had done something similar with our last failed project. It would have saved us a ton of time, money, and embarrassment."

That's when I decided to see if I could uncover other examples of prototyping.

In Search of Pretotypes

It's a well-known phenomenon that when your mind is focused on something, you begin to see instances of that thing all around you. For example, if you are thinking of buying a Volkswagen convertible, you start seeing VW convertibles all over the road. Something similar happened to me with prototyping. After hearing the IBM speech-to-text story and coming up with the term *prototyping*, I started to notice and collect anecdotes and examples of techniques that, like the IBM example, could qualify as prototyping.

I also started researching and collecting instances of ideas for new products that had failed in the market despite all kinds of optimistic predictions. I eliminated from that set all the failures that could be attributed to poor execution of the idea, so what I had left was a list of *well-executed failures*. These were ideas that FLOPPed in the market not due to incompetent execution in Launch or Operations, but because the very Premise of the idea was off—the idea was The Wrong It. This is not only the most common failure scenario, but also the most costly and painful one: we work hard to build It right, only to discover that we have built The Wrong It. Ouch.

This list of painful failures included my own, those of my friends and colleagues, and many that were reported in business articles and the news. (One of the good things about researching market failures is that there is never a shortage of examples.) After a few weeks of being on the lookout for examples of pretotypes and market failures, I had collected a short list of pretotyping techniques and a long list of The Wrong It failures. And that's when things got really interesting.

I took each of those failures and asked: Could this market failure have been prevented by one or more pretotyping techniques? To put it another way, could we have learned, before getting in too deep, that the premise for this product was wrong (the product was The Wrong It), using some creative pretotyping techniques?

In almost every case the answer was a clear and resounding yes! Most of those painful and costly failures could have been easily prevented with well-planned and -executed pretotyping experiments. No approach or set of tools can give you a 100 percent guarantee, but if properly used, pretotyping tools will help you determine if an idea is The Right It or The Wrong It faster and more reliably than any market-research approach based in Thoughtland.

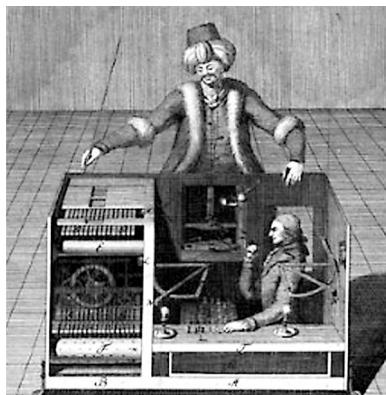
If you think that this sounds too good to be true, I don't blame you. That was my initial reaction. I am a skeptic by nature; but after several years of using, coaching, and teaching these tools and techniques, I am convinced that they work. But don't take my word for it. After all, I am biased, and my experience is at best OPD and at worst anecdotal—two types of data I've warned you not to depend upon. So I say, "Don't trust me.

Test me!" The best way to convince yourself of the logic and power of pretotyping is to experience it firsthand. Go get Your Own DAta.

In the following pages I will introduce you to pretotyping techniques that, alone or in combination, can be applied to collect valuable YODA to help you validate any new product idea. If you've ever worked on a new product that failed in the market because it was not The Right It, you will probably discover one or more techniques that might have prevented that failure.

The Mechanical Turk Prototype

The Mechanical Turk prototype borrows its name from the famous Mechanical Turk chess-playing "machine" that toured the world in the late eighteenth century. People were led to believe that the "Turk" was a mechanical contraption (an automaton) programmed to play chess. In reality, however, the box concealed a small expert chess player making the moves by manipulating the mannequin.



A Mechanical Turk prototype is ideal for situations where you can replace costly, complex, or yet-to-be-developed technology with a concealed human being performing the functions of that supposedly advanced technology.

Sound familiar? It should. The IBM speech-to-text experiment with which I started this chapter is a great example of a Mechanical Turk pre-

totype in action. Developing a good enough speech-to-text engine would have taken years and a huge investment. But a human typist, hidden in another room the same way the chess player was hidden inside the Mechanical Turk device, easily simulated that complex function and allowed IBM to collect the YODA it needed.

Let's look at another example where a Mechanical Turk prototype can help us validate our idea.

Example: Fold4U

Most coin laundries have machines to wash clothes and machines to dry them. But at the end of the drying cycle, we have to first sort out a jumbled pile of assorted garments and then fold and stack them by hand. We have self-driving cars, but we still have to fold clothes manually? That's unacceptable! Okay, perhaps that's a bit of an exaggeration, but wouldn't it be great if there were a clothes-folding and -stacking machine to take care of that last step?

Ivan the inventor believes that he can build such a machine, and he's convinced that, by leasing it to coin laundries for a fixed monthly price plus a per-use fee, he can turn piles of clothes into piles of cash. All he needs is \$50,000 and about six months to build a proof-of-concept prototype. Ivan does not have the money, because the invention from his previous venture—the RoboDogWalker—did not sell as well as he had anticipated. So he offers Angela, a friend who also happens to be an angel investor, a 25% equity stake in his new company, Fold4U, in exchange for the \$50,000.

Angela has full confidence in Ivan's technical prowess. She knows that if Ivan says that he can build an automatic clothes folder and stacker, he will. But she is not at all confident in Ivan's business model and financial projections for Fold4U. Ivan's plan is based on the premise that most coin laundry customers would be willing to pay an extra \$2 to \$3 to have their clothes folded and stacked.

When Angela calls Ivan's Market Engagement Hypothesis into question, Ivan gets defensive: "It's not an assumption, Angie. I've done my market research. I've interviewed 632 coin laundry users while they were folding their clothes. And 421 of them told me that they hated doing it and would gladly pay a few extra dollars if there were a machine that could do

that for them.”

“Isn’t that the same kind of survey you did for RoboDogWalker?” Angela asks.

Ivan’s face turns beet red. He responds, “Look Angie, if you are not interested in investing in Fold4U, just tell me. There’s no need to humiliate me. I know that RoboDogWalker was a failure, and the unfortunate accident with the poodle didn’t help. But this is a much better—and less risky—idea and a completely different market.”

“Ivan, I *am* interested,” replies Angela. “Quite interested, in fact. I can definitely see the potential for Fold4U. But before I invest \$50,000 to build a prototype, I need stronger evidence that all those coin laundry customers who *say* they’d pay for the service will *actually* pay for it. I want to see those people put their clothes in the machine and pay for it.”

“That’s why I need the money to build the prototype, Angie. How can we test that people would pay for the Fold4U if we don’t have one available?”

Before you read on, spend a couple of minutes thinking how you would reply to Ivan if you were in Angela’s shoes. Would you give Ivan the \$50,000? How could you use a Mechanical Turk prototype to validate Ivan’s MEH? C’mon, give it a shot. It’s simple, and I am confident you’ll get it right.

* * *

All right. I hope you tried the exercise. If you did, compare your answer to what Angela and Ivan came up with.

Angela shares with Ivan the IBM speech-to-text story. By the time she is done, Ivan’s face is still red, but for a different reason. His irritation has changed into excitement. “That was so clever of those IBM folks. I can’t believe I never heard this story before. I think we can use this *prototyping* thing to test Fold4U.”

“It’s called *prototyping*,” says Angela with a laugh, “but considering how quickly it gets you results, prototyping would also be a good name.”

After some discussion, Ivan and Angela come up with the XYZ Hypothesis:

At least 50% of coin laundry customers will pay \$2 to \$4 (depending on location) per load to have their clothes folded and stacked.

Then they hypozoom into and decide to test the following xyz hypothesis:

At least 50% of Lenny's coin laundry customers will put their clothes in a Fold4U machine and pay \$2 to have them folded.

And now the fun starts. Ivan meets with Lenny (a local coin laundry owner), explains to him the Fold4U idea, and offers him \$200 to let him run a pretotyping experiment in his shop. Lenny agrees to the deal, and since he's just as excited and interested in the idea as Ivan, he also agrees to help Ivan set up and run the experiment. He even gives Ivan an old broken clothes dryer—a perfect prop for the experiment.

Ivan modifies the old dryer by replacing the drum with a compartment that has a hidden door in the back. This way, after people put their clothes in the machine and pay, Ivan can open the hidden back door, pull out the clothes, fold them by hand, and put them back. To complete the illusion, Ivan makes a recording of mechanical noises that he has playing inside the machine while he manually folds the clothes. At the end of the “fold cycle,” he rings a little brass bell from inside the machine to alert the customer that the clothes are done.

The prototype works so well that none of the users suspect a thing. They all believe their clothes are being folded by some kind of robot. But although most of Lenny's coin laundry customers are intrigued by the new machine, few choose to use it. And most of those who do admit to doing so out of curiosity. The initial pretotyping experiment falls well short of expectations:

xyz hypothesis: At least 50% of Lenny's coin laundry customers will put their clothes in a Fold4U machine and pay \$2 to have them folded.

YODA: 12% of Lenny's coin laundry customers paid \$2 to have their clothes folded by a Fold4U.

Just to make sure, over a period of two weeks, Ivan runs a few more experiments using different prices and different coin laundries. Unfortunately, the results don't change much—even when the price is dropped

to just \$1. In Thoughtland, people said (and probably believed) that they would pay \$2 to \$4 for such a service, but when the time came to actually put some skin in the game (in the form of coins in the clothes folder), very few did.

Does this mean that there is no chance for Fold4U? Not necessarily. But Ivan's plan and business model was based on 50% or more of coin laundry users paying for the machine; if the actual number is less than 15%, he will have to revise many of his assumptions if he hopes to convince investors like Angela to back him.

Ivan may be disappointed that Fold4U is unlikely to be The Right It, but he's also relieved that, unlike RoboDogWalker, he did not waste two years of his life as well as a big chunk of money to learn that lesson.

What do you say, Ivan?

"Thank you, pretotyping!"

* * *

Did I hear some of you say that, after all this talk about failure, you would like a happy ending once in a while? Well, I might argue that preventing Ivan from another business disaster like the RoboDogWalker *is* a happy ending, but I know what you mean; you want a proper, Hollywood-like happy ending. No problem. You deserve it. Here you go.

Alternative Ending: Not only are most of Lenny's coin laundry customers intrigued by the new machine, they are lining up to use it. In fact, the new Fold4U is quite the sensation. People applaud every new batch of folded clothes that comes out. Everybody wants to see and use the new machine. But, of course, it's not a machine doing the folding, but poor Ivan. The next day, just as his arms were ready to fall off from all that folding, Ivan ends the experiment. He puts an "Out of Order" sign on the prototype and goes to meet Angela. This time, instead of Thoughtland surveys, he presents her with fresh YODA:

xyz hypothesis: At least 50% of Lenny's coin laundry customers will put their clothes in a Fold4U machine and pay \$2 to have them folded.

YODA: 78% of Lenny's coin laundry customers paid \$2 to have

their clothes folded by a Fold4U.

Angela is thrilled. To make sure the great results aren't a fluke, she and Ivan agree to hire part-time assistants to help with the folding and to run additional experiments. As the novelty wears off, market engagement drops a little (it turns out that some people were more curious to see if and how the machine worked than in using it regularly). But the average market engagement (the number of coin laundry patrons who pay to have their clothes folded by the Fold4U after drying them) stays at a healthy 62%—providing strong confirmation for Ivan's 50% or more projection in the XYZ Hypothesis.

People said that they would pay for such a service, and when the time came to actually put some skin in the game, they did. In this case, YODA matched opinions. It happens. Just not as often as we'd like, and that's why we need to test the ideas.

Angela decides to invest in Fold4U, and by using the impressive YODA from the prototype, Ivan is able to increase his company valuation and recruit additional investors. Not only that, but when the time comes to market and sell the Fold4U, Ivan can provide coin laundry owners with a compelling business case: "Our data shows that over 60% of your customers will pay an extra \$2 to \$4 per load to have their clothes folded. This will increase your total revenue and profits by at least 20%." Sold!

What do you say, Ivan?

"Thank you, pretotyping!"

As these two different endings show, investing a little time and resources to prototype an idea is a win-win tactic:

If YODA from the experiments does not validate your hypotheses, pretotyping will save you from a very likely failure.

If YODA confirms your hypotheses, you will be in a better position to recruit partners, secure investors, and convince potential customers.

Every idea deserves to be prototyped, and there's (at least) a prototype for every idea—so let's give the Mechanical Turk (and Ivan's arms) a well-deserved rest, and let's explore a few more pretotyping techniques.

The Pinocchio Prototype

The Pinocchio prototype is named after the beloved fictional character Pinocchio, the wooden puppet who dreamed of becoming a real boy. You will understand why I picked this name after I share with you the example that inspired it.

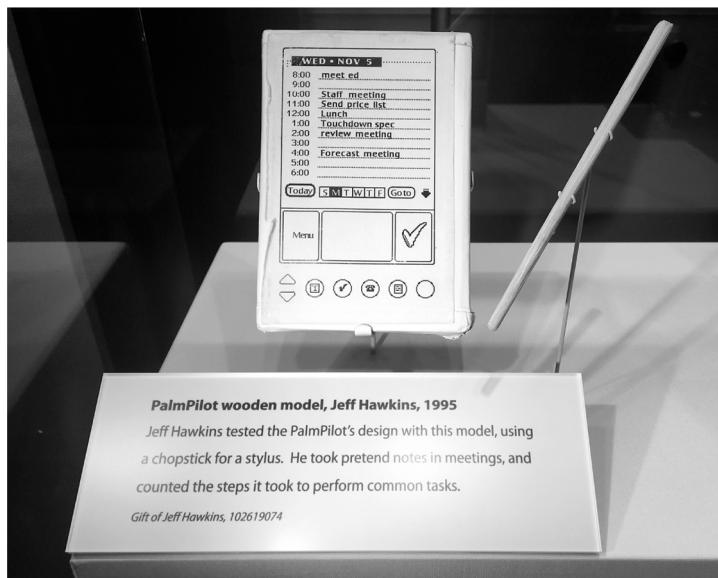
Example: The PalmPilot

In the mid-1990s, brilliant innovator and entrepreneur Jeff Hawkins had an idea for the personal digital assistant (PDA) that would eventually become the PalmPilot. But before committing to it and investing in building an expensive prototype, which would have required a full team of engineers and a lot of time and money, he wanted to validate some of his assumptions about the device. He *knew* he could build it, but would he use it? What would he use it for? And how often would he use it?

His solution was to carve a block of wood to match the intended size of the device, whittle down a chopstick to make a stylus, and use paper sleeves to simulate various user screens and functions. He carried the block of wood in his pocket for several weeks and pretended that it was a functional device in order to get insights into how he would use it. If someone asked for a meeting, for example, he'd pull out his wooden block and tap on it to simulate checking his calendar and scheduling a meeting reminder.

With the help of his prototype, Hawkins collected valuable YODA. He learned that he would actually carry such a device with him and that he would be using it mostly for four functions: address book, calendar, memo, and to-do lists. His simple experiment provided him with enough YODA to convince him that he would love to have a working version of the device. He knew, of course, that a sample size of one (himself) was not sufficient to determine if other people would respond to the Pilot the same way he did. He would have to follow up this test with additional experiments to validate the rest of the market. But the idea passed an important first test: its own inventor found it useful. This may seem a trivial threshold to pass, but you'd be surprised how many people bring to market a product without first validating that they themselves would use it.

The data collected from the simple wood and paper prototype helped



The PalmPilot wooden model on display at the Computer History Museum, Mountain View, CA.

to guide and justify the much greater investment needed to develop a proper working prototype. Not only did the PalmPilot become incredibly successful, it also paved the way for smartphones and established a form factor (i.e., shape and size) for most portable electronic devices that continues to this day. Pinocchio was a wooden puppet who dreamed of becoming a real boy. The PalmPilot prototype was a wooden PDA that Jeff Hawkins dreamed might one day become a real product. Both dreams came true.

In addition to being a great example of a powerful pretotyping technique, the PalmPilot story also illustrates some of the key concepts I've been emphasizing. Below is how *TIME* magazine reported it in March 1998. I've marked some of the key points in italic:

Hawkins, 40, Palm's chief technologist and Pilot's creator, designed one of the first handheld computers, the GRiDPad, a decade ago. It was *an engineering marvel but a market failure* because, he says, it was still too big. Determined *not to make the same mistake twice*, he had a ready

answer when his colleagues asked him how small their new device should be: “Let’s try the shirt pocket.”

Retreating to his garage, he cut a block of wood to fit in his shirt pocket. Then he carried it around for months, *pretending* it was a computer. Was he free for lunch on Wednesday? Hawkins would haul out the block and tap on it *as if* he were checking his schedule. If he needed a phone number, he would *pretend* to look it up on the wood. Occasionally he would try out different design faces with various button configurations, using paper printouts glued to the block.*

This story embodies the core motivations for and principles of pre-totyping:

- Hawkins’s painful experience of spending years and millions to produce the GRiDPad, a product that turned out to be “an engineering marvel but a market failure.”
- His realization that the mistake was not that he built It wrong, but that he had built The Wrong It.
- A commitment “not to make the same mistake twice.” In other words, he told himself something along the lines of, “Next time, make sure that you are building The Right It before you build It right.”
- The creation of a first prototype—not to test if the Pilot could be built, but to test if, how, and how much one would actually use it, by collecting firsthand YODA to inform design decisions for the actual prototype and eventual product. For example:
 - Carried the device in my pocket 95% of the time.
 - Pulled it out to use it an average of 12 times a day.

For scheduling appointments: 55% of the time
To look up phone numbers or addresses: 25% of the time
To add to or check a to-do list: 15% of the time
To take notes: 5% of the time

* David S. Jackson, “Palm-to-Palm Combat,” *TIME*, March 16, 1998.

- Using his imagination (i.e., pretending) to fill in the missing functionality using a dummy of the envisioned product as a prop.

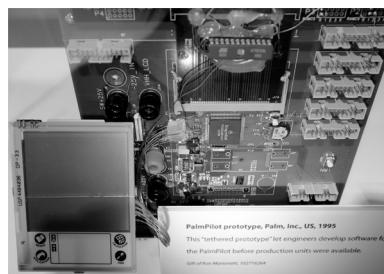


The PalmPilot prototype next to the product.

Mock-ups and nonfunctional prototypes are quite common in innovation, but the act of pretending that the mock-ups are functional and using them as such (especially for an extended period of time, as Jeff Hawkins did) is rare. Remember, *pretending* is an important part of pretotyping.

You Say *Proto*, I Say *Preto*

The PalmPilot story illustrates one of the key differences between pretotyping and prototyping that we previously discussed. The picture below is an example of what any red-blooded engineer has in mind when the word *prototype* is mentioned:



A PalmPilot working prototype on display at the Computer History Museum, Mountain View, CA.

As one of those red-blooded engineers myself, I love building prototypes. I can't wait to fire up my oscilloscope and my soldering iron. But

I've learned to wait before investing a lot of time building a working prototype.

Remember that the primary purpose of *prototypes* is to answer such questions as:

- Can we build it?
- Will it work as intended?
- How small/big/cheap/energy-efficient can we make it?

These are important questions. But experience and a ton of evidence tell us that most of the time we can build it, we can make it work as intended, and eventually we'll be able to optimize size, energy efficiency, and so on. In other words, we should be confident in our ability to build it and make it work as intended.

The primary purpose of *prototypes*, on the other hand, is to answer such questions as:

- Would I use it?
- How, how often, and when would I use it?
- Would other people buy it?
- How much would they be willing to pay for it?
- How, how often, and when would they use it?

The answers to these questions will help us answer the most critical question of all: Should we build it?

With that out of the way, here are two more examples of the Pinocchio prototype in action.

Example: Smart Horn

Your car horn is a blunt tool for communicating with other drivers. If you are like most people, you use your horn for several different purposes while on the road. A *hooonk!* can mean any of the following:

“Move your butt!” to a driver who takes more than a millisecond to start moving after the light turns green.

“Thank you,” to a driver who lets you pass on a two-lane road.

“Hi, Bob,” to your friend Bob driving by.

“You got a death wish, you moron?” to a pedestrian who runs in front of you.

And the all-purpose “You #\$\$%^&*!”

Enter the Smart Horn, a four-button horn that can accompany each honk with a programmable message—so you don’t have to stick your head out the car window to yell it. For example, you can program it with the following: “Move it,” “Thank you,” “Hi there,” “Watch it,” and “You @#\$%^&*!”

There’s no question that such a horn *can* be built. But would people use it, how would they use it, and how often? You can do what Jeff Hawkins did and begin by pretotyping it for yourself—to see if *you* would use it. The simplest way to do that is to put four stickers, each one with a different label representing a different horn sound, on your steering wheel and then drive around for a couple of weeks pretending that those stickers are working buttons for the Smart Horn. You might discover that, as tempting as it sounds, having your car yell “You @#\$%^&*!” to a group of leather-clad motorcyclists would not be such a good idea.

If you are mechanically or electronically adept, you can easily upgrade from stickers to buttons that count how many times you’ve pressed them, which will make your data collection more precise. With some persuasion, you might also convince your family and friends to pretend along with you. Ask them what messages they’d like on their Smart Horn, put stickers on their steering wheels, and ask them to keep track of how often they actually use them.

Example: Smart Speaker, or Voice-Controlled Assistant

At the time I am writing this, smart speakers like the Amazon Echo, Google Home, and Apple HomePod are a hot and competitive new tech category. I could not have predicted how successful smart speakers would be in the market, but after I heard that such devices were in the works, I predicted—with confidence—that *I* would buy and use at least three of them a couple of years before the first one was launched. I made this pre-

diction using the Pinocchio pretotype.

I took a can of pinto beans and put some black masking tape over it to make it look high-tech. I named my Pinocchio pretotype “HAL” (after the HAL 9000 computer in *2001: A Space Odyssey*), set it on my living-room coffee table, and began pretending that HAL was functional. I would say to it:

“*HAL, what will the weather be like today?*”

“*HAL, remind me to call my mom in one hour.*”

“*HAL, play some Led Zeppelin.*”

“*HAL, wake me up at 5 a.m. tomorrow.*”

Of course, the can of beans did not respond to my commands. I would have checked into a mental institution if it had. But the simple act of pretending that it could have carried out my requests provided me with valuable YODA and insights on where, how, and how often I would use such a device. I learned, for example, that I would want at least three such devices: one for the living room, one for the bedroom, and one for my study. After a few days of interacting with my pretotype, I discovered that, in addition to a volume knob, I wanted it to have a “Stop Listening” button—just in case those pinto beans were listening to my private conversations. I also determined that, ideally, the microphones should either be sensitive enough to register a soft command or have a “whisper mode,” so I would not wake up the entire household at 5 a.m. by yelling, “HAL, is it going to rain this morning?”

Within a week of pretending, I was convinced that such a device would definitely be The Right It for me and that it had a very good chance of being The Right It for millions of people, and thus a market success. When Amazon announced the Echo in 2015, I was one of the first people in line to buy one, then a second, and then a third. Not only that, but when I first saw a photo of the Echo, I just had to smile at how closely it resembled my can-of-pinto-beans pretotype.

The Fake Door Pretotype

The name for the Fake Door pretotype comes to us courtesy of Jess Lee, who at the time was CEO and cofounder of community-based shopping



The author's bean-can prototype of HAL next to Amazon's Alexa.

site Polyvore. Thanks, Jess!

The basic concept behind the Fake Door pretotype is that you can get some data on how many people would be interested in your idea by putting up a *front door* (e.g., an ad, a website, a brochure, a physical storefront) to help you pretend that the product or service exists when, in fact, you have nothing to offer quite yet. If you can't get enough people to *knock* on your product's front door (i.e., to show interest in your idea), then you go back to the drawing board and review your ideas and your hypotheses.

Early in his legendary and influential career, Kevin Kelly, bestselling author and founder of *Wired* magazine, used this approach to test the market for his first business idea, a catalog of budget travel guides. This is how Kelly describes it in Tim Ferriss's book *Tribe of Mentors*:

I started my first business with \$200. I bought an ad in the back of *Rolling Stone* magazine advertising a catalog of budget travel guides for \$1. Neither the catalog nor the book inventory existed. If I hadn't gotten enough orders, I would have returned the money [from any order], but it all worked out by bootstrapping.*

* Tim Ferriss, *Tribe of Mentors: Short Life Advice from the Best in the World* (New York: Houghton Mifflin Harcourt, 2017), p. 249.

Ads in the back of magazines seem so quaint these days, but this happened in the early 1980s when those relatively cheap ads were one of the few ways a small-time entrepreneur could reach a target audience.

Around the time Kevin Kelly was experimenting with the market for travel guides, I was busy finishing college and learning computer programming. When the IBM PC was launched in 1981, I saw a unique opportunity for putting my newly acquired programming skills to good use by writing video games, which were becoming very popular in those days. With a \$5,000 investment from my father (Thanks, Dad!)* I bought one of the first IBM PCs ever produced and launched my first business: a one-man video-games company.

I named it Heigen Corporation, because I thought that sounded big and impressive. Some of my games, especially *Ramsak*, a primitive Pac-Man-like game, were successful. But other titles, like *BitBat* or *XO-Fighter*, produced disappointing sales. Unfortunately, unlike Kevin Kelly, I did not have the foresight to invest a few hundred dollars to test the market interest for each game before investing two to three months in developing it. I did not realize it at the time, but that was my first exposure to the Law of Market Failure and the importance of making sure that you are building The Right It before you build It right. Had I known about the Fake Door pretotyping technique, I would have done things differently.

Before developing a full game with multiple levels, I would have created some static screenshots and brief descriptions for several possible games. Then I would use those screenshots to compose and publish “coming soon” ads for each of them. The ads would encourage people to mail us a letter with a self-addressed stamped envelope (email did not exist then) to receive a \$5 discount coupon and to be notified when the game was available. Let’s say, for example, that I was considering four new ideas for my next game:

* That figure, \$5,000, was a significant amount of money for our family in those days—about six months’ rent. And when you consider that at the time I had very limited programming experience and zero business experience, it was a foolish thing to do. Worrying about paying my dad back kept me up at night, and I think he worried a lot about it too. Fortunately it all worked out in the end.

Lost in Bitland: A maze adventure with puzzles.

Digi Kong: Stealing bananas from a giant monkey.

Pixel Racer: A racing game.

Tapeworm: I'll skip the description for this one.

I would create and publish similar ads for each game and, a few weeks later (that's how long it took in the pre-internet days), I would be able to compare the results:

Game	Number of Responses
<i>Lost in Bitland</i>	127
<i>Digi Kong</i>	15
<i>Pixel Racer</i>	255
<i>Tapeworm</i>	3

I was rooting for *Tapeworm*, but “data beats opinions.” I would get busy working on *Pixel Racer* first, put *Lost in Bitland* next in the queue—and scrap plans for *Digi Kong* and, alas, for *Tapeworm*. At the same time, I would publish additional ads for *Pixel Racer* in several other magazines, since I had data that those ads would generate a strong response and be worth the investment.

Two or three months later (I was a fast programmer in those days) the people who were interested in *Pixel Racer* and *Lost in Bitland* would receive a letter with the promised \$5 discount coupon, announcing that the *Pixel Racer* game was available for purchase now and that *Lost in Bitland* would be released in a few months. What about the people who were interested in the two other games? I would send them a letter explaining that, regrettfully, we decided not to publish *Digi Kong* and *Tapeworm*, and as a form of atonement I would include a free copy of *Pixel Racer*.

I can imagine many of you cringing a bit at the trickery involved with this technique. Good. It means that you have a working ethical compass. I also cringe a bit every time I present it. The Fake Door prototype is both my most favorite and least favorite technique: most favorite, because it’s so darn efficient and effective; least favorite, because it contains an element of deception. Because of that deception, one should not use this strategy

with certain product categories (e.g., medical devices or services), and use it with extreme awareness and consideration of ethical issues with *all* types of products and services.

I also recommend being generous with the people who knock on the door. Give the people who give you YODA something valuable in return for their trouble—this way you get a win-win-win situation, as I did in this example. Think about it:

1. The people who were interested in *Pixel Racer* and *Lost in Bitland* win, because they are going to get the games they wanted—and a \$5 discount on top of that.
2. The people who were interested in *Digi Kong* and *Tapeworm* are not going to get those games. But they still win, because I am sending them a free copy of one of my other games. I suspect that, for most of them, the surprise of receiving a new game (a \$29.95 value) for free will more than make up for not being able to pay to play a game called *Tapeworm*.
3. I win by not wasting my time and money creating and advertising games that not enough people are interested in.

Fun fact: I am not much into playing computer games, but I really enjoyed designing and developing them. Once I graduated from college, however, I got out of the computer-game business even though my games were selling well. Why? Because my dad (and investor) said, “Computer games are just a fad. If we stick to them, we will never be a big business. The next program you write should be some kind of business application.”

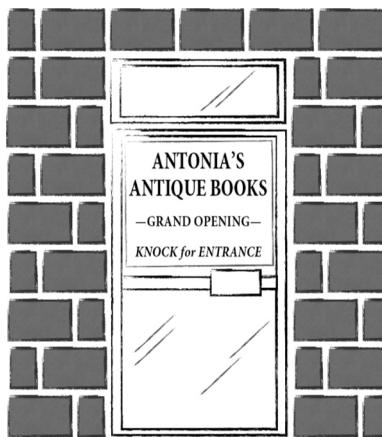
I could have developed *Super Mario*, but instead I developed *Super-mailer*, a mailing-list manager. Ever heard of it? Thought so. Contrary to my father’s opinion, today the video-game business is bigger than either the movie or the music business, with several game companies worth billions of dollars. They say “Father knows best.” Perhaps. But not when it comes to predicting market success.

Let’s walk through a couple more examples of Fake Door prototypes in action, beginning with a brick-and-mortar example.

Example: Antonia’s Antique Bookstore

Imagine that, on a bleak December day, you are wandering weak and weary on a busy downtown street, when you walk by a door with a sign that announces the opening of a new antique bookstore.

As a book lover, you can hardly contain your delight. With visions of volumes of forgotten lore—perhaps a first edition from one of your favorite authors, Edgar Allan Poe—you gently rap on the door: *knock-knock*.



No answer. You knock again. Still no answer. You knock a third time. Nothing. No telltale sign of anyone behind that door. “The owner must be napping or perhaps can’t hear my tapping,” you say to yourself and, a bit disappointed, you walk away.

Without realizing it, you’ve just taken part in a Fake Door pretotype and provided Antonia with a valuable morsel of YODA.

You see, Antonia is seriously thinking of quitting her job as a book editor and opening an antique bookstore in that neighborhood, but at this time there isn’t a single book for sale behind that door—let alone a full bookstore. In fact, there’s nothing behind that door but a vacant piece of real estate. Antonia doesn’t have a lot of money to spend on traditional market research for her bookstore, but her Market Engagement Hypothesis is that if she opens the store on the right street and advertises it with a big sign, a lot of people will discover it as they walk by, and after that word-of-mouth marketing will do the rest.

For this plan to work, she determines that she needs at least 0.5% of

people (1 in 200) who pass by each day to show enough interest to visit the store at least once. Before investing some serious capital to lease a space, buy inventory, hire staff, and so on, she wants to validate that hypothesis. So she invests \$20 to make a sign, \$2 on double-sided tape, and a few hours of her time testing the sign at various streets and locations that she believes will have the right kind of pedestrian traffic (i.e., a decent percentage of bookworms). After taping up her signage, she sits across the street with a notebook and keeps track of:

1. How many people pass by the door
2. How many of those people notice the sign
3. How many of them stop and knock
4. How many times they knock (the more they knock, the more interested they must be)
5. The age, gender, and other relevant characteristics of each knocker (e.g., middle-aged well-dressed male professional; female college student).

She runs her experiment on both weekdays and weekends to see if and how the amount and composition of the pedestrian traffic changes.

After a few days, Antonia has collected a lot of great YODA. Unfortunately, the data doesn't support her market hypothesis—not even close. At one location she counted a grand total of 3 people knocking out of 4,000 people passing by (that's less than 0.1% of the foot traffic). In another, she counted over 5,000 people and not a single knock.

Antonia is disappointed in the result, but she is also relieved that she was able to collect this data and test her market hypothesis so quickly, with very little money—and without leaving her job. Pretotyping saved her from a potentially disastrous business decision.

Does this mean that Antonia should give up on her bookstore idea? No, not at this point. But it does mean that she cannot count on just a door sign to get people into the store—she needs to revise her MEH, and she may have to adjust her plan to include some advertising budget, at least at first. She also begins to wonder if, as much as she likes the idea of a physical bookstore, perhaps her idea for selling antique books would

work better online. The Fake Door prototype proved quick and effective in the real world, so she wonders if she can use it online. Of course she can, just like her friend Sandy, who is the squirrel aficionado in our next example.

Example: A Guide to Squirrel Watching

Sandy is thinking about writing a book about her passion: squirrel watching (a rodent variation on the already popular hobby of birdwatching). Sandy knows that most books fail in the market. So before investing months of precious time away from actual squirrel watching to write her tome, she wants to gauge the level of interest in such a book. An online Fake Door prototype is a very effective way to do that.

First, Sandy buys the SquirrelWatching.com domain (\$10). Then, using a free DIY website design tool, she creates a basic website. The site's landing page consists of a mocked-up version of her book, along with a brief description of what the book is about, a short bio of the author, and a "Buy Now for \$20" button. When people click on the "Buy" button, they are redirected to another web page that displays the following message:

Fellow Squirrel Enthusiasts,

Thank you for your interest in

A Guide to Squirrel Watching.

I am hard at work on the book, but it's
not quite ready for publication.

If you want to reserve your first-edition copy,
enter your email in the form below, and I'll let
you know as soon as the book is available.

In the meantime, happy squirrel watching,
and don't forget your rabies shot!

Sandy (Squirrelgirl) Watson

Once the Fake Door website for the book is up and running, she needs a way to let squirrel enthusiasts around the world know about it. She composes a web ad:

Do you like stalking squirrels?

Go to www.SquirrelWatching.com

to preorder *A Guide to Squirrel Watching*
by Sandy Watson. Only \$20.00.

Then she invests \$60 to have her ad displayed on nature-related websites and as a sponsored link whenever people do squirrel-related online searches.

Now she's all set to collect her YODA. When people click on her ad, they are redirected to her website, where they have the option of submitting their email (a bit of skin in the game) to be alerted when the book is ready. Executing this Fake Door pretotype will cost as little as \$100, take just a few hours of work, and require minimal technical skills—but it will provide Sandy with invaluable YODA.

For example, by dividing the amount she spent on ads by the number of clicks on the “Buy” button, she can determine her customer acquisition cost (or CAC). If \$60 spent on ads results in, say, 15 “Buy Now for \$20” clicks, her CAC will be around \$4 (\$60 divided by 15)—a promising result, since \$60 in ads would have resulted in \$300 in sales. If, on the other hand, she only manages to get 1 or 2 “Buy” clicks, she might have to revisit either her marketing (the website design, the wording of the ad, etc.) or her Market Engagement Hypothesis. In either case, Sandy would get some hard, firsthand data to help her decide whether to write her book.

More on the Ethics of Fake Door Pretotypes

I know that we've already touched on the potential ethical issues associated with this prototyping technique, but I want to talk about it a bit more, because I know that many people (including myself) care about such things. Did Antonia and Sandy do something ethically wrong, or at least questionable, in order to find out if their ideas are The Right It?

Assuming that you are not interested in an elaborate philosophical discussion of Antonia and Sandy's actions, one way to analyze the ethics of Fake Door pretotypes is by considering possible scenarios in which Antonia and Sandy do *not* use this pretotype, but rely on other methods to evaluate their MEHs.

Instead of using a Fake Door pretotype to collect YODA, Antonia decides to go the market-survey route. Armed with a notepad, she plants herself at the corner of Main and Oak (the area where she's thinking of opening her bookstore) and asks passersby:

Do you think this street could use a great antique bookstore?

Would you visit such a store? How many times a year?

How many books a year do you think you might buy?

Antonia's body and notepad may be at the corner of Main and Oak, but (cue in *Twilight Zone* theme music) her data comes from another dimension, from a world inhabited by ideas and opinions, a place we call Thoughtland.

Antonia's Thoughtland-based "research" indicates a great need for the kind of store she has in mind. Most people (77%) say they'd love an antique bookstore and would shop there regularly for themselves and for gifts for other people. An elderly woman comments, "Old books make such unique and thoughtful gifts for my friends. And I have many friends, you know. I'd buy at least a few books each month from you." A college student says she spends at least \$100 on books each month and would love a local bookstore. But not everyone is so enthusiastic and optimistic; a few people warn her that, given that other local bookstores closed due to a lack of customers, she is unlikely to succeed. But Antonia subconsciously ignores those naysayers (confirmation bias) as she crunches her numbers and makes her plans.

Eventually, she comes up with sales projections of over \$14,000 per month. Empowered by this projection, she quits her job, gets a \$100,000 loan, signs a three-year lease on a space, buys a bunch of old books, and holds a grand opening. Six months later she has a not-so-grand closing. She's more than \$100,000 in debt and jobless. Pretty painful for Antonia, wouldn't you say?

Similarly, instead of enjoying time outdoors watching squirrels chase each other's tails, Sandy makes the decision to go ahead with her book based solely on the opinions of her family, her friends, and a retired park ranger who says, "Everyone I know loves squirrels and is super interested in them." She spends two years at her desk and several thousand dollars to write and self-publish her book. And now she avoids going into her garage because she can't bear to see those fifty boxes of unsold books.

In both of these scenarios, by depending on opinions instead of data Antonia and Sandy fall victim to the Law of Market Failure and Thoughtland's false positives. They commiserate over a couple of bottles of Chardonnay and wonder where they went wrong.

"Most of the people I interviewed seemed so positive and excited

about the bookstore. Where did they all go?” asks Antonia before taking a big gulp of wine. “Doesn’t anyone buy books anymore?”

“Well, they ain’t buying mine for sure,” answers Sandy, refilling her glass. “I spent so much damn time and money on that squirrel book . . . I don’t think I can bear to look at another one of those rodents again.”

Now think again about the original scenarios in which Antonia and Sandy used the Fake Door prototype, described above. Antonia was out \$22 and a few hours of her time. The few people who actually knocked on the Fake Door were initially disappointed, but they forgot all about it a minute later. No real harm done.

In Sandy’s case, the time spent and inconvenience caused to the few people who clicked on her Fake Door online ad for the squirrel book are negligible compared to the time, money, and effort she was about to put into writing and publishing a book too few people would be interested in.

I hope you would agree that, in both Antonia’s and Sandy’s cases, the second scenario causes more pain and waste than the first one. The few minutes of people’s time wasted knocking or clicking on the fake doors are negligible compared to Antonia’s and Sandy’s potential losses.

Each year, millions of people just like Antonia and Sandy launch products, services, and businesses that fail in the market. Think of the cost to society for all the failed businesses and products that nobody wants. Think of the millions of unsold products that, after a huge investment in development, production, advertising, and shipping, end up in the garbage. Unless you are in the bankruptcy or landfill business, wouldn’t you prefer to have people like Antonia and Sandy either gainfully employed or running a successful business—and not in debt or collecting unemployment?

Not only that, but people who are not interested in the Fake Door offer are not going to knock on the door or click on the ads—so they suffer no inconvenience. And by knocking or clicking on the Fake Door offer, the people who are interested in the idea and might actually want the bookstore or the squirrel book to exist are, in a sense, “voting” for it and thus increasing the probability that the idea will become a reality.

Even with these rationalizations, you can probably understand why the Fake Door is, as I mentioned, both my favorite and my least favorite technique. I love the fact that it can be executed so quickly and inexpen-

sively, making it possible to collect real-world data in a matter of hours. But I am still somewhat bothered by the minor deception involved. If that bothers you too, I offer two solutions.

The first solution is to be up front with and reward the people who knock on your fake door or click on your “Buy” button. For example, after a person knocks on the bookstore’s fake door, Antonia can walk up to them, admit that she was just running a test, apologize, and perhaps even give the person a \$10 Amazon gift certificate to buy a book. Sandy can do something similar with her Fake Door website; perhaps give all people who click the “Buy” button a free one-page guide to squirrel identification or some other inexpensive squirrel-related gift. If you decide to use a Fake Door pretotype, I encourage you to do something similar—turn it into a win-win: potential customers get a free gift, and you get your YODA without a guilt trip.

The second solution is to use a variation of the Fake Door pretotype, the Facade pretotype, which I share with you next.

The Facade Pretotype

The Facade pretotype differs from the Fake Door in one important respect—when potential customers knock on that door or click that “Buy” button, someone answers and something happens. They may even get precisely what they were looking for. Let me illustrate this technique with a great example.

Example: CarsDirect

At the dawn of the internet age, IdeaLabs CEO and world-class innovator Bill Gross envisioned an online car-selling service. Such a website is something we take for granted these days, but at the time it was a very novel idea and market success was far from certain. Before making a major investment, before even having a single car in inventory, Bill Gross validated the idea using what we will call a Facade pretotype. Here’s how he explains it:

In 1999, we started CarsDirect. Back then people worried about putting credit cards online; here I wanted to sell a car online! We put a site up on a Wednesday night; by Thursday morning, we had four orders.

We quickly shut the site down (we'd have to buy four cars at retail and deliver them to these four customers at a loss), but proved the thesis. Only then did we start building the real site and company.*

Even though CarsDirect had zero cars to sell, the website that it put up that Wednesday night is considered a Facade prototype and not a Fake Door. Had it been a Fake Door, when people clicked "Buy" next to a photo and description of a car, they would have received a message along the lines of, "Sorry, the car you wanted is no longer available."

But instead of getting an apology and an excuse in exchange for their involuntary participation in a market-research experiment, the first few people who clicked "Buy" on that CarsDirect website soon had the car they wanted sitting in their driveway. And what did Bill Gross and his team get? The best form of validation for the idea: YODA with lots of skin in the game. And the very best type of skin in the game: four checks for a few thousand dollars each.

A Facade prototype requires more investment and commitment than a Fake Door, so why would you choose it over the faster, cheaper Fake Door? Well, depending on the idea and situation, the added investment might be worth it. First, as I've already mentioned, for some categories of products and services, using a Fake Door prototype might be unethical or downright illegal, for example, if you pretend that you have a cure for some disease.

Second, you can learn a lot more about your potential business with a Facade than you can with just a Fake Door. In the CarsDirect example, Bill Gross and his team not only validated the demand for the service (i.e., people's willingness to buy cars online), but in the process of delivering actual cars to their initial set of customers, they also learned firsthand about the necessary financial and legal paperwork required and about the back-end process associated with each sale. Not to mention that checks for a few thousand dollars each from several customers are more compelling and convincing evidence for potential investors than a spreadsheet showing

* Bill Gross, "Here Are the 12 Lessons I've Learned in My 30 Years of Being an Entrepreneur," <http://www.businessinsider.com/bill-gross-lessons-2011-12#would-anyone-actually-buy-a-car-online-29>.

how many people knocked on a door or clicked on a “Buy” button.

Example: Antonia’s Antique Bookstore Revisited

We’ve already seen how Antonia pretotyped her bookstore using a Fake Door with a minimal investment of time and money. Had she been willing to invest a bit more in exchange for learning a bit more about her market and customers, a Facade pretotype would have served her well. She could pretotype her brick-and-mortar business doing something similar to what CarsDirect did online.

Instead of simply putting a sign on the door of some vacant building or store, she could make arrangements to rent the space behind that door for just a few days and put a desk in front of a couple of bookcases filled with books she already owns. When people knock and come in the store, she explains that she’s still working on her book inventory. But if the customers already have an idea of the kind of books they are interested in, she’d be happy to help them find those books. Here’s how such an interaction might go.

A potential customer opens the door expecting rows of bookshelves stuffed with thousands of books and is surprised to see just a couple of bookcases and a desk where Antonia is working on a computer.

“Oops, sorry. I thought this was a bookstore,” says the customer.

“Oh, but it is,” answers Antonia with a beaming smile. “Or it will be, once all my inventory arrives.”

After getting out from behind her desk and shaking hands with the still slightly confused potential customer, she explains: “My name is Antonia. I’m just getting started, testing the waters and the neighborhood, so to speak. But I can already help you. Are you looking for any specific books?”

“Actually, I am very interested in Stoic philosophy and wanted to see if you had any interesting or unusual books on that subject for my collection.”

“Ah, yes, the Stoics. I believe a beautiful leather-bound nineteenth-century translation of Marcus Aurelius’s *Meditations* is available. It’s not cheap though, about \$200. Would you like me to look it up and order it for you? Or are you looking for something a bit less expensive?”

“Sure, if it’s not too much trouble. I don’t mind spending that much

if the book is worth it.”

“No trouble at all. By the way, while the computer searches for it, may I ask you—book lover to book lover—about your book collection . . .”

As you can see, with a Facade pretotype, Antonia will be able to capture much more data than just how many people knock on the door. She can get an idea of the type of people who would come into the bookstore, the kind of books they’d be looking for, and the price range with which they would be comfortable.

As you can probably tell from these examples, I love books. But I also love movies and videos—and not just for learning and entertainment, but also for prototyping, as we shall see in this next section.

The YouTube Pretotype

Since their invention, movies and videos have helped us imagine and experience events, places, and devices that don’t yet exist (e.g., spaceships, time machines)—they’ve helped us *pretend*. This makes videos a natural tool for prototyping. The YouTube prototyping technique takes advantage of the “magic of movies” to bring to life product ideas that are not yet fully developed or widely available, so you can share them with your target market (using YouTube or any other video platform or device) in order to collect YODA about the market’s interest in your idea.

Example: Google Glass Explorer Edition

Google Glass is the name for an optical head-mounted display in the shape of a pair of eyeglasses. In addition to the ability to display information directly on the lenses, Glass contains a camera so the wearer can surreptitiously record or broadcast a video of what he or she is seeing. Well before Glass was ready for prime time, the team working on it made a video showing what the world would look like through Google Glass. It was a given that such a visionary (pun intended) concept—especially one coming from Google—would generate a lot of buzz and interest, but would that buzz and interest translate into commitment? Were enough people willing to invest real money to get their own pair of Google Glass? How would they use it? And, more important, would they keep using it after the initial geeky excitement wore off?

Sure enough, once the video introducing Glass was posted on YouTube, the buzz started big time. Everyone was talking about it; everyone had a prediction about how Glass would (or not) dramatically change the way we interact with the world. No surprises here, but also no data—just a bunch of Thoughtland opinions and speculations. How many people would actually be willing to part with some serious cash to buy a pair of Google Glass? And, even more important, how many of them would use it regularly? What would they use it for?

In order to turn a video of your yet-to-be-developed idea into a legitimate prototype, you must use it to collect more than online views, thumbs-ups, or comments. You must find a way to turn that video into a YODA-generating experiment.

The Glass team accomplished that by following the video demonstration with an offer to join the Google Glass Explorer program. To qualify for the Explorer program, you had to put quite a bit of skin in the game. First, you had to express interest by posting a message on Twitter using the #IfIHadGlass hashtag and describe what you'd do with Glass if you had it (e.g., #IfIHadGlass I would use it for a cooking show).

Thousands of people posted tweets with their ideas for using Glass. After reviewing these tweets, the Glass team selected a few thousand tweet authors and notified them that they had been accepted into the Explorer program. All they had to do to join was to pay \$1,500 for their Google Glass and travel (at their own expense) to a Google office in San Francisco, Los Angeles, or New York for fitting and training.

Now that's quite an investment of money and time—lots of skin in the game. Nevertheless, many people paid the fee, made the trip, sat through the training, and returned home with their own pair of Google Glass. At first, the Explorers were enthusiastic, some of them perhaps a bit too enthusiastic. A well-known tech blogger, for example, was so taken with Glass that he posted a photo of himself wearing them in the shower.

Unfortunately, the initial wave of interest was soon followed by a wave of criticism and a major backlash. Perhaps out of envy, perhaps because of the fact that they could be secretly recording a video, Google Glass wearers quickly went from being the focus of attention to being called “glassholes.” Many bars and restaurants banned their use. And,

worst of all, after the initial period of excitement, most Glass Explorers stopped wearing them.

Although Glass showed a lot of promise for some applications, the original set of expectations was not met, and the Glass project was canceled. The idea may be resurrected in some other form or for some other market, but—despite all the initial hype—this particular version of the technology at this particular time was not *The Right It*.

You might wonder if this is a case of prototyping giving a false positive—just like those focus groups and other Thoughtland techniques I criticized. After all, initial interest was high, and many people were willing to pay \$1,500 for Glass. Quite the contrary. Google Glass is a great example of how, for some products, initial levels of interest and commitment are necessary, but not sufficient to determine if a product is *The Right It*. The success of some products and services depends on *repeated* use and *continued* engagement.

It's relatively easy, especially for companies like Google and Apple, to create a lot of buzz for a new idea. The real test is whether that initial buzz translates into ongoing interest and consistent usage. By combining the YouTube prototype with the Explorer program, Google not only determined the initial level of interest in the product, but was also able to track how many of those initially enthusiastic Explorers remained enthusiastic after the initial excitement wore off.

Of course, the Google Glass team was disappointed, but they had never assumed success. If that were the case, they would have jumped ahead to manufacturing and tried to sell hundreds of thousands of units instead of validating the idea first.

Because in the movies anything is possible, the YouTube technique can be used to prototype any idea. But remember that metrics like views or thumbs-ups don't count as data. The key is to combine a video that shows your idea in action with a way to collect skin in the game.

The YouTube prototype can often be combined with another prototyping technique for even better results. Let me illustrate the power of this approach using some of my previous examples.

Example: The Smart Horn Revisited

A few pages earlier, we used the Pinocchio technique to prototype

the Smart Horn idea. We installed four dummy buttons for four different horn sounds in our car to see if, when, and how often we would use them. We can combine the Pinocchio with the YouTube prototype by making a video that shows the various buttons and sounds in action. We begin by recording a video of someone driving around in a Smart Horn-equipped car, showing how it would be used in various situations. A driver distracted by being on the phone does not notice the light has turned green and is given a polite *beep-beep*. Someone cuts our driver off and hears a more assertive “You #\$\$%^&*!”

Of course, no Smart Horn exists yet, so those buttons don’t do anything, but that’s where the magic of video comes in. With a little editing, you can add the appropriate horn sounds to the video soundtrack and create the illusion that the Smart Horn actually works. Once you have such a video, you can post it online and give people who watch it an opportunity to preorder it or give you their email to receive more information about it.

Example: The Portable Pollution Sensor Revisited

In addition to showing the product-to-be in action, the YouTube prototype gives you a wonderful opportunity for testing various stories or scenarios for marketing your idea. Remember the portable pollution-sensor idea we introduced in a previous chapter? The team believed that their first target market should be parents of children who live in very polluted cities. To validate their market hypothesis, they can make a video that tells a story of how two concerned parents used the portable pollution monitor to keep their daughter healthy by not letting her spend too much time outside when pollution levels are too high. For prototyping purposes, the pollution-sensor device shown in the video can be simulated by any nonfunctional object whose shape and dimensions are similar to the ones they are envisioning for the actual product.

Example: FeeBird

One product category that is ideally suited for the YouTube prototype is software. By turning a PowerPoint (or Apple Keynote) presentation into a video, you can simulate the functionality of any program or app you can envision without writing a single line of code. Let me give you an example.

Let’s say you have an idea for a mobile app called FeeBird. Your app

makes it possible for bird-watchers (let's give squirrels a break for now) to earn money from their hobby by sharing the location of rare or elusive birds that they've spotted—for a fee. As the developer of FeeBird, you will make money by selling the app for \$5 and by collecting 20% of each transaction.

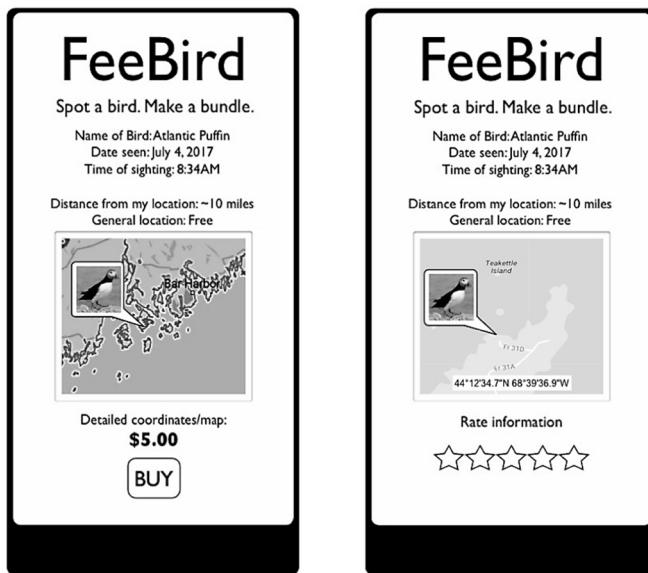
If you are a software developer, like me, you can't wait to fire up your computer and start coding. But let me ask you this: "Do you have any doubt that you can build such an app?" Of course not! It's just a SMOP (Small Matter of Programming). And even if you are not a software developer, you can safely assume that you can easily hire one to develop FeeBird for you. In other words, there is zero risk or uncertainty about building the app. There is, however, a nonzero cost associated with it. An app like this would take at least several weeks of work to develop, test, and debug. Since we know that most apps don't get many users or make much money, you should use prototyping to make sure that FeeBird is The Right It before you make that investment.

So instead of running to your computer and firing up your software development tools, fire up your favorite presentation or graphics software and use its capabilities to simulate what your app would do. Let me show you what I mean.

Below are mock-ups of two FeeBird screens I put together on Apple Keynote in about ten minutes. The first screen shows the general location of an interesting bird near you, along with an option to buy a precise map/location for a fee of \$5:

The second screen shows what happens if users decide to buy the detailed information: they get a detailed map, GPS coordinates, and a chance to rate the information.

In less than one hour, you can create several such slides, each with a mock-up screen showing the result of a user action (search for bird, report a bird, confirm a sighting). You can then combine them in an animated sequence to make them look like a functioning app. For example, when you click on the slide with the "Buy" button, the screen transitions to the next slide, which shows the detailed information on the bird location—and it would look to the viewer as if the click on the "Buy" button worked. Once you have put together an animated sequence, add some narration to com-



plete the demo:

After you've told FeeBird what kind of birds you are interested in, the Alert Screen will let you know whenever such a bird is spotted near you.

Here, for example, you can see that there's an Atlantic Puffin within a ten-mile radius of your current location. For \$5 you can get the precise location of the bird.

You click on the "Buy" button and now you see a screen with detailed map, GPS coordinates, and directions.

You drive to the location, hike a few hundred feet, and . . . success, there's a pair of Atlantic Puffins. Delighted, you give a five-star rating to your fellow bird-watcher.

At the end of this process, not only will you have a compelling and realistic-looking video demonstration of what your app will do, but you will have probably learned a lot about how you would design it and what features you would put into it.

But this is not a prototype yet; it's just a sophisticated, dynamic mock-up. To turn it into a prototype you have to use this video to collect some data. You can do that in any number of ways. For example, you can create a dedicated website where you show the video and give people an

opportunity to sign up to be notified when the app is released, or show the video at a bird-watching meeting and see if anyone is interested enough to give you some form of skin in the game (email address, money, etc.).

Return on Pretotyping Investment

This last example gives me an opportunity to introduce the concept of *return on pretotyping investment*: how a few dollars and hours invested in pretotyping an idea can save you from wasting a ton of money and time building The Wrong It.

Let's assume that you've invested ten hours and \$100 to create a polished YouTube pretotype of the FeeBird app in action, develop a simple website, and buy some online ads in order to collect some YODA. After one week, your video has collected two thousand views, a bunch of hostile comments (e.g., "No self-respecting bird-watcher would charge or pay money for this kind of information"), and—most important—not a single piece of skin in the game. You make some changes, run another ad campaign, and get similar results. You decide to try another tack, so you show your video at a meeting of bird-watchers—and you are booed off the stage. Ouch! Time to go back to the drawing board.

This outcome may be disappointing. But imagine how much more disappointed you would have been if, instead of ten hours, you had invested ten weeks (roughly four hundred hours of engineering time worth thousands of dollars) to develop a real app, only to find out exactly the same information about your market (i.e., zero sales and that most bird-watchers recoil at the premise of buying or selling such information). Ten hours and \$100 versus four hundred hours and thousands of dollars to learn the same lesson—that's a darn good return on your pretotyping investment, wouldn't you say?

Test a little before you invest a lot. Don't jump in bed with an idea until you've gotten to know it a bit better. And speaking of jumping in bed, let's move on to the next pretotyping technique.

The One-Night Stand Prototype

I named the One-Night Stand pretotype after the performing-art practice of holding a single performance of a play or show at a particular place—

but go ahead and associate it with the more salacious use of the term if you prefer.

As the name suggests, the main characteristic of the One-Night Stand pretotype is the lack of a long-term commitment or investment. It does not necessarily have to be exactly one night or one shot, by the way; don't take the name too literally. The duration of the pretotype experiment can be as brief as a couple of hours or as long as a couple of months; the point is that it's a relatively short-term commitment—just the time you need to collect enough data to make an informed decision. If you want 100 data points and you can get them in one day or with a single experiment, then make it one day and one experiment. If it will take you a week and multiple experiments to get the necessary data, take a week. Having said that, two of my favorite examples of One-Night Stand pretotypes in action come from Virgin Airlines and Airbnb, both of which began with a one-shot/one-night offer.

Example: Virgin Airlines

In the early 1980s, legendary entrepreneur Richard Branson had booked a flight to the British Virgin Islands to meet his then girlfriend for a romantic vacation. When his flight was canceled, instead of moaning, groaning, and cursing the airline—as most of us would have done—he decided to create his own One-Night Stand Airline. He borrowed a blackboard, wrote on it “Virgin Airlines / \$39 one-way ticket to BVI,” rounded up a bunch of the other passengers who had been bumped, and sold enough tickets to fill a chartered plane.

Encouraged by this successful experiment, he returned from his romantic vacation and decided to call Boeing: “Do you have any used 747s for sale?” They did. Branson grabbed one and stepped up from a one-flight pretotype to a one-plane airline pretotype. Eventually, Virgin Airlines went on to become one of the most successful and innovative airlines in the industry. Branson’s girlfriend must have also been quite impressed because she ended up marrying him.

Example: Airbnb

Sometime in 2007, two Airbnb cofounders, Joe Gebbia and Brian Chesky, could not pay the monthly rent for their San Francisco residence. To make some quick cash, they came up with the idea of renting out three

air mattresses in one of their apartment's rooms (hence the "air" in the Airbnb name) and, perhaps to make up for the somewhat uncomfortable sleeping conditions, included a home-cooked breakfast in the deal (hence the "bnb" in the name). They bought the airbed andbreakfast.com domain, created a simple one-page website with a map showing the location of their apartment, and advertised it on Craigslist. A few hours later, they had two men and one woman signed up for their one-night-plus-one-breakfast deal, paying \$80 each.

That's skin in the game—quite literally. Those three Airbnb pioneer customers risked their skin when they agreed to spend the night in a room with two strangers in the home of other strangers. I don't know about you, but all kinds of horror-movie plots come to mind; I am not sure I would have slept very peacefully that night. In fact, if someone had described this as an idea for a business, my Thoughtland-based opinion would have been: "It will never work. I'd never pay to spend the night in a stranger's home. What's wrong with a hotel—or a proper bed-and-breakfast?" This is another great example of how wrong our initial reactions, opinions, and predictions can be, because when I'm traveling these days, Airbnb is the first website I check, and more often than not, I end up booking one of its homes.

After the first guests left, Joe and Brian realized that this could be a big idea: The Right It. A few years later, with lots done right and going right, Airbnb was worth over \$10 billion. I suspect that Joe and Brian don't worry about being able to pay their rent anymore.

Example: Tesla's Pop-Up Showroom

Not only is opening up an auto dealership really expensive, it's a long-term commitment to one particular location. What if that location doesn't work out for some hard-to-foresee reason? How can you get some data to guide your decision? It sounds like a perfect job for the One-Night Stand prototype.

To expose its cars to new markets and test the level of interest in those markets, Tesla designed and built a portable pop-up auto showroom consisting of two modified shipping containers that could be easily trucked to a location and then expanded into a 20- by 35-foot showcase in a matter of hours. Not only could potential customers experience the cars firsthand,

they would also have an opportunity to make a \$5,000 deposit and place an order online—lots of skin in the game. The pop-up store could provide Tesla with great firsthand data on how well its cars would sell in a particular location with minimal commitment—brilliant!

Let's say that Tesla wants to open a new dealership in the greater Los Angeles area, but first it wants to figure out which LA location will result in the most sales. The presence and success of other luxury car dealerships in an area might be a good starting point, but it's OPD (Other People's Data), and you can't automatically assume that the people who buy from traditional luxury or sports car makers, such as Bentley, Mercedes, Cadillac, Ferrari, or Lamborghini, are the same people who would buy a Tesla. Tesla knows that its stores attract a lot of interest wherever it puts them, but how many of the people who visit a particular store are tire kickers and how many are serious potential buyers?

Tesla can use existing data to narrow down its options to three possible locations within a 20-mile radius and then combine its pop-up showroom with a One-Night Stand pretotype and an xyz hypothesis (e.g., at least 0.5% of people who walk into the Beverly Hills showroom will place a deposit for a Tesla Model S) and get some valuable YODA—not “This is a good location for a luxury car dealership,” but “This is a good location for a *Tesla* dealership.”

Test a Little Before You Invest a Lot

As with most pretotyping techniques, the One-Night Stand seems obvious in retrospect; it's a simple matter of applying the “test a little before you invest a lot” concept to the dimension of time: try it one time or for a few hours, days, or weeks. In other words, before making a long-term commitment, validate your long-term XYZ Hypothesis with a short-term xyz experiment.

But rational ideas, no matter how obvious they might be, don't always translate into rational actions. When I look at how most people and organizations approach investing in a new idea, I see the exact opposite happening: organizations sign long-term commercial-space leases and make all kinds of long-term commitments without the data necessary to prove that their idea will work.

In the past, I've been as guilty of this as anyone. In the businesses I

started, I regularly signed long-term leases for thousands of square feet of space (enough for dozens of employees in engineering, sales, marketing, operations, etc.), even though at the time we had just a few employees, we were at least one year away from having a product we could sell, and the only validation we had for our product was a bunch of opinions with no skin in the game.

The Infiltrator Prototype

Sometimes creating or manufacturing a new product on a small scale requires minimal investment and little risk. The big risk is investing too much to build it right and manufacture that new product in quantity before you have enough data to confirm that there is sufficient interest or demand for it. Wouldn't it be great if you could use a small batch—perhaps even just a single unit—of your idea and leverage someone else's marketing and sales resources to see if anyone would buy it?

That's where the Infiltrator prototype comes in. As the name suggests, the Infiltrator technique involves sneaking your product into someone else's existing sales environment (it could be physical stores or online) where similar products are normally purchased to see if people will be interested enough to put some skin in the game and buy it.

Example: Walhub

My inspiration for and favorite example of this prototyping technique comes from Justin Porcano, who leads an independent design firm in San Francisco called Upwell Design. Justin had an idea for an innovative switch plate, that rectangular piece of plastic or metal that goes around a light switch to protect the wall from finger smudges. His switch-plate design, which he called the Walhub, has hooks and pockets that you can use to conveniently hang or store things like keys, umbrellas, or flashlights. You can put a Walhub, for example, on the switch near your front door to secure your keys and hold letters that you have to mail or near the cellar door to hold a flashlight in case the lights go out and you have to check the breakers in the basement.

Like all inventors, Justin thought that his idea was great and believed that other people would see its usefulness and buy it. He also thought that

home improvement and furniture stores, like IKEA or Home Depot, would be a great place to sell it. However, unlike most inventors, Justin was wise enough to seek data to validate his beliefs (smart man!) and came up with a unique way to do that.

First, Justin went on eBay and bought a used IKEA employee shirt. Next, he created a few official-looking IKEA product labels and price tags and put them on a handful of early prototypes of the Walhub. As a final clever touch, since IKEA is famous for its quirky Nordic-sounding names, for the purposes of this experiment he changed the name of his product from Walhub to Wälhub to make it even more believable.

With a couple of accomplices, he infiltrated the local IKEA store wearing a yellow employee shirt and carrying a bagful of Wälhubs. After looking around to make sure no real IKEA employees were nearby, he proceeded to put a handful of Wälhubs on display in several areas of the store where IKEA shoppers had a chance to see them—and purchase them. Since he was wearing an official-looking IKEA shirt, other employees assumed he was one of their colleagues setting up a new display.

Then he stood back to observe how people reacted to his product. How many people would stop to check it out? How many, if any, would put a Wälhub in their big blue IKEA bag and buy it, thinking it was a bona fide IKEA product? Which location within the store (e.g., kitchen, living room, garage) resulted in the most interest and sales?

It turns out that people were interested in the Wälhub, and several shoppers put one in their shopping bag and headed for the checkout. As you can imagine, Justin's fake IKEA price tags did not scan properly, and the cashiers did not recognize that product. Despite a bit of confusion at the checkout stand, in the end everyone who tried to buy a Wälhub was able to take it home for free. This was a win-win: customers got a freebie, and Upwell Design got valuable YODA. Justin and his team filmed the whole experiment and posted a short video on YouTube (search for "Upwell Walhub Ikea"). The video is well worth seeking out, because it's quite inspiring to watch the technique in action, and it's also quite funny.

What a brilliant way to validate an idea and to go from opinions to data. When it comes to skin in the game, customers putting a Wälhub in their bag expecting to pay for it is as good as it gets. Here's an exam-

ple of the kind of YODA Justin might have collected using the Infiltrator prototype:

- Duration of experiment: one hour
- Number of people who walked by the display: 240
- Number of people who picked up a Wälhub to check it out: 12 (5%)
- Number of people who tried to buy a Wälhub: 3 (1.25%)

Of course, what Justin did took more than creativity; it took guts and it involved risk. I don't know the penalty for infiltrating a major chain store pretending to be an employee of that store and using its retail space for your own market-research purposes. But I suspect that, if caught, you might get into some trouble—or get a lot of press and attention for your product (which is exactly what happened in this case).

The good news is that you can use the very same technique without having to risk being arrested. It's not as exciting as infiltrating an IKEA store under false pretenses, but Justin could have offered several independent hardware store owners a bit of money (say \$100) in exchange for putting his product on display in their store for a couple of weeks to see if anyone would buy it.

Let me end this example with a few words from the daring Justin Porcano about his IKEA experiment:

The results were better than I could've hoped for. The experiment helped to not only validate a consumer market for the product, but it revealed information about the effectiveness of the packaging, the price point, and ideal locations within a retail space. Beyond the research aspect of the experiment we used the video as a marketing tool. We received 75,000 YouTube views, an interview on national TV, and creative accolades from press outlets like Advertising Age. Upwell was able to stretch a \$600 marketing budget about as far as it would go while receiving valuable sales and market research.*

* <http://www.upwelldesign.com/portfolio/walhub-hacks-ikea-video/>.

It's fun, exciting, and illuminating to run an Infiltrator prototype in brick-and-mortar stores, because you can not only keep track of numbers, but also observe how people react to your product. If most people pick up your product, look at the price, let out a whistle, and put it back, you can safely deduce that they think it's too expensive. However, since a significant—and growing—amount of shopping has migrated to online stores, you might wonder if you can run an Infiltrator prototype on the internet. Of course you can! And you should. Taking advantage of an established website's traffic is almost always a much cheaper and faster option than trying to attract traffic to a new website.

First, you need to identify an existing online retailer with an established client base that already buys products in the same category as yours. Next, you contact the retailer and work out a deal to display your product on a trial basis. In exchange for the data you will collect, for example, you can offer to let the retailer keep any of the revenue from the sales or throw in a few dollars to "rent" a bit of its online presence—it will be well worth it. As always, it's easier to contact and work with smaller businesses than giant corporations to run this kind of trial.

The Relabel Pretotype

The Relabel pretotype takes advantage of the fact that with a minor change in external appearance, we can leverage an existing product or service to pretotype a new product or service. By putting a different label on a product, you can pretend that it's something other than what it is—and see if people are interested in it.

Sounds fishy? Not really, but this pretotyping technique did have a fishy origin.

Example: Second-Day Sushi

A few years ago, while I was having lunch with a small group of Stanford undergraduates, a plastic box of prepackaged sushi pieces, the lunch choice of one of the students, triggered the following conversation:

"How's that sushi in a plastic box?" asked one of the students between chomps of a cheeseburger.

“Expensive! Almost ten bucks . . . but at least they gave me a free pair of chopsticks.”

“I think it’s a rip-off,” interjected a third student, who was eating a bowl of chili. “There’s no reason for it to be so expensive; it’s just a bunch of rice with a few little pieces of fish.”

“Ah, but the fish has to be really fresh—and fresh fish is expensive,” replied Cheeseburger Guy.

“I bet that I could feed you not-so-fresh sushi, and with all that soy sauce and wasabi you couldn’t tell the difference,” said Chili Gal. “In fact, I bet you there would be a big market for cheap sushi.”

“Yeah, right. I can already see it. Second-Day Sushi. Better go grab that domain name right now,” said Cheeseburger Guy, laughing.

“Actually, I don’t think it’s such a bad idea,” said Sushi Man. “If it were reasonably tasty, safe to eat, and cheap enough, I’d consider Second-Day Sushi. Heck, I’d eat sushi every day if I could afford it.”

“I doubt it,” said Cheeseburger Guy. “And even if you were crazy enough to actually go for it, no sane person would risk food poisoning or worse just to save a few bucks.”

Normally, I avoid work-related conversations when I am eating, but in this case I could not resist turning this Thoughtlandish exchange of opinions into a teachable moment: “You know, we could settle this matter with a pretotype . . .” Ten minutes later we had a plan.

First we came up with an XYZ Hypothesis:

At least 20% of packaged-sushi eaters will try Second-Day Sushi if it’s half the price of regular packaged sushi.

Then we hypozoomed to the Stanford campus:

At least 20% of students buying packaged sushi at Coupa Café today at lunch will choose Second-Day Sushi if it’s half the price of regular packaged sushi.

Finally, after a minute of *pretostorming*—brainstorming about ways to pretotype the idea—we came up with the Relabel technique. We would create some labels that said, “Second-Day Sushi: 1/2 Off!”, put those labels on half of the boxes available for sale at the café, and then count what percentage of people who buy sushi for lunch decided to risk food poisoning and intestinal parasites to save a few bucks.



As you can probably guess, the idea for Second-Day Sushi may have sounded plausible to some (in Thoughtland), but when tested in the real world it proved difficult to find anyone (let alone 20% of the market) willing to take the bait (sorry!). The Second-Day Sushi idea was dead in the water—ha!

As you may have noticed, this example combines a Relabel pretotype with an Infiltrator pretotype. We are leveraging not only an existing product and package, but also an existing customer base and infrastructure (i.e., the café and its lunchtime traffic). By combining multiple pretotyping techniques, you can dramatically reduce both the cost and the time it takes to run your experiments. In the pages that follow, you will see several additional examples of pretotyping combinations. And speaking of pages . . .

Example: Book Covers

You can't judge a book by its cover, but you can get some market data from it. My friend Mike—not his real name, his real name is Steve—is an avid collector and disseminator of (mostly bad) computer-programming jokes. His collection includes such pearls as: “What do you call a programmer from Finland? Nerdic.” (And you thought my puns were lame.)

Since I've known him, Mike has been talking about putting together a book of such jokes. He's convinced that many programmers would buy it: “Dude, I'd sell a ton of them. It would be the perfect gift for geeks.” He also thinks he has the perfect title for it: *100000000 Programming Jokes*. (100000000 is the number 256 written in binary code. Get it? If not, don't worry. I can guarantee you're not missing much.)

Once again, the idea sounds plausible, and I am sure he would sell some, but how many? Could he sell enough to justify the effort and ex-

pense to produce the book? With prototyping, he could get some YODA to answer those questions, and a Relabel prototype combined with an Infiltrator would work extremely well in this case.

First we have to translate his fuzzy Market Engagement Hypothesis into an XYZ Hypothesis. Time to “say it with numbers.” Mike thinks that “most programmers will buy the book,” but when we translate “most” into a number we get “at least 50%”—and even Mike realizes that that sounds a bit too optimistic. Eventually, he settles on a more realistic number, and we converge on the following XYZ Hypothesis:

At least 5% of programmers will buy *100000000 Programming Jokes* for themselves or their friends at \$9.95.

Then we hypozoom to an xyz hypothesis we can test with the help of a local bookstore (yes, a few still exist):

At least 25% of programmers browsing for computer science and programming books at Books Inc. in Mountain View who see a book titled *100000000 Programming Jokes* on the shelf will pick it up and examine it.

If the XYZ Hypothesis is correct, then our xyz experiment should reflect it. In other words, if at least 25% of the people who see the book cover while browsing the computer programming section pick it up and check it out, it’s plausible that one out of five of them would buy it—if it were an actual book of jokes. We can test the hypothesis by relabeling an existing book with a realistic-looking *100000000 Programming Jokes* cover, putting that book on the shelf, and then counting how many people who see the title pick it up.

Once they open the book, of course, they will realize that you truly can’t judge a book by the cover. But at that point, Mike can swoop in, explain the experiment, apologize for the little trick, and give them a small gift to make up for it (perhaps a sample page from the book with ten of his jokes). Or he can explain that the book is not ready yet, but if they give him their email address (some skin in the game), he’d be happy to send them a copy. If the data from a few experiments like this confirms his hypothesis, Mike can begin working on a proper book.

Of course, you can do a similar test online (and you probably should do that to confirm the brick-and-mortar store results), but it’s so much

fun to do some of these experiments in person. Just remember to keep it legal and ethical—and be generous with the people who provide you with YODA.

Pretotyping Variations and Combinations

I've had fun collecting and sharing what I believe are compelling examples of pretotyping techniques and assigning them memorable names. But I want to emphasize that the list of pretotypes I've shared with you is by no means exhaustive; think of it as just a few examples of the *pretotyping mindset* at work. Consider it a source of inspiration for coming up with *your own* pretotyping techniques, variations on the existing techniques, or combinations of two or more techniques. You've already seen me do this: the Second-Day Sushi example combined an Infiltrator pretotype with a Relabel pretotype. But let me share with you two more examples.

Example: The Live Demo Pretotype

Instead of using an online video to help the audience visualize and understand the potential of a not-yet-functional product in action (as you would with a YouTube pretotype), you put on a demonstration in front of a live audience—as they used to do in markets in the days before television and way before YouTube.

Let's assume that you have an idea for an app to help students relax and focus, so they are in a better mental state for studying and for test taking. You have done enough research and experiments on yourself and your friends to convince you that such an app would work. But before you invest weeks or months in properly developing, testing, publishing, and marketing it, you want to know what percentage of people in your target market would be willing to pay \$5 for it. You go through the steps with which you are familiar (i.e., Market Engagement Hypothesis → XYZ Hypothesis → . . .) until you have a neatly hypozoomed xyz hypothesis you can test right away, something like this:

10% of Stanford students going to the campus bookstore at lunchtime today who stop to watch a three-minute demonstration of the Relax, Focus & Study app will give us their stanford.edu email address to be notified when our app is launched.

You set up a table and chair near the bookstore and put on a little show:

Come gather around to see the amazing power of our Relax, Focus & Study app. By using a combination of scientifically proven visual and audio cues, our app will turn your smartphone into an amazing tool to reduce anxiety and induce a state of relaxed focus, ideal for studying, in as little as two minutes. My partner Lisa, sitting here, is connected to a heart-rate and blood-pressure monitor. As you can see, right now Lisa's high heart rate and blood pressure indicate that she's in a state of mild stress—no surprise with all of you strangers staring at her. But watch what happens as Lisa puts on her earbuds, starts the Relax, Focus & Study app, and concentrates on the screen. Wait for it... Wait for it... There! Her heart rate has begun to drop along with her blood pressure. And it continues to drop...

You get the idea.

At the end of the demonstration, you tell the students in the audience that the app is not yet ready for launch, but if they go to RelaxFocusAndStudyApp.com and enter their stanford.edu email address (not some throwaway email account like myantispamemail@hotmail.com), they will be able to buy the app for \$1 instead of \$5 when it launches. Or if you have enough chutzpah, you can try to collect the \$1 at that time—even more skin in the game—provided you are prepared to refund the money if necessary. Of course, for the demonstration, you don't need to have a working version of your Relax, Focus & Study app; just find a video with music that helps Lisa relax and use that for now.

Example: The Morsel Prototype

We've already used a Fake Door prototype to validate the market interest for a nonfiction book on squirrel watching and a Relabel prototype to test a novelty book of nerdy jokes. But let's assume that you are thinking of writing something a bit more ambitious and literary in nature, like a novel. In addition to being written in good-enough prose, to be successful a work of fiction must engage the readers with an interesting story and great characters.

Most authors, like most entrepreneurs and inventors, believe that their idea for a story and their characters will be as captivating to the market as they are to them. Unfortunately, that's not the case. The Beast

of Failure has a taste for ink, and most books (especially novels) fail in the market. Even if you could persuade a literary agent or a publisher to read your masterpiece, their decision (which is usually a rejection) will be based on their expert opinion and understanding of the market—and we know how that plays out. Some of the most successful novels in history, from *Harry Potter* to *Moby Dick*, were originally rejected by scores of publishers.

So what are fiction authors or publishers to do to maximize the chances that a book will be successful? They should prototype the book by writing a few sample chapters, expose those chapters to a subset of their target audience, and collect some YODA. In other words, give the market a few free morsels of your yet-to-be-completed work and then ask for some skin in the game in exchange for more.

There are many ways to do this, but my favorite example these days comes from Andy Weir, author of the novel *The Martian*, a story about an astronaut marooned on Mars. A software engineer by trade, Weir enjoyed writing science fiction as a hobby but, like most aspiring writers, he experienced nothing but rejection after rejection from literary agents and publishers. Frustrated, he decided to post chapters from *The Martian* as a free serial on his website. A growing number of people came back week after week to read what happened to the marooned astronaut. Eventually, he had thousands of online fans who not only read the chapters, but volunteered their time to edit, fact-check, and suggest ideas for the book—all examples of skin in the game and a great initial indicator of market engagement.

But the story gets even better. At the request of some of his fans, Weir published the novel on Amazon as a Kindle e-book. He wanted to make it available for free, but was forced to ask for a little bit of skin in the game since the minimum price Amazon would allow was 99¢. In three months, he sold over 30,000 copies—enough to qualify as a science-fiction bestseller on Amazon’s ranking. With that kind of YODA, you don’t need to knock on agents’ or publishers’ doors; they come knocking on yours. In a matter of weeks, Weir had an agent, a publisher, and an offer from a major studio to turn the book into a motion picture. Both the book and the movie were spectacularly successful. *The Martian* was The Right It.

What Makes a Prototype a Prototype

As you have seen, prototypes can take many forms. But to be worthy of the name, a prototype must satisfy three key requirements:

1. A prototype must produce YODA with skin in the game.
2. A prototype can be implemented quickly.
3. A prototype can be implemented cheaply.

But even if you stick to these requirements, you will have a large number of prototyping techniques and combinations to choose from. Which raises a few questions:

How do you choose which prototypes to use?

How many different experiments do you need to run?

How much data do you need to collect?

When can you stop testing?

To help you answer those questions, you need a final set of tools, analysis tools, which are the focus of the next chapter.

6 Analysis Tools

You have learned how thinking tools, like the XYZ Hypothesis and hypozooming, help you transform a fuzzy and poorly articulated idea into a clear, objective, and testable set of hypotheses. You have also learned how prototyping tools, like the Mechanical Turk or the Fake Door, make it possible for you to collect data about your idea by testing those hypotheses quickly and inexpensively. Now you will learn how analysis tools can help you make sense of the data you collect, so you can make that important transition from data to decision.

The Skin-in-the-Game Caliper

I've already introduced and casually used the term *skin in the game* a number of times. Now it's time for a more thorough introduction and discussion of this key concept along with examples of how to put it into practical use.*

It's not clear who is responsible for coining the term *skin in the game*. Some people attribute it to legendary investor Warren Buffett; others have found no evidence to support that claim. For the longest time, I thought it was a phrase used by fur trappers playing poker and betting animal skins instead of dollars. (Not a bad guess given that the slang term *bucks*—as in dollars—comes from *buckskins*, which were considered a form of currency at some point.) Despite a lack of consensus about the origins of the term, most people agree on its meaning. Generally speaking, in the expression *skin in the game*, the word *game* is a metaphor for some activity with a possibility of winning or losing, and *skin* is a metaphor for having something of value at risk in that game (e.g., money, time, or reputation). In the context of this book, the *game* consists of bringing a new idea into the

* I highly recommend Nassim Nicholas Taleb's *Skin in the Game: Hidden Asymmetries in Daily Life* (New York: Random House, 2018) for a broader and more in-depth discussion and analysis of this important concept.

market, and winning or losing is determined by whether that new idea succeeds or fails in that market.

Unlike games such as chess or football, our game is not a zero-sum game: one new product's loss (market failure) does not necessarily mean that another, similar product will win (market success). It is also extremely rare for any one product to capture 100% or even 90% of any one market and continue to dominate that market for decades. In other words, it's a tough game to win and, as we've seen, most people lose at it most of the time. If you lose (your idea fails in the market), you lose all or most of your investment (skin). But if you win (your idea succeeds in the market), you get your investment (skin) back along with a few—and sometimes a lot of—extra pelts. And it's those extra pelts that make the game and its risks worthwhile and fun for many of us.

Entrepreneurs, inventors, and investors in a new idea automatically have skin in the game and typically lots of it. If Mark quits his good job, takes on a second mortgage, and works eighty hours a week to start a new business, he's putting all kinds of skin in the game, including money (giving up a salary, taking on a loan) and time (eighty hours a week)—he's risking a lot.

When Mary convinces her company to invest in her new invention to let her lead its development, she may not be using her own money and the company is paying for her time, but she's investing (and risking) her reputation, future promotions, and earning potential if the invention fails to meet the expectations she set for the company. When venture capitalists or angel investors decide to invest in an idea, they are investing and risking not only money and time, but also their reputation in the VC community.

As an entrepreneur, inventor, or an investor in a new idea, you have no choice but to have some and often a lot of skin in the game. Everyone knows and expects this—and it's the way it should be. Having skin in the game means that you have something at risk, something significant to gain or lose, depending on the outcome. This skin is an indication that you are serious about the idea, that you have done your homework, and that you will deal with challenges as they arise rather than abandoning your idea at the first sign of trouble. Skin in the game indicates commitment,

seriousness, and thoughtfulness.

But make sure that you don't put too much skin in the game to develop an idea unless and until you can demonstrate that the market is interested enough in the idea to put up some skins themselves. Not opinions, not predictions, but skin!

Until and unless individuals put some of their own skin in the game, you can't count those people as likely customers or users. Rather, they are merely spectators—they've got nothing to lose. Heck, they might even enjoy watching you and your idea crash and burn.

When it comes to gauging an idea's market potential and likelihood of success, our deliberation must be based on hard data, and the data must come with some skin in the game. I'll say it again, because this is so important: *before you put a lot of skin in the game on your idea, make sure you can get some skin in the game from your target market.*

But what qualifies as skin in the game from your target market? And how much of it do you need?

To help you answer those important questions, I've developed a Skin-in-the-Game Caliper that you can use to assign skin-in-the-game "points" to different types of target market responses. The caliper can be customized for specific products and markets.

Example: The Tortell-o-matic

Let's assume that my idea is the Tortell-o-matic, a \$250 automated tortellini maker for the home. Fill the Tortell-o-matic with flour, water, eggs, salt, and your choice of filling, and two minutes later this amazing machine starts to pop out perfectly shaped, freshly made tortellini. Yum!

In Thoughtland (and tummyland) the Tortell-o-matic scores great. Many of my friends and colleagues love the idea and say that they would definitely buy a Tortell-o-matic for their home (yeah, right!). Unfortunately, designing, developing, and manufacturing the Tortell-o-matic will require a big investment. Even a one-off prototype is likely to take months and cost tens of thousands of dollars. That's a lot of my own skin in the game. So before I undertake that investment, I need something more tangible than opinions and promises from my target market. I need to see some YODA with some of their skin in the game in it.

I can use one of my favorite prototyping techniques, the Mechanical

Turk, to set up an experiment and collect that YODA. A dummy Tortell-o-matic sits on a table. Under the table and hidden by a tablecloth, an accomplice holds a bucket of premade tortellini. When I push a button on the nonfunctioning machine, the accomplice plays a recording of mechanical noises and begins to push tortellini through the machine. I can use this prototype to collect YODA in a live demo, an online demonstration, or both.

The question is, after people get to see the Tortell-o-matic in action, how do I assign skin-in-the-game points to the responses I get from the market? That's where a Skin-in-the-Game Caliper comes in handy. As an example, here's the Skin-in-the-Game Caliper for my Tortell-o-matic:

As you can see, I am brutally selective when it comes to assigning skin-in-the-game points. For example, I ignore not only random opinions, but also so-called expert opinions. Why? Because we know from experience that all too often experts don't know any better than nonexperts. For example, the people at Ford who came up with the idea for the Edsel, one of Ford's biggest commercial failures, were experts in the automotive market with many previous successes under their belt. Similarly, few companies have more expertise in developing internet products than Google, and yet Google failed with Google Wave, Google Buzz, and many other internet-based ideas. At the same time, many ideas that experts considered to be hopeless turned out to be huge successes. Author J. K. Rowling, for example, received many rejections by the world's most experienced publishing houses before an editor took a chance on the first *Harry Potter* book. That's why opinions—of any kind and from anyone—rate a zero on the caliper.

I also assign zero points to online likes, thumbs-ups, thumbs-downs, tweets, retweets, comments on social media, polls, and surveys of all types. It's too easy to mindlessly click on a like, make a comment, or write a tweet. No skin = no points.

The smallest piece of evidence I consider data in this example is a *validated* email address given willingly to you by its owner with the understanding that it will be used to contact the person for offers, updates, or information about your specific product idea. By validated, I mean that it must be a bona fide email account that the person actually checks and

Type of Evidence	Examples	Skin-in-the-Game Points
Opinion (expert or nonexpert)	“Great idea.” “Nobody will buy it.”	0
Encouragement or discouragement	“Go for it!” “Keep your day job.”	0
Throwaway or fake email address or phone number	bogusemail@spam.com, (123) 555-1212	0
Comments or likes on social media	“This idea sucks,” thumbs-up or thumbs-down, Like	0
Surveys, polls, interviews online or off	“How likely are you to buy on a scale of 1–5: ____.”	0
A validated email address with the explicit understanding that it will be used for product updates and information	“Give us your email to receive updates about the product: ____.”	1
A validated phone number with the explicit understanding that you will be called for product updates and information	“Give us your phone number so we can call you about our product: (____) ____-____.”	10
Time commitment	Come to a 30-minute product demonstration	30 (1pt./min.)
Cash deposit	Pay \$50 to be on the waiting list	50 (1pt./\$)
Placing an order	Pay \$250 to buy one of the first 10 units when available	250 (1pt./\$)

uses regularly, not the kind of throwaway email address people often give when they are forced to do so. Based on my experience (and probably your own) most people are protective of their main email address and cautious about whom they give it to, which means it has some value to them. Because of that, I consider a validated email address—as long as it’s know-

ingly and willingly* given to you for the purposes of being informed about your specific product—to be skin in the game, but the very smallest possible unit of it: 1 point.

A validated phone number, on the other hand, I value at 10 points. Why so much more than an email address? Because most people are at least an order of magnitude more protective and careful about sharing their phone number than they are about sharing their email address.

I value a person's time at 1 point per minute. If someone is willing to invest 30 minutes to listen to a new product presentation, they get 30 points of skin in the game, because it's a reliable indication that they have a bona fide interest in the product.

Finally, we come to the ultimate type of skin in the game: buckskin—good ol' money. As the saying goes, "Money talks, opinion walks." All right, that's not exactly how the saying goes. I've taken the liberty of replacing bovine dung with something even less appealing and useful: opinions. At least manure can be used as fertilizer. When it comes to money, I keep things simple: \$1 counts for 1 skin-in-the-game point.

Feel free to substitute your local currency and adjust accordingly for your own idea or market, but don't try to be too precise (e.g., an email is worth .4 points and a phone number 3.7). Instead, think in terms of orders of magnitude (e.g., 1, 10, 100) and use your intuition and experience to guide you in making those determinations. The distinction and clear separation between no skin (zero points) and skin is far more important than being precise with the points. Just make sure you are honest, objective, and reasonable in assigning relative values: for example, a \$50 deposit should count much more than a validated email address.

Example: A Tale of Two Teams

I use the Skin-in-the-Game Caliper all the time when students or clients come to me with what they *think* is data. Here's a sample scenario.

Imagine that two teams, Team A and Team B, come to you with two different opportunities to invest in a new product idea. Team A makes

* You should assign no value at all to the kind of email addresses that you collect (or buy) without the owner's explicit consent and for the purpose of spamming (e.g., "Get 100,000 email addresses for only \$39.99").

their case by showing you a flashy YouTube video demo of their future product in action. Then they flaunt a bunch of skinless metrics:

In one week we got 140,000 views, 20,000 thumbs-ups, and only 100 thumbs-downs. And look at these comments: “Awesome,” “This will be great!” “Amazing idea” . . .

Team B also has a video, but instead of using total views, or thumbs, or comments to make their case, they present their data as follows:

We made a two-minute video to demonstrate what our product does and spent \$400 on online ads to draw traffic to it. In one week, our video was seen in its entirety by 8,000 people. At the end of the video we gave people an opportunity to give us their email address to be informed when the product will launch. We received 120 email addresses, 40 of which were not confirmed, so we only counted 80 as valid. A week later we sent a follow-up email to those 80 people giving them the opportunity to buy a prerelease (handmade) version of the product for \$125 (a 50% early-adopter discount), and we received 20 orders.

Using the above Skin-in-the-Game Caliper, how many points does Team A get? That's easy. Zero! Goose egg! To an untrained eye, the absolute numbers presented by Team A may be more impressive than those of Team B (i.e., 140,000 views vs. 8,000), but they are not good YODA. Team A did not even tell us how much time or ad money they spent to get those 140,000 views.

Team B, on the other hand, gave us not only numbers, but also the context in which to evaluate them and, most important, their YODA comes with some skin in the game:

\$400 in ads → 8,000 views = 0 skin-in-the-game points (no points for views, but an interesting first estimate for how much it will cost to get a view, i.e., \$0.05/view)

8,000 views → 80 validated email addresses = 80 skin-in-the-game points

80 emails → 20 orders for \$125 each = 2,500 skin-in-the-game points

It's entirely possible that Team A's idea is The Right It and that it will be a smashing success. They have a lot of impressive-looking numbers to share, but to be convinced I'd need to see real data—YODA with skin in

the game. I'd give them a copy of this book and ask them to come back to me with some pelts.

On the other hand, Team B has given me real and useful data: a \$400 investment in ads resulted in 80 validated email addresses (\$5 per address) and \$2,500 in orders. I would, of course, want to run a few more experiments (I'll show you why and how in a bit), but it's a promising beginning.

All data is not created equal—even YODA. Team A focused on quantity, while team B focused on quality. Quality of data beats quantity of data, and there's no better indication of data quality than data that comes with a lot of skin in the game attached to it.

The TRI Meter

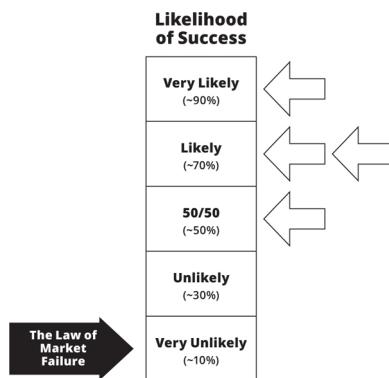
Collecting YODA with skin in the game to validate our Market Engagement Hypothesis is a necessary first step, but raw data by itself is not sufficient. In order to extract value out of data and use it to make rational and well-informed decisions, we need a way to interpret it, put it on a scale, compare it, and combine it with other relevant data.

The data from a cholesterol test, for example, is just a ratio of two values, the number of milligrams of cholesterol per deciliter of blood. Let's say you go for your annual physical exam, take a blood test, and learn that your total cholesterol is 300. That number by itself is not very meaningful, but when the doctor pulls out a chart and shows you that, statistically speaking, people with a cholesterol level of 300 are 4.5 times as likely to die of heart disease than those with a cholesterol level of 200, you may decide to lay off cheeseburgers for a while.

The Right It Meter, TRI Meter for short, is a visual analysis tool I developed to help you interpret the YODA you've collected as objectively as possible. More precisely, the TRI Meter is a gauge to help you estimate how likely it is that an idea will succeed in the market, but it's a gauge that is not too technical, complicated, or confusing—which can easily happen whenever probability and statistics are involved.

First let me show you what the TRI Meter looks like in action. Then I will explain how to use and interpret it. The image below shows a TRI Meter after four prototyping experiments (represented by the four white

arrows on the right). As you can see, the TRI Meter scale is partitioned into five likelihood-of-success categories ranging from Very Unlikely (10% chance of success) to Very Likely (90% chance of success), each representing the likelihood that your idea is The Right It.



Why only five categories and not seven or ten? I've debated whether to add more categories (e.g., Extremely Unlikely and Extremely Likely), but that would only add complexity and suggest greater precision than our experimental tools and subjects (i.e., people) can provide. It would also bring us uncomfortably close to a feeling of certainty in an environment in which there are no guarantees. That's why the scale goes from 10% to 90% in broad 20% increments instead of 0% to 100% with finer increments. I am a big fan of "say it with numbers," but asking for too much precision or too much confidence in these kinds of predictions reminds me of the following lines from Dr. Seuss's wonderful 1990 book *Oh, the Places You'll Go*:

And will you succeed?

Yes! You will indeed

98 and 3/4 percent guaranteed.

Those lines always bring a smile to my face. Unfortunately, I can't give you anything as precise as that 98 and 3/4 percent guarantee—nobody (with the possible exception of Dr. Seuss) can. And I am pretty sure that the good doctor himself could not have predicted that *Oh, the Places You'll Go* would sell over 5 million copies and still be a bestseller two decades later—talk about a book that is The Right It.

If you are working in physics or chemistry, and your instruments are precise enough, a measured difference between, say, 62.7% and 63.3% may be both accurate and relevant. But here we are dealing with the behaviors of markets and people, which are much harder to quantify. However, if you properly apply the tools and techniques I've been sharing with you, you will significantly increase your chances of success—80ish percent guaranteed!

Now about the arrows in the diagram. That big, ominous black arrow at the bottom labeled “The Law of Market Failure” points to Very Unlikely. That arrow is there to remind us of the hard fact that most new ideas will fail in the market. It’s there to help us make sure that we run enough experiments and collect enough YODA to counteract the dismal initial odds for success predicted by the Law of Market Failure. Let me use an analogy to illustrate this important point.

In US criminal courts, a defendant is considered innocent until proven guilty beyond a reasonable doubt. Not only that, but the burden of proof falls on the prosecution. In a criminal trial, a defendant does not have to prove his or her innocence; it’s the prosecution that has to provide enough convincing evidence to prove guilt. But when we move from criminal law to the law of the market to put an idea on trial, we begin with a presumption that our idea is “guilty” of being The Wrong It—that it will fail in the market. It is our job to provide enough hard evidence to swing the jury in favor of the idea.

In a similar vein, astronomer Carl Sagan popularized the phrase “Extraordinary claims require extraordinary evidence.” Products that are The Right It are not exactly *extraordinary* (there are too many of them), but they are the exception rather than the rule. So perhaps our motto should be: “Exceptional claims require sufficient evidence to support the exception.” The only evidence that our court will allow is YODA with skin in the game. And the only way we can get that YODA is by running pretotyping experiments. Which brings me to the other arrows.

Each of the smaller, light-colored arrows to the right of the scale represents a separate pretotyping experiment and how that experiment maps onto a specific likelihood of success. To perform that mapping, you have to determine if and how well the data you collected from an experiment

corroborates (i.e., supports) your hypotheses. One way you can do this is by asking the following question after you've run an experiment and collected data from it:

If this idea is destined to succeed in the market, assuming that it is competently executed, how likely is it that this particular pretotyping experiment would have produced this result?

Remember that a pretotyping experiment is designed to test a specific xyz hypothesis, which is derived from a Market Engagement Hypothesis. So what you are really asking is this:

If our MEH is true, how likely is it that this pretotyping experiment would generate this data?

Here's a guideline to help you map your answer on the TRI Meter:

If the data **significantly exceeds** what the hypothesis predicts, you point the arrow to **Very Likely**.

If the data **meets or slightly exceeds** what the hypothesis predicts, you point the arrow to **Likely**.

If the data **falls a bit short** of what the hypothesis predicts, you point the arrow to **Unlikely**.

If the data **falls really short** of what the hypothesis predicts, you point the arrow to **Very Unlikely**.

Finally, if the data is for some reason **ambiguous, potentially corrupted, or hard to interpret**, you point the arrow to **50/50** or optionally discard it. After all, even in science not all experiments produce clean and reliable data.

Example: Second-Day Sushi

All of this sounds more complicated than it actually is, so let me show you how to use the TRI Meter on our Second-Day Sushi example, which, unlike the fish itself, should still be fresh in your mind. First, we have to make sure that we have an XYZ Hypothesis that we can hypozoom into a set of xyz hypotheses.

If you recall, we had the following XYZ Hypothesis for Second-Day Sushi:

At least 20% of packaged-sushi eaters will try Second-Day Sushi if it's half the price of regular packaged sushi.

And from that, we hypozoomed to our first xyz hypothesis:

At least 20% of students buying packaged sushi at Coupa Café today at lunch will choose Second-Day Sushi if it's half the price of regular packaged sushi.

To test the first xyz hypothesis, we came up with a Relabel prototype: stick a label that says “Second-Day Sushi: 1/2 Off” on half of the boxes on display and count how many people choose to buy them. Let’s assume that 100 boxes of sushi are on display and that we label half of them (50 boxes) as Second-Day Sushi. The key piece of data we need to collect is the percentage of Second-Day Sushi boxes sold compared to the total number of sushi boxes sold. In other words, how many people who wanted sushi for lunch chose to buy Second-Day Sushi?

Let’s assume that during lunch, students bought a total of 40 boxes of prepackaged sushi. How many of those boxes were the relabeled Second-Day Sushi? Here are a few possible scenarios:

Outcome	Number of Boxes Sold out of 40	% of Total Boxes
A	0	0
B	2	5
C	6	15
D	8	20
E	16	40
F*	2	5
G**	30	75

* On the day of the experiment, there was an article in *The Stanford Daily* about the risks of eating raw fish.

** The lunch crowd included a group of 130 Japanese students visiting the campus.

Here’s how these results map on the TRI Meter:

Outcome A (0% of all boxes sold): Ask yourself, “If Second-Day Sushi is The Right It, how likely is it that we would sell 0 boxes out of forty?” Given that your first xyz hypothesis predicts 8 boxes and you sold 0, this is an easy call. The arrow from that result should point to Very Unlikely.

Outcome B (5% of all boxes sold): This result is not as discouraging as the previous one; after all, two people bought into the idea of slightly stale sushi, but it's well below the predictions of 20% from our hypothesis. Unless we are ready to dramatically revise our business model and expectations (e.g., target a few daring souls who are *really* short on cash and hungry for sushi), outcome B should also point to Very Unlikely.

Outcome C (15% of all boxes sold): The data from this experiment provides evidence that some sort of viable market might exist, but that that market is not as big as we need it to be to make the business successful based on our existing hypothesis. For now, and unless we decide to adjust our business model and expectations accordingly, these data point to Unlikely.

Outcome D (20% of all boxes sold): This is at the bottom range of our hypothesized market, but it fully meets the minimum requirements for confirming the hypothesis. Yay! It deserves a Likely rating.

Outcome E (40% of all boxes sold): Whoa! This result blows our prediction out of the water. If we ask, "If Second-Day Sushi is The Right It, how likely is it that we would sell 16 boxes out of forty?" we can confidently answer Very Likely.

Outcome F (5% of all boxes sold): This is a discouraging result, but the data is questionable because, by an unlucky coincidence, the school's daily paper had a front-page article on the risks of eating raw fish. Because of that, we either point the arrow to 50/50 (inconclusive) or discard it.

Outcome G (75% of all boxes sold): This is a fantastic result, but we have to be objective, so we cannot ignore the fact that on the day of the experiment an unusually high number of high-school students on a college tour from Japan visited the café. Perhaps these young students did not fully understand the implication of the Second-Day Sushi name, or perhaps they did not have a lot of money for lunch. Either way, since this was not a normal situation, we should probably dismiss this particular result. As much as we'd like to believe that our idea is the greatest ever, we have to be careful not to fool ourselves.

How Much Data Do You Need?

After you understand the TRI Meter scale and know how to map data

we collect into the likelihood of success, you need to answer an important question: How much data is enough data? First of all, let me be clear and tell you that a single experiment will not do, no matter how decisive or conclusive you think the results from it are.

Think about it this way. If, using the above example, our first experiment results in outcome A (0%), do we then give up on the idea? Or if it results in outcome E (40%)—twice as good as we expected—do we drop everything and dedicate ourselves full-time to Second-Day Sushi? Before you answer, consider a couple of examples of other types of important decisions:

Would you propose marriage or accept a marriage proposal after a single date? Hopefully not, even if that one date was *the perfect date*. Those first few hours together might be a promising indication that you may have found your Mr. Right or Ms. Right. But, just like The Right It, the Right Him or the Right Her are the exception and not the rule, so the smart thing is to confirm that initial result with more dates.

If you were interviewing a potential candidate for a job in your organization, would you ask just one question and make a final hiring decision based on that one answer?

“How many Ping-Pong balls can you fit in a school bus?”

“Ahem . . . I am not sure . . . 100,000?”

“Wrong! Not even close. You are clearly not Pong Industries, Inc., material. Thank you for coming and good luck with your job search. The door is that way.”

A single prototyping experiment is not enough to reliably determine whether or not our idea is likely to succeed—even if the results from that one experiment are clear and compelling. Why? Because many possible factors can skew an experiment. We can discount or discard some results if we are aware of factors that may have tainted them (e.g., the scary news article about sushi safety or the spike in Japanese visitors in our Second-Day Sushi example), but we can’t possibly know or account for all possible ways that our data may be skewed or corrupted.

A total novice at archery may hit the bull’s-eye on the first-ever shot, while an experienced archer may miss badly once in a while. That’s why you need several arrows on the sample TRI Meter.

In order to have confidence in your results, you need to run multiple pretotyping experiments and validate multiple xyz hypotheses. How many experiments do you need to run? That's like asking how many dates you should go on with someone before you propose or agree to marry that person, or how many interview questions you should ask a candidate for an important job before extending an offer. The answer to such questions depends on a number of factors (how well those dates went, how critical the position you are trying to fill is, etc.), but the number of dates (or questions) should be more than one or two—wouldn't you agree?

Similarly, when it comes to pretotyping, the answer depends on a number of factors, such as:

How much are you planning to invest in the idea?

How much time or money can you afford to lose if it doesn't work out?

How much certainty do you need before making a decision?

Are the results from the experiments you've run so far conclusive or inconclusive?

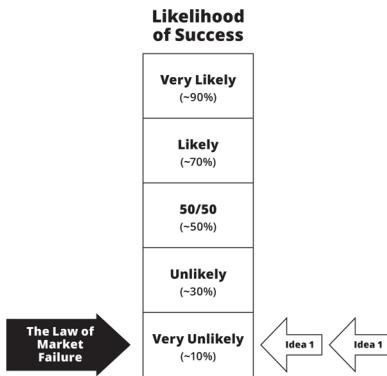
As a rule of thumb, I'd say you need to design and run a *bare minimum* of three to five experiments—and several more if executing the idea involves a significant risk (e.g., quitting your job or “betting the company”) or a major investment. The number of experiments should be commensurate with the investment and the consequences of failure—the amount of your skin in the game.

Interpreting the TRI Meter

Now that you know how to map YODA from individual experiments on the TRI Meter, I will show you how to interpret the overall results and decide on the next steps. To help me do that, I will use a sample scenario that follows a typical journey from an initial version of an idea to The Right It. Put on your boxing gloves and mouth guard, because we are about to enter the ring and go for a few rounds with the Beast of Failure.

Round 1: Punched in the Face

Let's begin with the most common scenario. Unless you get lucky, after a couple of experiments the TRI Meter for the first iteration of your idea (Idea 1) will look something like this:



If you are new at this, those first punches are going to surprise, hurt, and disorient you. But don't let this kind of result demoralize or discourage you.

First of all, welcome to the club! What club? The very crowded club of people who thought that their idea was for sure—no doubt about it—The Right It, only to have their hopes and expectations mercilessly dashed by the Beast of Failure.

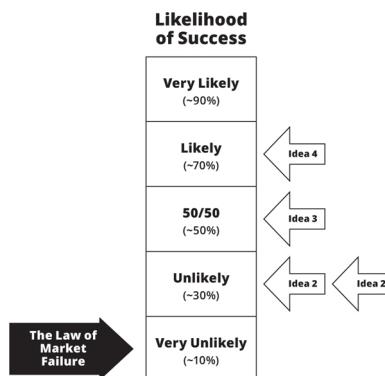
Second, think how much worse off you'd be if you had gone ahead with that idea without testing it. After investing months of work and lots of money to develop and market your product, you find out that your idea was The Wrong It all along—a knockout punch that sends you to the hospital. Fortunately, our thinking, prototyping, and analysis tools can help you avoid that. A little pain now can save you tons of pain later. By learning quickly and cheaply that a particular idea is not likely to be successful, you will have plenty of time and resources left to modify your original idea or explore a new set of ideas—to go a few more rounds.

Based on this TRI Meter, we should concede that our beloved new product idea is most likely headed for failure in the market. Round 1 goes to the Beast of Failure. If you are really passionate about your new product, you may decide to get back into the ring and run a few more experiments with the same exact idea—just to be sure. But a more logical and less painful course of action would be to go back to the drawing board (or back to your corner, if you like the boxing metaphor) and use what you've learned from your experiment to tweak your idea.

Rounds 2–4: We Take Some, We Give Some

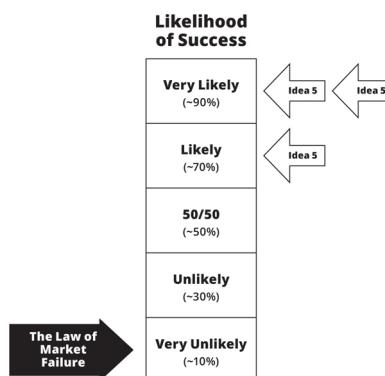
We make some tweaks to our original idea (Idea 1) and run some tests with each of the variations (Ideas 2, 3, and 4). When we map the results on the TRI Meter we get the “Likelihood of Success” shown below.

We still get punched quite a bit—especially with Idea 2—but not as hard as before. Our tweaked versions of the idea manage to stay out of the Very Unlikely zone, and we even manage to land a punch with the fourth version of our idea (Idea 4). That’s a very good sign—we are learning more about the market, tweaking accordingly, and moving closer to The Right It territory.



Round 5: We Land a Few Good Punches

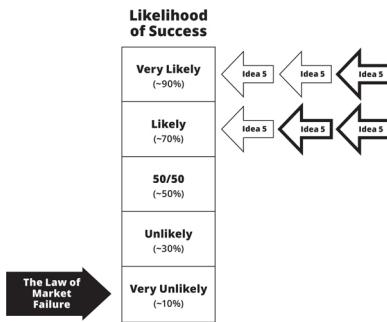
Using Idea 4 (the one that scored a Likely) as the starting point, we make a couple of additional tweaks and go back into the ring with Idea 5.



The arrows from our three experiments with the fifth version of our idea all point to Likely or Very Likely. This is great! Assuming that the experiments that produced those results were properly designed and run and that the data from each was interpreted fairly and objectively, there's strong evidence that this idea might be The Right It. But that ominous black arrow at the bottom does its job and reminds us how rare it is for a new idea to succeed in the market. Are those three positive results enough to balance and counteract the Law of Market Failure?

Developing this particular idea will require a major investment and commitment, and we want a higher degree of confidence before going ahead. So we decide to run three more experiments using Idea 5.

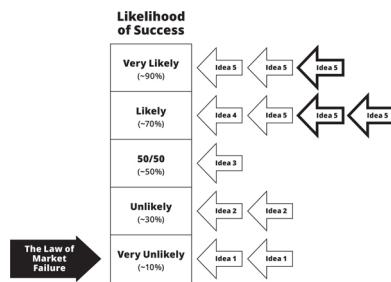
We map the new results on the TRI Meter alongside the first set of results (second set of results shown in **bold**), and we get the following:



All right! The new set of experiments on Idea 5 confirms the first set of results. This is great. We can't completely ignore the big black arrow—the market may still surprise us with an unexpected punch—but there's a good chance that the fifth iteration of our idea is The Right It.

To help you visualize the process, here's what our sequence of tweaks and experiments looks like if we chart them all (from Idea 1 to Idea 5) on a single TRI Meter:

We ran a total of twelve prototyping experiments on five different ideas (or versions of a similar idea). That may sound like a lot of tweaking and experimenting, but with prototyping this would not have taken more than a couple of weeks—less time than what most teams would spend to write an OPD-based business plan.



As we wrap up our discussion of the TRI Meter, let me repeat that only arrows that represent actual data from carefully designed and personally conducted experiments are allowed. No opinions and no OPD (market research done by other people, with other methods, at other times—you know the drill). Your arrows must consist of only YODA with skin in the game.

Part III

Plastic Tactics

7 Tactics Toolkit

In Part II, I introduced you to a set of tools to add clarity to the way you *think* about your idea, accelerate the way you collect data to *validate* your idea, and add structure and objectivity to the way you *analyze and interpret* the data you collect. It's a powerful toolkit, with many tools to choose from, different ways to use them, and countless ways to combine them. But how do you decide which tools to use, how to use them, and when to use them? That's what we will cover in Part III: Plastic Tactics.

In case you are wondering, the word *plastic* in the title for this section is not a reference to Tupperware, but to *plasticity*—the ability to change and adapt one's plans and actions in response to new and unexpected situations. Having this ability is critical, because when it comes to bringing new ideas into contact with their intended market, plans rarely go smoothly—regardless of how diligent and careful we are in making those plans.

Sticking with our boxing analogy from Part II, the best quote I've heard on the subject of planning comes from an unlikely source, boxer Mike Tyson. Asked to comment about one of his opponent's plans for a fight, Tyson replied, "Everybody has a plan until I punch them in the mouth." Expect the market to punch you in the mouth a few times and be prepared to change your plans and tactics accordingly.

I cannot give you a one-size-fits-all plan or a step-by-step set of instructions—the kind you get when you buy IKEA furniture. But I will share with you some of my favorite and most effective tactics to help you take full advantage of our tools, so you can avoid some market punches and perhaps punch back a few times. I will follow this chapter with one final example that illustrates all our tools and tactics in action.

Ready? Let's go.

Tactic 1: Think Globally, Test Locally

If the phrase "Think globally, test locally" has a familiar ring to it, it's because it was inspired by the popular "Think globally, act locally" slogan

(and bumper sticker) commonly associated with ecologically minded people and organizations. In our case, the objective is not to save the whales or protect the ozone layer, but to save our time and protect our valuable resources by making early contact with the local market instead of wasting time in Thoughtland making premature grandiose plans for worldwide sales and distribution of our product.

“Think globally, test locally” means that you can have global plans for your product, but before you spend any time developing and executing those ambitious international plans, you should validate your idea on a much smaller and easier-to-reach subset of your target market. Make that initial market your town, your neighborhood, your workplace, or your school. The closer and more easily accessible it is, the better.

Go ahead and spend a few minutes daydreaming that your unique idea for a pizza restaurant will become the next California Pizza Kitchen—a chain with hundreds of locations in several countries. But don’t waste time making plans for global-market conquest until you have achieved consistent success with one location.

The words *globally* and *locally* suggest geography, but the “test locally” principle applies to more than just physical distance or geographic regions. It applies to any market with multiple conceptual groupings or industry standards, each of which requires additional investment to reach and address. If your market is smartphone users, for example, where those users physically live is less important than what smartphone platform they use (e.g., Apple iOS, Google Android). Many mobile app developers double or triple their up-front investment and waste months of engineering and marketing time to develop and launch their new app on multiple mobile platforms at the same time—only to learn that Google Android users are just as uninterested in their app as Apple iOS users. Validate your idea on one platform at first—the one that is technically or conceptually closest to you—before you set out to conquer the rest.

If you are a mobile app developer and you are most comfortable and experienced with the Google Android operating system, then that’s your local “neighborhood.” Android users thousands of miles away are easier for you to reach than iOS users living next door. So begin by pretotyping and testing your new app idea on the Android market. If your pretotyping

experiments indicate that the app is likely to be successful with those users, then you can start thinking about developing versions for other platforms.

Early in my career, blinded by dreams of global success, I consistently ignored the “test locally” tactic and ended up paying a hefty price for it. In one of the companies I cofounded, for example, we hired sales teams to cover Europe and Asia Pacific before we had achieved consistent and repeatable sales success and user adoption in the US. To support each new country, we had to translate, customize, and test multiple versions of our product and documentation for that country. All of this cost us a lot of time and money and was a huge distraction. I can’t blame the company’s eventual failure on that one decision, but it certainly did not help.

But what if there is no local market for your idea? What if you live, say, in Montana, and your idea is solar-powered surfboards? In that case, I suggest that you move to southern California or Hawaii, look for a new idea, or come up with creative ways to bridge that distance (e.g., partner with someone who has ready access to surfers).

“Test locally” is one of my favorite tactics, because it’s the best way to quickly get yourself and your idea out of Thoughtland and into contact with the market. With this tactic, we take hypozooming and go a step further. We don’t just zoom into a small test market, we zoom into a small *and local* test market.

Remember how in the Relabel prototype example for Second-Day Sushi we zoomed in all the way to within a few hundred feet of our current location? Not just California, not just Palo Alto, not even just Stanford University, but to the very same building where the students were at the time. By doing that, we were able to come up with a prototyping experiment that we could execute easily and right away.

Say It with Numbers: Distance to Data

You can—and should—measure how well you are following the “think globally, test locally” tactic by calculating your *Distance to Data* (or DTD). If you plan to collect your data in the physical world (e.g., at a store, on a street corner, or in a club meeting), you can measure DTD using your favorite unit for distance (e.g., meters, miles, furlongs) and then try to minimize it. By keeping initial market-validation efforts as local as possible, you save valuable time and money, which allows you to run more

experiments or test more ideas. You'll be surprised how much good YODA is often available right in your neighborhood or other nearby areas. Let me give you an example.

Linda has an idea for a \$19 device to make those visits to the coin laundry more efficient—and less depressing. She calls her gizmo LaundroDone. Toss LaundroDone into the washing machine or dryer along with your clothes, and when the machine completes the cycle and stops spinning, the device sends a text to your phone to let you know that your clothes are ready. Thanks to LaundroDone, Linda explains, she doesn't have to wait around at the coin laundry, sitting on plastic chairs and fending off romantic advances by pajama-wearing Romeos down to their last pair of (hopefully) clean underwear. She can escape the depressing glare of those fluorescent lights and the nauseating potpourri of odors from detergent and fabric softeners. Thanks to the LaundroDone, she can wait in the safety and comfort of her car, listening to Coldplay, until she gets the alert that her clothes are ready.

Linda comes up with a brilliant way to test her idea using a combination Mechanical Turk and Facade prototypes, and she is eager to put it into action and collect her first YODA. She believes that the best market for her idea is large coin laundries in major metropolitan areas (big cities like New York or Los Angeles), and that's where she wants to run her tests.

But Linda lives in a tiny residential community in rural southern California. There's only one coin laundry in town that, in Linda's own words, "gives me the creeps and is a magnet for weirdos." Even though the creepy coin laundry is what motivated Linda to invent the LaundroDone, she does not want to set foot back in there if she can avoid it. Instead, Linda plans to drive 120 miles to Los Angeles, book a hotel room, and spend a couple of days running her tests in several La-La Land coin laundries. Nothing wrong with this plan, but does she really have to drive that far to collect some of her first YODA?

By applying the "test locally" tactic and using the Distance to Data metric as a guide, Linda identifies a midsized town with several not too creepy coin laundries that is less than 20 miles away. It's not as local as her hometown, but at least it's in the same county. By staying relatively close to home, she saves herself a 200-mile round-trip and hotel costs—

time and money that she can use to run more tests. Makes sense, doesn't it?

What happens, though, when your product idea is meant to be marketed, acquired, or used online? In that case you can replace units of physical distance with virtual ones such as emails, web posts, or web pages. Instead of counting physical steps, you count digital steps. It's easier than you think. Let me illustrate with another example.

A few years ago I came up with an idea for an audio tone-control device to make poorly recorded music sound richer and less harsh. I identified the target market as audiophiles—people who spend a lot of money on audio equipment in search of sonic nirvana. These days, most audiophiles make their purchases online because brick-and-mortar audio stores are becoming as rare as bookstores. So my plan was to market and sell my audio device online, and I designed my prototypes and tests accordingly.

I wanted to follow the “test locally” tactic. But what does “local” mean online? In my case, I identified my virtual neighborhood as the internet audio forum that I visited regularly and to which I actively contributed with my own posts and audio product reviews. I was one of the first members of the forum, and I was on good terms with the forum’s founder, so I figured that if I asked him nicely, he would let me write a post introducing my device to see if any forum members were interested in buying it. In this case, my DTD was three digital steps:

1. Writing one email to the forum moderator
2. Writing one post to introduce my product
3. Creating a basic website with one landing page to collect some skin in the game (emails, deposits, etc.) from potential customers

Because I was already a member of the forum and on friendly terms with its moderator, I was able to keep things in my “online neighborhood” and get my prototype up and running very quickly.

Keep “think globally, test locally” in mind when you craft your first xyz hypotheses. Don’t just zoom in on a specific market; zoom in to where you are *now*. And speaking of now . . .

Tactic 2: Testing Now Beats Testing Later

“Testing now beats testing later” does not require much explanation. The message is clear: don’t delay testing. Take your idea—and your butt—out of Thoughtland and into the market as soon as possible. After you’ve identified your initial Market Engagement Hypothesis, expressed it in XYZ Hypothesis format, hypozoomed into xyz hypotheses, and designed a prototyping experiment, it’s time to move from abstract thinking to concrete testing.

But most of us are reluctant to leave the comfort of Thoughtland. We dwell in it for months, sometimes years, thinking and talking about our idea, writing and revising our multiyear international business plan—even though we don’t have a single piece of YODA that validates our idea. Why do we do that?

I’ve been stuck in Thoughtland myself many times, so I feel qualified to posit an answer: fear! More precisely fear of rejection, the fear of finding out that there is no market interest for your beloved idea. Most people don’t like to admit having this fear, but their actions indicate otherwise. I am no Sigmund Freud, but lingering in Thoughtland and postponing market contact might be a way to subconsciously avoid that potentially painful first contact with the market.

I’ve often compared the fear of the pain, humiliation, and disappointment that can come from market rejection to the fear of romantic rejection. I get it, rejections—romantic or otherwise—are very unpleasant and psychologically difficult. But faint heart never won fair lady—or market share.

There’s no escaping the Law of Market Failure. Most of your ideas will be rejected by the market—and it will hurt. But the tools and tactics in this book will make this unpleasant yet unavoidable part of the process less painful, quicker, and easier to take. So don’t delay. If your idea is going to be rejected, better to find out now rather than later.

Over the years, I’ve worked with hundreds of teams on thousands of new product ideas, and I’ve noticed the following patterns:

Teams that spent too much time in Thoughtland working with opinions and OPD and spent months writing business plans usually failed.

Teams that rushed their product to market with minimal planning and testing usually failed.

Teams that rushed to *test* the market usually succeeded.

In other words, you don't want to spend too much time in Thoughtland, but you also don't want to rush a finished product to market. Instead, take that eagerness to launch your product into the market, and use it to first *test* the market.

Say It with Numbers: Hours to Data

Hours to Data (or HTD) is a measure of how many hours it will take you to execute a prototyping experiment and collect some high-quality YODA. The HTD for our initial Second-Day Sushi prototyping experiment, for example, was just two hours—that's all it took to print some labels and slap them on the boxes on display (and it helped that it was close to lunchtime).

All things being equal, the shorter the HTD, the better. When I assign a prototyping experiment to students, I typically set an HTD limit of forty-eight hours—and offer bonus points if they can prove that they got their YODA even more quickly. In one of my classes, while I was in the middle of explaining a prototyping assignment, one of the students raised her hand, waved a \$5 bill, and yelled, “Three minutes to data! How’s that?”

Before I had a chance to reply, she explained, “Our team’s idea is a bicycle-cleaning and tune-up service. For \$5 we will thoroughly clean your bike. For \$10, we will also check the brakes, the shifters, lube the chain, and inflate the tires.” Murmurs of approval from other students in the class. “So,” she continued, “I sent an email message with the offer to our class and—”

At that point a student sitting behind her stood up and said, “I saw the message and gave her the \$5 just now. I want to be first in line, because with all this rain my bike is filthy and muddy.”

I couldn’t help smiling. I started to clap, and the rest of the class soon joined in. It was just one data point, but this student definitely understood the basic principle and spirit of Hours to Data. As you can imagine, by coming in with an HTD of 0.05 (three minutes is 0.05 hour) she recalibrated the expectations of everyone in the class (including me) and set a new standard. The competition was on.

Note: Originally, I used a different name for this metric. I called it Time to Data. But I eventually changed it to Hours to Data in order to recalibrate expectations and increase the sense of urgency. It worked incredibly well. In both my classes and projects with corporate clients, the time it took teams to get their first YODA shrunk from a few days to a few hours.

This name change, by the way, is an example of what psychologists call *priming* (or *anchoring*). By using hours as the basic unit of measure, I primed people to think in terms of hours and not days or weeks—and that's exactly what they did.

Tactic 3: Think Cheap, Cheaper, Cheapest

If you follow the techniques in this book, you will be able to collect high-quality YODA not only faster, but also much more cheaply than you could with other market-research approaches, possibly 10, 100, or even 1,000 times cheaper. Many of the companies I work with are accustomed to allocating months and hundreds of thousands of dollars for market research, so when they come up with a prototyping experiment budget in “only” the tens of thousands, they are thrilled—but I am not. I tell them that going from hundreds of thousands to tens of thousands is good progress, but that they can probably get the same YODA for mere thousands or even just hundreds.

Most ideas for new products can be properly tested with very little money—some with virtually no money. I like it best when “pizza for the team lunch” is the most expensive item in the prototyping budget.

I urge you not to settle on the first or second prototyping experiment idea you come up with. Ask yourself, “Is this the best that we can do?” More often than not, you will come up with a cheaper way to test your idea that does not sacrifice YODA quality. Then try to do even better than that. The “think cheap, cheaper, cheapest” tactic was inspired by the following story.

Henry Kissinger was a demanding boss. When he was Nixon’s national security advisor, Kissinger asked one of his staff members to write a position paper for him. The staffer spent several days working on the doc-

ument. When he thought that it was good enough, he presented it to his boss, who thanked him and said that he would read it later that evening.

The next day Kissinger called the staffer in, handed the paper back, and asked him, “Is this the *best* that you can do?”

Surprised and a bit embarrassed, the staffer said that he could probably do better. He canceled all of his other plans and spent several days focused on rewriting the paper.

Kissinger’s response to the revised version was the same: “Is *this* the best that you can do?”

The staffer was stunned and felt humiliated, but he asked for another chance and swore that he would do much better this time. A few days later, he delivered the third version of the paper to his boss.

Before the staffer left, Kissinger asked him, “Is this *really* the best that you can do?”

“Yes, sir. It is really the best I can do,” the staffer answered.

“Good. Now I will read it,” replied Kissinger.

Since I was not present, I cannot vouch for the veracity or accuracy of this story, but I like Kissinger’s approach, because it’s rare that our first solution is the best, most effective, or most efficient one.

In Google’s early days, when the fledgling company’s resources were stretched to the limit, many requests for an increased budget were often turned down with the phrase “Creativity loves constraints.” And guess what? Most of the time people found a way to make the available budget work.

If you stir up your creative juices, you will usually discover a way to test your idea that is cheaper than your initial one. If you originally allocated a budget of \$1,000 for your experiments, challenge yourself to find a way to reduce it to \$100. And if you succeed at that, see if you can push it down to \$10 or, even better, zero.

Say It with Numbers: Dollars to Data

Dollars to Data (\$TD) does not require much explanation, and feel free to replace dollars with the appropriate currency for the project: dollars, euros, yuan, bitcoin, donuts . . . Hey, wait a minute. Donuts? Yes, donuts. This metric does not have to be based on a conventional currency. If your project is staffed by volunteers and you reward them with break-

fast donuts, then Donuts to Data is a reasonable metric to use.

Tactic 4: Tweak It and Flip It Before You Quit It

Don't let disappointing YODA from your initial set of experiments discourage you prematurely. The Right It might be just a few tweaks away—and prototyping can help you find those tweaks. Let me explain.

Most new product ideas, even the craziest-sounding ones, are usually based on not-so-crazy premises; the people behind those ideas are rarely lunatics (although I have met a few who would qualify). More often than not, they are people who have a decent understanding of the industry, believe that their idea solves a real problem for real customers, and are convinced that there is a bona fide market opportunity to take advantage of—and they are usually right about that. The problem is that their original idea is most likely in the close-but-no-cigar zone of that market opportunity. Let me illustrate, literally, what I mean.

In the following image, the light gray area represents The Right It zone for a specific market opportunity, while the dark gray area represents The Wrong It zone.



The market opportunity is there, and it's real. But not all products that address it will be successful: they may be too expensive, too big, too complicated, come in the wrong color, have the wrong name, and so forth. The market can be very, very fussy and fastidious. If you don't come up with a product combination it likes (i.e., The Right It), it will reject your idea—even if, in many other respects, your product does a good job of addressing the problem or opportunity. If you are really committed, or in-

terested, or passionate about a specific market problem or opportunity, stick with that market, but tweak and experiment with variations on the original idea.

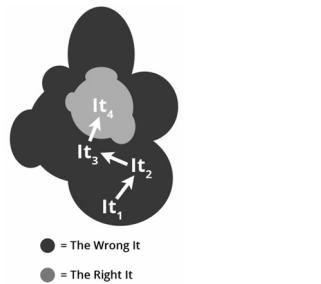
Let's assume that you've run several prototyping experiments with your initial idea (represented as It_1 in the next image), and the data shows conclusively that your idea—as it currently stands—is The Wrong It.



You are disappointed—and that's understandable. But in the process of testing that idea, you have discovered interesting facts about your target market. Perhaps you learned that one of your key Thoughtland-based assumptions was dead wrong (e.g., most people *don't* think that \$8 is too expensive for packaged sushi). Or you observed that 80% of packaged-sushi shoppers carefully inspect the label to check the “packaged on” time stamp—and put the box back if that time indicates that the box is more than a day old. With each experiment, you gained valuable YODA you can use to inform and guide your next step.

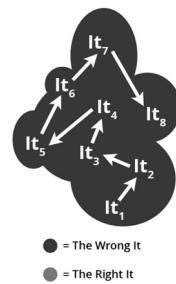
Even if the original idea for Second-Day Sushi proves to be The Wrong It, it is quite plausible that somewhere in the Thoughtland neighborhood from which Second-Day Sushi originated dwells an idea and business model for budget packaged sushi that is The Right It. Try testing a weekly subscription service and tweak the name and slogan to suggest convenience rather than a lack of freshness: “Sushi2You: The convenient and affordable sushi subscription service.”

And if the market does not respond to that particular tweak, explore and prototype a few more tweaks (e.g., “Groupie Sushi: Order sushi as a group and save”) until you find a combination that is The Right It.



But what if, even after a bunch of tweaks, you still haven't found The Right It?

At that point you should consider the very real possibility that, as the image below shows, The Right It for the budget-sushi idea does not exist; all variations on that idea are destined to fail, because too many people associate cheap sushi with bad sushi—and bad sushi with all sorts of nasty consequences.



What now? Well, if your heart is set on being in the packaged-sushi business one way or another, it's time to turn up your creativity by several notches and explore another set of variations on the packaged-sushi theme.

I used to think of myself as somewhat of an expert in creativity, a master of brainstorming, until I met a real expert on the subject: Stanford professor Tina Seelig.* In 2016 I had the honor of teaching a graduate

* I highly recommend Tina's book *inGenius: A Crash Course on Creativity* (San Francisco: HarperOne, 2012)—the next best thing to taking one of her classes.

course titled “Creativity and Innovation” alongside Tina. The objective of the course was to teach students how to develop innovative solutions to problems by combining creativity techniques (in order to come up with a large initial number of ideas) with prototyping techniques (in order to test, validate, and whittle the initial set of ideas down to the ones most likely to succeed). I focused on teaching prototyping, and when Tina was teaching creativity techniques, I sat alongside the students, paying rapt attention and furiously taking notes.

In one of the class exercises, students were assigned a challenge (e.g., stop people from texting while driving) and had to come up with at least one hundred innovative ideas that could help solve the problem. As two-time Nobel Prize-winner Linus Pauling famously said, “The best way to have a great idea is to have a lot of ideas.” But coming up with a lot of ideas to solve a single problem sounds easier than it actually is—if you don’t know the right techniques. Most people struggle to get past forty or fifty ideas in this exercise and conclude that they are simply not creative enough.

Tina would strongly disagree with that conclusion; she believes that creativity is a skill that we can all learn and proves her point by teaching a set of techniques that makes the “one hundred ideas” exercise one hundred times easier. In one of the class’s most memorable and effective lessons, using a Harvard case study about Cirque du Soleil as an example, Tina showed the students how to take their preconceived notions and assumptions about a specific idea and flip them over to reveal alternatives that might lead to a successful variation on an existing idea. In an article she posted on Medium.com, Tina describes the exercise as follows:

One of my favorite exercises involves unpacking all the assumptions you have and then turning them upside down to reveal alternatives.

In my creativity course, I use a Harvard case study about Cirque du Soleil that gives students a chance to hone their skills at challenging assumptions. The backdrop is the 1980s, when the circus industry was in trouble. Performances were predictable and stale, the number of customers was diminishing, and animal treatment was under attack. It didn’t seem like a good time to start a new circus. But that is exactly what Guy Laliberté, a street performer in Canada, did by challenging every assumption about

what a circus could be.

After showing a video clip from the 1939 Marx Brothers movie, *At the Circus*, I ask the students to list all the assumptions we have for a traditional circus: a big tent, animals, cheap tickets, barkers selling souvenirs, several acts performing at once, clowns, popcorn, strong men, flaming hoops, etc.

I then ask them to turn these things upside down—to imagine the exact opposite of each one.

For example, the new list would include a small tent, no animals, expensive seats, no barkers, one act performing at a time, and no clowns or popcorn. They then pick the things they want to keep from the traditional circus and the things they want to change. The result is a brand-new type of circus, à la Cirque du Soleil. And we all know that Cirque du Soleil is thriving, while the traditional circus is essentially gone.

Once we do this exercise with the circus industry, it's easy to apply to other industries and institutions that are ripe for change, including fast-food restaurants, hotels, airlines, education, and even courtship and marriage. When you get the hang of it, this is an easy, back-of-the-envelope exercise you can use to reevaluate all aspects of your life and career. The key is to take the time to clearly identify every assumption. This is usually the hardest part, since assumptions are often so integrated into our view of the world that it's hard to see them. However, with a little practice, it becomes a useful way to look at your options in a fresh light.*

Let's apply this creativity technique to packaged sushi. Since we've already explored going the cheaper route with Second-Day Sushi, let's have some fun and go the other way. Let's flip our fishy idea on its head and go upscale: "Superior Sushi: the freshest and highest-quality packaged sushi you can buy." Who knows? We might learn that although there's not much of a market for cheaper, less-fresh packaged sushi, there is a strong market for extra fresh sushi that comes in a premium package. All we need to do is make a list of what people typically assume about or associate with packaged sushi and then come up with alternatives suitable for our Superior Sushi idea. For example:

* Tina Seelig, "What Does Your Life Look Like Upside Down?" Medium, August 3, 2017, <https://medium.com/@tseelig/what-does-your-life-look-like-upside-down-66a5048df461>.

Packaged Sushi	Superior Sushi
\$7–10 price	\$14–20 price
Up to 3 days old	Guaranteed fresh
Cheap plastic box	Fancy bamboo box
Fake wasabi	Real wasabi
Cheap soy sauce packet	Premium soy sauce in a cute little glass bottle

Yum, I am getting hungry just thinking about it. I must admit that I like the Superior Sushi idea a whole lot better than Second-Day Sushi. I can even imagine myself spending an extra few dollars to get fresher fish, real wasabi (not that artificially colored green stuff that comes in tubes), a bamboo box that I can save and use for something else, and a cute little glass bottle of premium soy sauce instead of those plastic soy packets. But that's just me in Thoughtland. How many other people would actually spend close to \$20 for packaged sushi—regardless of how fancy the packaging is? I am sure that by now you know what you need to do in order to answer that question.

By the way, you don't necessarily have to wait for a new product idea to turn out to be The Wrong It before you start tweaking it and exploring other ideas. Our tools and techniques give you the power to test any idea quickly and efficiently; that means that now you are in a position to test several different ideas (or different tweaks on a basic idea) before committing to a specific one. You don't have to come up with one hundred different versions of an idea (although, why not?), but you should think of at least a few different ones, because chances are your first idea may not be the best idea.

Tweaks Beat Pivots

Most people understand that it's unlikely that the first version of their idea is The Right It. They know that few ideas come out of Thoughtland fully baked and ready to succeed. They assume that they will have to do some tweaking. Unfortunately, they do their tweaking in Thoughtland, using opinions and OPD instead of YODA to guide them. No wonder most ideas flop miserably when they hit the market.

The only way to discover what the market *really* wants and tweak accordingly is to make *real* contact with it. Don't just *ask* the market what it *thinks* it wants; put a prototype of your idea out in the market and *demand* some skin in the game as evidence of interest. The sooner you do that, the better, because the longer you wait, the more painful and expensive the lessons get. And eventually you will find yourself too beaten up, too discouraged, and with no resources left to try other tweaks.

These days, the term *pivot* is tossed around a lot by entrepreneurs, product managers, and venture capitalists. A pivot is generally understood to be a *major* change in the fundamental idea or market hypothesis for a new product or business. You are forced to pivot because—surprise, surprise—the original idea turned out to be The Wrong It. The problem with most pivots is that, unlike tweaks, they happen *after* the team has already invested significant time and money in developing the original idea. I participated in several pivots earlier in my career, but we didn't use that term at the time. We just said, "We screwed up!" When the word *pivot* enters the conversation in a product meeting, it's inevitably accompanied by that most pungent of aromas: *eau de desperation*. By then, the team will have usually wasted most of their resources on The Wrong It, severely limiting their options. It does not have to happen this way.

If you prototype your idea early on and make your tweaks along the way, you will avoid traumatic pivots and dramatically increase your chances of eventually landing The Right It. Ten tiny tweaks are better than one painful pivot.

8 Complete Example: BusU

We are now ready to see how the various tools and tactics we've learned fit together. I'll pick an idea for a new business, and we'll work through the entire process from Thoughtland, to data, to decision. I will not only show you the steps I'd take, but also give you insights into my thought process as the plan unfolds. Keep in mind that this is only one of a myriad of possible ways we can apply these techniques. There is no single best approach. If you think you would do things differently or better, that's great; it means I've taught you well.

The new business idea we'll use for this example popped into my head while crawling along in commute traffic on Highway 101 on my way from my home in Silicon Valley to downtown San Francisco for a morning meeting. A few years ago, this drive would have taken about forty-five minutes. Today, with the business and building boom all over the Bay Area, it can take up to two hours to cover less than 40 miles, and thousands of people make this three- to four-hour round-trip commute every single day. What a waste—and what an opportunity!

That morning, while staring at a river of red brake lights, I came up with the idea for BusU: turn commuter buses into classrooms and commute time into learning time. While inching toward my destination, I even had time to come up with a slogan: “Take a class on the bus.”

I don't believe, of course, that I was the first to have had this idea. Others may have already thought of such a business. Some may have even tried to build it, and we may learn something from their attempts. But by now you should know how I feel about using other people's success or failure with similar ideas. We might take OPD into account, but that's no substitute for running our own experiments and collecting our YODA.

Below is a rough outline of the key steps we'll take to collect that YODA and use it to guide our decisions. As you can see, we will be using all three sets of tools (thinking tools, prototyping tools, analysis tools):

- Describe the original BusU idea.

- Identify BusU’s Market Engagement Hypothesis (MEH).
- Write BusU’s MEH in XYZ Hypothesis format.
- Hypozoom into xyz hypotheses we can test quickly.
- Identify a set of prototyping experiments we can use to validate our hypotheses.
- Tactically prioritize experiments based on Distance to Data (DTD), Hours to Data (HTD), and Dollars to Data (\$TD).
- Run the first set of experiments.
- Based on an objective analysis of the YODA from the experiments, determine the next steps.

Remember—this is just our coarse, initial plan. Like all such plans, it’s likely to change once our idea makes contact with the market and we collect our first data. After we get going, we may find ourselves backtracking or jumping around in those steps quite a bit. For example, right in the middle of running our first prototyping experiment, we may run into an unexpected obstacle or an opportunity that will dramatically affect our Market Engagement Hypothesis (e.g., California passes a law that makes it illegal to run businesses on buses). Or, as often happens, while working on it, our original idea morphs into a different and potentially much better or bigger idea. Prepare yourself to be surprised and to gain new insights, and don’t hesitate to revise any or all of your plans and hypotheses. You are entering unknown territory, so you should be flexible and adaptable. This is why Part III is called “*Plastic Tactics*.”

But although our tactics may change, our guiding principles will not. We will use our tools to think clearly, test thoroughly, and analyze objectively.

Thinking Clearly About Our Idea

The Original Idea

Here's how, while stuck in traffic, I originally envisioned the BusU idea:

By partnering with local universities, BusU will offer accredited college-level classes taught by top-rated professors while transporting professionals to and from work each day. The buses will be customized to resemble a classroom and will have a capacity of 30 to 50 students. We will begin by offering BusU service from San Francisco to Silicon Valley and back. The length of the commute is perfect for offering a standard fifty-minute class each way.

To make the numbers work, we envision BusU as high-end executive and professional training, and it will be priced accordingly. We anticipate a fee of \$3,000 for a ten-week course. This fee may seem high, but we expect that most high-tech companies that already subsidize continuing education and training for their employees will cover at least part of the tuition.

That's a pretty good high-level description of our vision for BusU. We've even managed to add some interesting details and included some numbers—all of which will make writing the XYZ Hypothesis and xyz hypotheses easier. But before we do that, we must take the next critical step: identifying the Market Engagement Hypothesis (MEH).

Market Engagement Hypothesis

As we've seen in [Part I](#), lasting business success requires that many factors and assumptions line up just right. For BusU to succeed, we must be able to find accredited universities and proven college professors willing to work with us; it must be possible—and not too expensive—to customize buses for classroom use without violating transportation safety standards; and so on. But in this case, as it is in the great majority of cases, the most important assumption we need to test and validate relates to market interest. Remember, "If there's a market, there's a way." If there's enough market interest, we will find ways to partner with schools, recruit professors, and meet transportation standards.

Since the target customers for BusU are business professionals with long commutes, our MEH should be based on this segment of the population. Will enough professionals be willing to pay private university-level tuition to take a class on a bus?

Here's our first rough pass at the MEH:

A lot of professionals with long commutes will pay university-level tuition to take buses with classes.

It's a start, but it's too vague and fuzzy a description to be of much use. What's "a lot of professionals"? How long is "a long commute"? How much is "university-level tuition"? We need to *say it with numbers* and express it in a way that can be tested by experiment and observation.

XYZ Hypothesis

Let's combine our Thoughtland-based vision (which already includes some numbers) with our MEH, so we can express the latter in XYZ Hypothesis format, at least X% of Y will Z:

At least 2% of working professionals with daily commutes of one hour or longer each way will pay \$3,000 to take an accredited ten-week class on BusU at least once a year.

Nice! We've shaved off a lot of fuzz, made our implicit assumptions explicit, and included an educated guess at some numbers that we can use in our experiments.

If we can get at least 2% of our target market to take a \$3,000 BusU class once a year, we'll have a good foundation for making BusU a viable and valuable business. But that's a big *if*. It all sounds very promising, reasonable, and plausible; remember, however, that we are still in Thoughtland at this point and, for all we know, we are be about to fall into a false-positive trap. Time for our idea to pack its bag, say good-bye to Thoughtland, and get ready to be tested in the real world. Time to hypozoom.

From XYZ to xyz

Our next step is to take our XYZ Hypothesis and turn it into three xyz hypotheses that we can test quickly and easily with prototyping. We want to minimize our investment of time and money by using what we already have available to us—and near us. Time to put the "think globally, test locally" tactic into action.

I begin by looking at my current situation and resources to see what I can leverage and put to good use:

I live in Mountain View, California, home to Google and LinkedIn. Both companies employ thousands of professionals and offer them education subsidies and tuition reimbursements.

Many Google and LinkedIn employees who work in Mountain View live in San Francisco and commute to work either by car or on company-sponsored buses.

I have many friends who work at Google and a few who work at LinkedIn.

I have a great relationship with several top-notch Stanford professors.

Bottom line: I have access to a wealth of resources I can use for my first experiments. Many of the people who work at Google and LinkedIn are engineers and, since most engineers are eager learners, they are a great subset of BusU's target market for our initial test. But should I use Google or LinkedIn (or both) for our first set of experiments? To help me decide, I can use the Distance to Data metric.

Geographically speaking, Google's and LinkedIn's offices are both less than 10 miles from my home and within a couple of miles of each other—so that's a tie. But since I know many more people at Google than I do at LinkedIn, if I measure DTD in terms of the number of emails I will have to send in order to reach the right people, then Google emerges as the better choice. I can save my LinkedIn contacts for future experiments to see if they confirm the data from my experiments with Google employees. So my initial zoomed-in target market (i.e., the y in xyz) will be Google engineers who live in San Francisco and work at the company's Mountain View campus.

Great! Now that we have an easy-to-reach target market, we can use it to hypozoom. Here are three xyz hypotheses I can test using this group:

xyz1: At least 40% of Google engineers commuting from San Francisco to Mountain View who hear about BusU will visit the BusU4Google.com website and submit their google.com email address to be informed of upcoming classes.

xyz2: At least 20% of Google engineers commuting from San Francisco to Mountain View will attend a one-hour lunchtime presentation to learn more about BusU.

xyz3: At least 10% of Google engineers commuting from San

Francisco to Mountain View will pay \$300 for a one-week “Introduction to Artificial Intelligence” class on the bus taught by a Stanford AI professor.

Before we go further, let me address a question you may have about the numbers I’ve chosen for the value of x in these xyz hypotheses. I used 2% for X in the XYZ Hypothesis, so why do I use 40%, 20%, and 10% for x in the hypozoomed xyz hypotheses?

I did that because I am taking into account what is commonly called the *conversion funnel*. Not every person who walks into a store, attends a free seminar, or visits an ecommerce website becomes a paying customer. Quite the opposite. Data consistently shows that only a small fraction of people who sign up for free trials or seminars or visit a website convert to paying customers.

Out of every hundred people you invite to learn more about your new product, you’d be lucky if you got five of them to accept or follow up on your invitation (e.g., by coming to your store or visiting your website for more information). And out of those five or so, perhaps only one or two of them will end up making a purchase or some other form of commitment. Perhaps none of them will.

The other reason those three xyz hypotheses have different values for x is that each hypothesis involves a different amount of skin in the game:

xyz1: 1 point for a valid email address

xyz2: 60 points for an hour of time to attend a presentation

xyz3: 900 points for committing to pay \$300 and attend a one-week class (300 points for the \$300; 600 points for spending *at least* ten hours attending a class on a bus)

As the amount of skin in the game increases, you must anticipate that the number of people who sign up will decrease. Remember also that all of these numbers are, for now, just educated guesses—a starting point. Our experiments will tell us whether or not these figures are even in the ballpark; if they are not, we will revise our working hypothesis and plan the next steps accordingly.

Even after this explanation, you may still have some issues with how accurate or valid you think these hypotheses are. You might have even

thought of much better hypotheses we could have used. Or perhaps you believe that my numbers are unrealistic or way off.

Fantastic! That's the whole point of going through the effort of turning high-level ideas into detailed XYZ and xyz hypotheses. That's why we *say it with numbers*. Those initial numbers are only guesstimates for now (a *guesstimate* is a combination of a guess and an estimate). They are meant to stimulate discussion and bring differences of opinion and disagreements to the surface, so we can resolve them. If this were a real situation and we were part of a team exploring the idea for BusU, we'd invest a couple of hours discussing several possible xyz hypotheses before converging on a few. And then, after a couple of experiments and some further thought, we would probably revise them once again—or come up with completely new ones. That's how it works. That's how it's supposed to work.

So with the understanding that these may not be the best possible xyz hypotheses or the right initial numbers and that you'd handle this better or differently, let's proceed.

Time to Test

We have three xyz hypotheses to choose from, and we have to pick one of them to prototype for our first test. Each one of these hypotheses will provide us with valuable data, but which one should we start with?

Choosing Our First Prototype

We've already applied the “test locally” tactic, so let's see how the “testing now beats testing later” and “think cheap, cheaper, cheapest” tactics can help us determine where to start. To do that, we'll evaluate and score each xyz hypothesis in terms of Hours to Data and Dollars to Data. Let's begin.

xyz1: At least 40% of Google engineers commuting from San Francisco to Mountain View who hear about BusU will visit the BusU4Google.com website and will submit their google.com email address to be informed of upcoming classes.

To test xyz1, all we need to do is reach at least 100 Google engineers and develop a simple website. We estimate that that will take a couple of

days at most and just a few dollars. The time and cost estimate for xyz1 is:

Hours to Data: about 48

Dollars to Data: less than \$100

That's pretty darn good.

Let's see how xyz2 fares:

xyz2: At least 20% of Google engineers commuting from San Francisco to Mountain View will attend a one-hour lunchtime presentation to learn more about BusU.

To test xyz2, we still need access to 100 Google engineers, but we also need at least a few hours to create the presentation and a couple of weeks to get it on Google's calendar, send out the announcements, and so on. This experiment will get us more skin in the game, but it will also require more time and effort to set up than xyz1. The time and cost estimate for xyz2 is:

Hours to Data: at least 336* (two weeks)

Dollars to Data: less than \$100

Not bad. But "testing now beats testing later," and xyz1 can get us YODA faster than xyz2. So that is still our top choice for the first preotyping experiment.

What about xyz3?

xyz3: At least 10% of Google engineers commuting from San Francisco to Mountain View will pay \$300 for a one-week "Introduction to Artificial Intelligence" class on the bus taught by a Stanford AI professor.

This would require even more time and effort than xyz2. We'd have to line up (and possibly pay) a professor willing to do this, rent a bus, and so on. The time and cost estimate for xyz3 is:

Hours to Data: at least 672 (four weeks)

Dollars to Data: more than \$5,000

Compared to most market-research budgets, this qualifies as fast and cheap. But for us pretotypers, at this stage that's way too much time and

* That "336 hours" looks awkward, doesn't it? Great! It's supposed to. At this stage I want you to think in terms of hours, not weeks.

way too much money. Remember, “Testing now beats testing later” and “Think cheap, cheaper, cheapest.” At this point xyz1 is our fastest and cheapest route to YODA.

By applying our tactics and associated metrics, we’ve identified an xyz hypothesis (xyz1) that will give us our first taste of data in a couple of days and for just a fistful of dollars. If our initial experiments validate xyz1, we will then have YODA to justify investing a bit more time and money to test xyz2. And if xyz2 is validated, we can then justify investing a lot more time and money to test xyz3. But let’s not get ahead of ourselves. We have a great starting point, so let’s move on to prototyping.

Executing Our First Prototyping Experiment

Here’s the first hypothesis we will put to the test:

xyz1: At least 40% of Google engineers commuting from San Francisco to Mountain View who hear about BusU will visit the BusU4Google.com website and will submit their google.com email address to be informed of upcoming classes.

The next question I ask myself is this: What’s the best way to reach our *y*, the target market of Google engineers who commute from San Francisco to Mountain View?

Knowing Google, I assume the company has an internal website or mailing list for people who want to carpool with other Google employees or ride the Google buses. I ask one of my friends currently working at the company to see if that’s the case. He reports back to me with a list of several such resources, including an unofficial (i.e., employee managed) mailing list called MTVCarPoolers with over 1,600 members. Bingo!

I get an introduction to Beth, the list administrator, and we schedule a meeting so I can share the BusU idea with her. Beth loves the concept and agrees to help me test it. She confirms that more than half of the list members (820 of them to be exact) commute from San Francisco to the main Google campus in Mountain View. Perfect. I have potential customers for running at least eight different tests with 100 people each. (Since I am using a group of people that is representative of my hypothesized target market, 100 people is an adequate sample size for the results to be statistically meaningful.)

Now that I have an easy (and free) way to reach my target market,

we need to create a prototype website to establish a web presence we can use to collect our first YODA. I buy a suitable domain name and use one of the many drag-and-drop website development services (e.g., Squarespace, Weebly, Wix) to create a three-page website that introduces and explains the BusU concept. The total investment is \$20 and about two hours of my time—easy, fast, and cheap.

The website describes how the BusU service works and lists examples of the classes that will be offered and the bios of the professors who will teach them. Visitors to the site are asked to fill in the following form (skin in the game) if they want more information:

Name:

Email:

Google job title:

Classes and topics you are interested in:

Comments or questions:

I show the prototype website and the form to Beth. She asks me to make a change; she wants us to be up front with the fact that the service is not yet available but is currently being explored and that it is likely to happen if there is enough interest in it. Beth understands the importance of testing the idea first, but she wants to be as forthright and as ethical as possible. No problem. In fact, I thank her for bringing this up. I make those changes, and then we work together to compose the email message we will send to the first 100 Google employees on the San Francisco-to-Mountain View commuter list.

The next morning Beth sends out the email. In less than four hours, 88 people visit the website and 62 of them fill in the form. Woo-hoo! This is great. I had estimated 40% of the people would fill out the email form, and we got 62%.

But when I look at the data more closely, I notice that almost everyone who submitted a form asks if the service is free and, if not, they want to know how much it will cost and whether Google will help pay for it. Oops! I guess we should make those monetary points clearer in our email or on the website, especially since Google workers are accustomed to free meals, free massages, and many other perks.

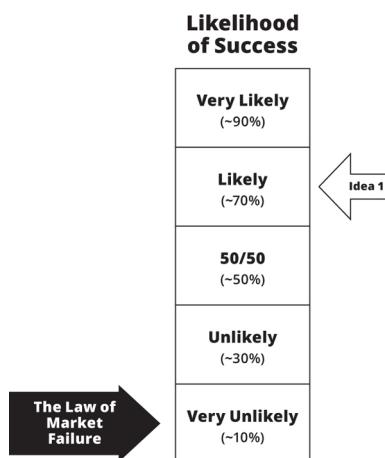
I talk to Beth about the feedback, and we agree that for the next ex-

periment I should edit both the email and website, being up front about the cost (\$3,000 for a ten-week class—which includes the bus ride). We also make it clear that at this time this is not a Google-approved continuing education program and that the students are responsible for the full tuition.

But before we tackle the next experiment, let's discuss how we would score this first experiment on the TRI Meter and what, if anything, we need to tweak in our hypotheses.

Analyzing and Iterating

Hypothesis xyz1 predicted a 40% response, and our test returned a very healthy 62%. This result exceeds our expectations and indicates a strong level of interest. Normally, a number this good (i.e., substantially better than our estimate) is an indication that an idea is Very Likely to succeed. But since we did not mention the hefty \$3,000 price tag in our email and many Google workers may have assumed that the BusU classes would be free (or be reimbursed by Google), I decide to be conservative in my interpretation of the result. Instead of Very Likely, I score it as Likely.



I could have been even more conservative and scored the experimental result as 50/50 or tossed it away as bad data. But getting a greater than

60% response to any idea in any market happens so rarely that I decided to take it as evidence of strong market interest, and “If there’s a market, there’s a way.” The next set of experiments will determine whether I am right or wrong.

We have an encouraging first result, but the big black arrow on the TRI Meter is doing its job. It’s keeping us grounded in reality. It reminds us that most new ideas will fail in the market and that we need to have several more successful experiments—a preponderance of positive evidence—to balance out the Law of Market Failure.

In the meantime, Beth meets with Google’s continuing education program manager and learns that Google will not consider BusU an approved educational expense until it is more established and has proven to provide comparable value to offerings by more traditional and already accredited educational institutions. Ouch. I ask a friend who works at LinkedIn to see if his company has a similar policy. Alas, the answer he comes back with is not what we want to hear: “They say that you need to have some track record and/or accreditation before they’ll consider it a refundable employee education expense.”

Darn! It looks like we should remove the possibility of employer subsidy from our hypothesis—at least at first. That’s disappointing, but “now beats later” also applies to bad news—better to know now, rather than later, that we cannot count on company subsidies until BusU is more established.

I edit both the email and the website. I make explicit the \$3,000 tuition cost and make it clear that—at least for now—BusU classes are not eligible for Google’s educational reimbursement. Then I cross my fingers and send out the next batch of 100 emails.

This time the results are less encouraging: only 42 visits to the website and, out of those, only 22 people submitted a filled-in form. I am disappointed but not surprised. When people are faced with the \$3,000 cost and the lack of company sponsorship (at least for the time being), it’s normal that fewer would be interested.

We had a plan, but we got punched in the face—not by Mike Tyson, but by the market. What now? Time to bring the *plastic* in plastic tactics into action.

The 22% response figure is approximately half our initial estimate of 40%. But considering that the 22 people who replied were okay with paying \$3,000 for the class out of their own pocket, I conclude that this number is actually quite good—something we can work with. Are we just lowering our expectations? No! We are testing, calibrating, and adjusting our initial assumptions against reality. We are not “cheating.” It is true that we are getting a lower rate of response, but those who are responding are willing to put more of their own skin in the game (i.e., their own money instead of Google’s).

To reflect this change, we can tweak xyz1 as follows:

- Lower our expected market engagement rate (x) from 40 to 20%
- Mention the \$3,000 cost
- State up front that the courses are not eligible for company reimbursement

Here's what this tweaked xyz1 would look like:

xyz1A: At least 20% of Google engineers commuting from San Francisco to Mountain View who hear about BusU's \$3,000 (not eligible for company reimbursement) courses will visit the BusU4Google.com website and will submit their google.com email address to be informed of upcoming classes.

Alternatively, we could:

- Keep the 40% number
- Use a smaller tuition number (say \$1,000)

Here's what this tweaked xyz1 would look like:

xyz1B: At least 40% of Google engineers commuting from San Francisco to Mountain View who hear about BusU's \$1,000 (not eligible for company reimbursement) courses will visit the BusU4Google.com website and will submit their google.com email address to be informed of upcoming classes.

Or we could move on to prototyping xyz2 or xyz3. We have several available options open to us. But while we are pondering the next steps something happens . . .

A Lucky Break

I get the following email from a Google employee named Bob:

Hi Alberto, I heard about your BusU idea from my friend Emily with whom I often commute to work. I love the concept, and I'd love to teach a class. I have a PhD in Artificial Intelligence from Berkeley, and I already have a ten-hour "Intro to Machine Learning" minicourse that I've taught several times at both Berkeley and Google (to great reviews, average 4.8/5.0). This would be a lot of fun for me, so you don't even have to pay me. When can we start?

I am thrilled by Bob's email and by his offer to teach an intro to AI class for free. In fact, it's almost too good to be true. Remember xyz3?

xyz3: At least 10% of Google engineers commuting from San Francisco to Mountain View will pay \$300 for a one-week "Introduction to Artificial Intelligence" class on the bus taught by a Stanford AI professor.

Bob is not a Stanford professor, and Machine Learning is a specific subset of AI, but close enough. This is a major lucky break! But I am not too surprised by it. I have learned to expect things like this to happen when I get out of Thoughtland and begin to prototype my ideas in the real world.

After I review the data from the first prototypes and consider this new development, I decide on a new course of action. I meet with Bob and tweak xyz3 as follows:

xyz3A: At least 10% of Google engineers commuting from San Francisco to Mountain View will pay \$300 for a one-week "Introduction to Machine Learning" class on the bus taught by a fellow Google employee.

Next, I call several local transportation companies and learn that chartering a suitable 40-person bus (including driver) from San Francisco to Mountain View will cost approximately \$1,000 per day, or \$5,000 per

week. I also learn that some of the buses already have television screens that the instructors can use to project slides or as electronic blackboards. Great.

I calculate that if I can get at least twenty people to sign up (at \$300 each) for the one-week AI class, I can cover the bus rental cost, gain some valuable firsthand experience running such a service, and even pay Bob for his time and commitment. Based on YODA from the previous experiment, I estimate that if we email 200 Google employees, we should get at least 10% of them to sign up for such a class. And if we get more, even better—the bus can accommodate up to forty people.

I tell Beth about Bob's offer and explain our new plan. She's impressed and even more enthusiastic than before: "I love the idea of employees teaching other employees." After confirming available dates with professor Bob, I list our first real course on the website and add a registration and an online payment page.

The next day, Beth sends an email to two hundred Google employees on the list describing the class, the \$300 cost, and the schedule. The message makes it clear that this is not a Google-sanctioned service, but a pilot program for a new company, and that it does not qualify as a reimbursable educational expense.

In less than two days forty-eight people register and pay! We sell out our first class and even have eight people on the waiting list. That's a 24% (48 out 200) response rate—considerably better than our 10% estimate for xyz3A. Things are looking really good.

We now have \$14,400 in the BusU bank account—that's 14,400 skin-in-the-game points (300 points for each \$300 payment times 48 registered students). That's a lot of the kind of YODA I like. The initial set of customers are also committing many hours of their time (even more skin in the game), but since they'd be spending that time on the commute anyway, I play it conservatively and decide not to include that in the skin-in-the-game calculation. It's time to board the bus!

I reserve the charter bus and, two weeks later, at 8:30 a.m. on a rainy Monday morning, the first BusU class leaves San Francisco with thirty-five students on board. Why only thirty-five people? Three people could not make it because of a change of plans—and ask for a refund. And two

of them arrive late and miss the bus. Unfortunately, it is too late to get the people on the waiting list to fill in the empty seats. I issue the refunds, and I still have plenty of money to cover the cost of the prototype. However, if I go forward with the idea, I will have to account for these kinds of situations by coming up with a refund and missed-the-bus policy. Perhaps I can do something similar to what airlines do and overbook the class by 5% or 10%. These are the kinds of invaluable real-world lessons you learn and data you collect by running prototypes.

On the plus side, the rest of the week goes very smoothly. Thirty-five students finish the one-week course and get the inaugural batch of BusU diplomas.

What's next? We had a successful first experiment, but we need more data. Specifically, we'd love to know if the *initial* level of interest in BusU will translate into an *ongoing* level of interest. Most businesses need repeat customers to be successful and stay profitable, and BusU is no exception. What percentage of our initial customers will sign up for another course?

From New Data to New Decision

It is customary to have students fill out a questionnaire after a course to rate the material and the instructor. In addition to that, such surveys also ask questions like, "Would you take another course from BusU?" and "Would you recommend BusU to your colleagues?" By now, you should realize that the answers to such "Would you . . . ?" questions may provide you with some interesting insights, but they are not data—because there's no skin in the game. So rather than asking skinless hypothetical questions, Bob and I decide to send an email offer to the first group of students with two skin-in-the-game options for the next class:

Option 1: A \$3,000 full ten-week AI course (as in the original plan)

Option 2: A \$300 one-week follow-up course, "Machine Learning 201"

We also ask them to share with us any feedback or suggestions they might have.

Two days later, we look at our new data:

- Twenty-one people signed up for the follow-up course. That's an

awesome result: more than half the students want to return.

- Nobody signed up for the \$3,000 ten-week class.
- A majority of the students commented that although they enjoyed the morning class and got a lot out of it, the evening session was much tougher to follow. They said that after a full day of work they were tired and all they wanted to do on the ride home was relax. (Bob also confessed that teaching the evening class was much harder, because he was also tired.)

This is a realistic example of the invaluable YODA that you can collect when you take an idea out of Thoughtland and prototype it. This data speaks loud and clear: \$300 one-week classes will be more popular—and much easier to schedule, plan, and sell—than the original idea of a \$3,000 ten-week course. Furthermore, we've learned that although the morning lectures worked well, the evening ones were a challenge for the students and the teacher, because everyone was tired at the end of the day.

Over the next six weeks we run two more prototyping experiments with one-week courses, one with Google employees and one with LinkedIn employees. The results from these new experiments align with and confirm xyz3A. This means that our hypothesis can now be promoted to data and that we have our first set of YODA:

YODA1: Out of a sample of 600, 132 (22%) of Google and LinkedIn engineers commuting from San Francisco to Mountain View paid \$300 for a one-week BusU course on artificial intelligence.

This YODA comes with almost \$40,000 of skin in the game (\$300 times 132).

Furthermore, in the process of prototyping, we've also acquired additional valuable YODA, including the following:

YODA2: Zero out of 132 (0%) students who completed a one-week \$300 BusU course on AI signed up for a \$3,000 ten-week course.

YODA3: 48% of students who completed a one-week \$300 BusU course on AI signed up for another one-week course at the same price.

YODA4: About 12% of students who sign up will miss the first class and ask for a refund.

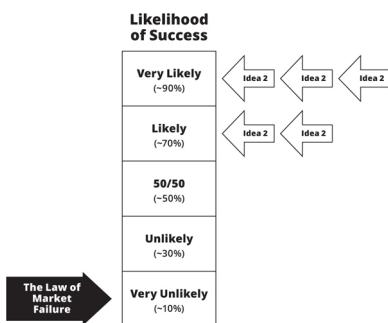
YODA5: 89% of the students prefer a morning-only class.

Instead of wasting time writing a BusU business plan based on OPD, opinions, and all sorts of untested assumptions, we invested the time to collect data to demonstrate that there is indeed a business. *Before writing a business plan, make sure that there is a business.*

What's Next?

The YODA spoke loud and clear. A significant number of high-tech professionals are interested in BusU's one-week courses, but not in the longer (and much more expensive) classes. So, at least for now, we abandon the idea of offering ten-week classes for \$3,000 (i.e., Idea 1). We decide instead to focus on one-week \$300 classes (Idea 2), but with a tweak to the original plan: we will limit the formal lectures to the morning commute, and use the evening commute for more relaxed and informal office hours and Q&A with the instructor.

We update the XYZ and xyz hypotheses and run a few more prototyping experiments to validate this new model. In the process we collect even more YODA and learn additional valuable lessons (e.g., we can earn an additional \$60 per student by selling coffee and snacks on the bus). Here's what the TRI Meter for the BusU business model for \$300 classes (Idea 2) looks like after five experiments:



It looks as if the new version of BusU has a good chance of being The Right It, wouldn't you agree?

A Few Notes About the BusU Example

Before we close this chapter, I'd like to point out a few important things about this example.

Note 1: In the process of validating our idea, we've collected the kind of firsthand data (YODA) with skin in the game that will help us make a very strong business case if we decide to seek VC funding.

- Instead of a Thoughtland-based business plan filled with hope, hype, and fantasy five-year financial projections, we can show actual costs, actual revenue, actual profit, and the kind of skin-in-the-game market feedback that is hard to ignore (e.g., 48% of the students who took one class signed up for at least another class).
- With several successfully completed courses, we have also demonstrated that we know how to run such a business (i.e., that we can execute the idea competently—build It right).
- By investing our own skin in the game (our time and money) to test the idea and by working through the numerous challenges and obstacles, we've shown commitment and resiliency.
- Finally, by tweaking our original vision and business model to reflect the data we collected, we've demonstrated flexibility and agility in responding to the market—two must-have characteristics to succeed in today's rapidly changing markets.

A team bringing all of the above data and evidence to a potential investor will not only significantly increase their chances of getting funded, but also be in a strong position to ask for a higher valuation.

Note 2: This was a fictional scenario meant to illustrate our methods, but it's based on a true story. I did have the idea for BusU while stuck in commute traffic and for a period of time I seriously considered starting such a business. Furthermore, everything I've described is both plausible and doable: Google does offer free commuter buses to its employees, Google does reimburse some tuition expenses, charter buses can be rented

for about \$1,000 per day, and I am sure that I would be able to find some AI experts willing to teach the classes.

Note 3: This example points toward a happy ending—but not for the first version of our idea. If we had gone full speed ahead with the original plan based on \$3,000 ten-week accredited classes, our BusU business would have crashed! The original BusU idea was *The Wrong It*; we found *The Right It* version of BusU through testing and tweaking.

Note 4: My goal for the BusU example was to show you one possible way the tools and tactics we've learned might be combined and a possible scenario of how things might work out. The specific sequence of events and results will vary from case to case. But, generally speaking, your goal is to go from rough idea, to testable hypotheses, to experiments, to data from those experiments, and then to use that data to make an informed decision on the next steps. Depending on the nature and quality of the data you get, those next steps can range anywhere from making minor tweaks to the idea or hypotheses (and running more tests), to scrapping the idea altogether, to—if you are lucky—determining that your idea is likely to be *The Right It* and going forward with it.

9 Final Words

I began this book with the following ominous warning:

It waits. Patient.

Confident that it will soon get its prey—it always does.

Few escape its bite, none its tentacles.

One way or another, the Beast of Failure gets us all.

I wrote those somber words in one of my notebooks in the days right after my startup failed. They came out of my pen as I was trying to figure out how the hell this could have happened. As you can probably tell, I wasn't exactly in a cheery mood at the time. It was a dark and bitter period for me for two reasons, one obvious and one not so obvious.

The obvious reason for my gloomy mood was that our company, after an incredibly promising start, almost \$25 million of investment from three of the most successful VC firms in the world, and five years of really hard work by dozens of top-notch people, had to be shut down and sold off. As much as that hurt and embarrassed me and many of the people associated with the company, I knew we'd all recover and move on.

The final meeting with the investors, the board of directors, and the lawyers was not exactly fun, but it was not as terrible as I—a novice at failure on this scale—thought it would be. No one yelled, no one got angry, no one pointed blaming fingers. Instead, the tone of the meeting was a combination of disappointment, understanding, and—to my surprise—a *lack* of surprise. The investors, the other board members, and the lawyers had been through the same scenario (brilliant idea + good plan + plenty of funding + strong and experienced team + competent execution = failure) many times already. For them it was par for the course. Most startups fail, even those that hold much promise and seem like a sure thing. Remember Webvan?

I knew the stats about startup failure too, but somehow I didn't think that those dismal odds had any relevance to our company. They couldn't possibly apply to us. For starters, we had gone through exhaustive technical due diligence, done our market research, wrote a great business plan, and gotten funding from great investors. Then, after putting together an

amazing team, we built the exact product we said we were going to build and our target market told us they wanted, needed, and would definitely buy. In other words, we had checked all the boxes.

So WTF!? Why The Failure!? It made no sense to me.

That was the root of the second, less obvious (but deeper and longer-lasting) reason for my gloom. What made it so pitch dark, so bitter, and so pervasive was that several of my deeply held beliefs about the way the world is supposed to work had been shattered. The formula for success that I witnessed, learned, believed in, and emulated had failed me. To my internal logic it was as if, all of a sudden, I could no longer count on 2 plus 2 adding up to 4.

At first, I felt lost, hopeless, betrayed. Then, slowly but steadily, the shock and pain of my first big failure morphed into something healthier: a desire to understand how and why something like this could happen—not just to our company, but to anyone who tries to bring a new idea into the world. I wanted to use that understanding to prevent it from happening again. As I mentioned in the Prelude, the Beast of Failure had bitten me, and I decided to bite back, or at least learn how to armor and defend myself against the worst of its bite.

I would figure out where and how we had gone wrong, discover ways to prevent those errors in the future, and share those findings with the world. That's what this book is all about.

In this concluding chapter I review and summarize those findings, repeat and highlight some of the most important points, and close with some important final words of advice and encouragement.

The Right It: A Recap

A Review of the Hard Facts

Part I began with the following imperative, the guiding principle of this book:

Make sure you are building The Right It before you build It right.

To reach The Right It, you must come to terms with a number of hard facts, hard because they are hard to take, hard to avoid, and unlikely

to change. The mother of all hard facts is the Law of Market Failure:

Most new products will fail in the market, even if competently executed.

Most new products that make it to market fail because they are *The Wrong It*, defined as *an idea for a new product that, even if competently executed, will fail in the market*.

Next hard fact:

No amount of design brilliance, engineering prowess, or marketing fireworks can save a product that is The Wrong It from the jaws of the Beast of Failure.

This second fact explains why even the world's most successful companies (e.g., Coca-Cola, Disney, Google) will often launch new products that fail in the market. These expertly executed new products fail because they are based on the wrong Premise—the market is not interested in them regardless of how well (or by whom) they are designed, built, or marketed.

I then introduced the hero of our book: *The Right It* is *an idea for a new product that, if competently executed, will succeed in the market*. Which brought us to our third hard fact:

Your only chance for success is to combine competent execution with a product that is The Right It.

But how can we know if a product is *The Right It* before we have actually built It? How can we tell if the market will want It if It does not even exist yet? The seemingly logical—but catastrophically wrong—answer to that question is: “We'll ask people if they want it or will buy it.”

Unfortunately, this approach, conceived of and carried out in Thoughtland, produces opinions, not data. And people's opinions, including so-called expert opinions, are not reliable predictors of success, because our thought process and conclusions are invariably distorted by a covey of cognitive errors and biases. In Thoughtland, many ideas that are *The Wrong It* get an enthusiastic thumbs-up from the target market (a false positive), while many ideas that are *The Right It* get shot down as lame or ludicrous (a false negative).

So when it comes to validating ideas for new products, we cannot depend on what people think, say, or promise. Instead, we must escape the treacherous trolls of Thoughtland by recognizing and acting upon our

fourth hard fact:

Data beats opinions.

But not just any old data, especially not Other People's Data (OPD). OPD is at best unreliable, because what happened to other people, with other products, at other times does not necessarily apply to our idea for a new product.

So the next hard fact specifies what kind of data we need:

You need to collect Your Own DAta (YODA).

If “data beats opinions” and YODA beats OPD, then does that mean that *YODA beats every other possible source or form of data* about the likelihood of success of your idea? Yes! Most emphatically YES! YODA beats them all—provided that YODA is collected, filtered, and analyzed rigorously and objectively.

The final hard fact assures the quality of your data:

To count as proper YODA, your market data must come with some skin in the game.

You cannot just ask the market if they are interested in and likely to buy your new product and take their word for it. You need something of value to back up their statements and promises, ideally money—the most universal and quantifiable form of skin in the game. But how do you solve the chicken-and-egg challenge of collecting YODA to determine if your idea is The Right It unless you have already built It? You use the tools in Part II.

A Review of the Sharp Tools

Finally, after a barrage of hard facts, the first bit of good news: not only is YODA the most reliable and relevant indicator of potential market success, but collecting YODA is faster, cheaper, and more fun than collecting worthless opinions and stale OPD—provided you have the right tools.

Thinking Tools

The first step in our quest for reliable YODA is to rid our idea of the mental fuzz (vague descriptions, unspoken assumptions, etc.) acquired in Thoughtland and *articulate it as precisely and as clearly as possible*. This step is particularly valuable for teams, because it helps identify and reconcile conceptual differences among team members.

Behind every idea for a new product is a Market Engagement Hypothesis (MEH). The MEH is a high-level description of how we assume (or hope) the market will engage with our product. In the case of Second-Day Sushi, for example, the MEH was:

Many people will buy less-than-fresh sushi if we make it cheap enough.

The Market Engagement Hypothesis is a necessary starting point, but it's usually too vague to be usable. We need to *say it with numbers*, turning the fuzzy MEH into an unambiguous XYZ Hypothesis using the format: "At least X% of Y will Z."

At least 20% of packaged-sushi eaters will try Second-Day Sushi if it's half the price of regular packaged sushi.

Finally, we need to go from a broad XYZ Hypothesis to a set of smaller xyz hypotheses that we can test quickly and cheaply. Here's one example:

At least 20% of students buying packaged sushi at Coupa Café today at lunch will choose Second-Day Sushi if it's half the price of regular packaged sushi.

In three quick steps, we went from a vague high-level idea to a clearly stated, ready-to-be-tested hypothesis. Now, armed with our xyz hypothesis, we are ready to test our new product idea on our target market. Even though we don't have the product to test our hypotheses with yet, pretotyping will enable us to forge ahead without it.

Pretotyping Tools

Pretotyping can play a key role in validating our idea. Pretotyping differs from traditional prototyping in that prototypes are usually designed, built, and used to verify that an idea can be built, to explore the best ways to build it, and to test that it will work as expected, and pretotypes are built for a single—but singularly important—purpose: to validate our Market Engagement Hypothesis.

Prototypes help us answer the key question: *Can we build it?* Pretotypes, in contrast, answer a very different question: *Should we build it?* With pretotyping, we can answer this question quickly and inexpensively. Whereas traditional prototypes can take weeks, months, or years to develop and cost millions, pretotyping experiments can produce data in a matter of hours or days and cost little.

Some prototyping techniques include the Mechanical Turk, the Pinocchio, the Fake Door, the Facade, the YouTube, the One-Night Stand, the Infiltrator, the Relabel, but these are just a small sample of the many great effective, efficient, and fun ways to prototype an idea. For any new product idea there is—at least—one great way to prototype it and to get some YODA in a matter of hours. I encourage you to modify, adapt, and combine these basic techniques to best fit your idea. Better yet, invent and experiment with your own technique and give it a name (e.g., Mike’s Magic Carpet prototype).

Analysis Tools

After running a few well-crafted prototyping experiments you will finally possess the most valuable, relevant, and reliable type of data: fresh YODA straight from your target market. But having fresh YODA is not enough; you need to calibrate it and interpret it before you can use it to reach a conclusion on which to base a decision. A pair of analysis tools, the Skin-in-the-Game Caliper and the TRI (The Right It) Meter, will help you weigh, analyze, and interpret the YODA you collected with rigor and objectivity.

The *Skin-in-the-Game Caliper* allows you to assign the proper value to the data you collect according to how much skin in the game accompanies it. A \$250 preorder counts more than, say, a \$50 deposit to be put on the waiting list, which counts more than a submitted email address. Opinions, of course, are worth zero points; as are thumbs-ups, likes, and comments.

The *TRI Meter* is a graphic tool designed to help you take the result from each prototyping experiment, put it against your XYZ and xyz hypotheses, and judge the degree to which the YODA supports the likelihood that your idea is The Right It. The TRI Meter uses a five-step scale that ranges from Very Unlikely to Very Likely, and, as a reminder that most new ideas will fail in the market, at the bottom, pointing to Very Unlikely, is the big black Law of Market Failure arrow. That arrow reminds us that we need more than just a couple of encouraging experiments to balance out those initial odds.

Since most new ideas will fail, if you’ve been objective in designing the experiments and fair in evaluating the results, you should not be sur-

prised if your first few TRI Meters yield Unlikely or Very Unlikely scores. But if you keep at it, unless you are really unlucky, you will eventually find yourself staring at a TRI Meter with Idea arrows pointing to Likely or Very Likely. There are no guarantees, of course, but—assuming that you've been careful, objective, and fair in applying the tools and judging the data—a positive TRI Meter should allow you to move on to the next stage in the development of your idea: building It right.

A Review of the Plastic Tactics

Some tactics to help you use our tools more efficiently and effectively include “Think globally, test locally,” “Testing now beats testing later,” and “Think cheap, cheaper, cheapest.” Those tactics suggest that you design your experiments to minimize Distance to Data, Hours to Data, and Dollars to Data. A final tactic, “Tweak it and flip it before you quit it,” teaches the importance of tweaking your idea before abandoning it if the YODA from your initial set of experiments is discouraging. The first version of your idea may be The Wrong It, but you may be just a few tweaks away from The Right It: test, tweak, repeat.

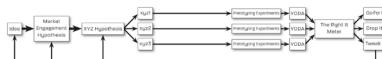
A final comprehensive example, BusU, teaching classes on commuter buses, showed how the many tools and tactics can be applied, combined, and put into action. We went from idea, to hypotheses, to experiments, to decisions. The example was realistic and peppered with unexpected challenges and opportunities. These demonstrated that it's perfectly okay to tweak your original idea and change your plans along the way based on what you learn about the market as you run your experiments. In fact, it's more than okay—it's exactly what you should do.

There Are No Guarantees

Pew, we've covered a lot of ground, haven't we? It may seem like a lot to keep track of. If I could have made it shorter or simpler, I would have, as in the quote usually attributed to Albert Einstein, “Everything should be made as simple as possible, but not simpler.” However, each of the steps fits into a logical sequence that should make it easier to remember and that, with a bit of practice, will become second nature and automatic.

1. Start with an idea.

2. Identify the Market Engagement Hypothesis.
3. Turn the MEH into a say-it-with-numbers XYZ Hypothesis.
4. Hypozoom into a set of smaller, easier to test xyx hypotheses.
5. Use pretotyping techniques to run experiments and collect YODA.
6. Use the TRI Meter with the Skin-in-the-Game Caliper to help you analyze the YODA.
7. Decide on the next step:
 - a. *Go for it!* You can't be 100% sure that your idea is The Right It, but the YODA looks promising enough for you to take a calculated risk.
 - b. *Drop it.* As much as you'd like your idea to succeed, the YODA stubbornly indicates otherwise.
 - c. *Tweak it.* In the process of testing your idea, you have learned invaluable things about your target market and its willingness (or unwillingness) to engage with your product. Don't hesitate to tweak your original idea (or your hypotheses) to reflect what you've learned along the way. You may find that although there is not enough market interest in your original idea, there may be great interest in something related to it. Or try flipping the idea on its head and see what you come up with.



Even if competently executed, these steps cannot guarantee that an idea will succeed in the market. It's impossible to predict or take steps to prevent all of the possible external factors that can turn even the most promising and well-tested idea into a market failure. But if you diligently follow and apply what I've shared with you in this book, I feel confident enough to make you three promises.

Promise 1: You will dramatically reduce your probability of failure. How dramatically? If you run enough well-designed pretotyping experiments, and their results provide strong and consistent validation for your hypotheses, then you have probably found an idea that is The Right It. Armed with The Right It, you should be able to flip the odds in your favor: from an 80ish percent chance of failure to an 80ish percent chance

of success. Provided, of course, that you execute the idea competently (i.e., you build It right). Unforeseeable and unpreventable external events and bad luck may still cause you to ultimately fail. But if that happens I can make the next promise to you.

Promise 2: Even if you fail, you will not feel like a fool. If you crash and burn in the market *because* you haven't adequately tested your idea, you should feel like a fool. Especially now, because after reading this book you should know better. But if you fail *despite* all your diligent and objective market testing, then you might feel disappointed and discouraged, but you should not feel foolish. If a poker player holding four aces (1 in more than 4,000 odds) bets big and loses to another player's royal flush (1 in more than 70,000 odds), that's not being foolish; that's being extremely unlucky.

Promise 3: If you stick with it, you will eventually succeed. Throughout the book, I've talked a lot about the Beast of Failure and the Law of Market Failure. Failure is the villain of this book and, like all good villains, it grabs our interest and demands attention. But we should not forget that although market success happens much less frequently, it is not *that* rare. It's not a one-in-a-million shot. It's not like winning the lottery. It's much, much better than that. If 80% to 90% of new products fail in the market, that means that some 10% to 20% succeed. Those are odds that we can work with.

That means that, with some luck, if you apply what you've learned in this book, you may have to go through and test five or ten ideas (or variations on the original idea), but you will eventually hit on an idea that is The Right It—provided, of course, that each successive idea (or idea variation) integrates and takes into account what you've learned from your previous market experiments. Even if a prototyping experiment fails to validate your hypotheses, you will discover something new about the market: new facts, new opportunities you did not know about, new resources. And if you are smart, you will use that new information and those newly discovered resources to guide your next steps—which will help you find The Right It sooner.

It's a bit like card counting in blackjack: if you keep track of the cards that have already been dealt and bet accordingly (bet less when the re-

maining deck is unfavorable, more when it's favorable), you can turn the odds of winning in your favor. That's why most casinos ban card counters. The real world is, of course, infinitely more complicated and far less predictable than a casino game. In the market, the risks, rewards, and odds of success for any given idea change all the time (that's why it's so important to have fresh firsthand data—YODA). But the fact remains that the more data you collect by experimenting in a given market, the better you will understand some of its behaviors, and the more likely you are to converge on an idea that is The Right It.

All right, we are almost done. But there is one final exceedingly important topic that we need to cover.

What to Build?

As I hope you will learn firsthand by applying them, the tools and tactics I've shared with you are powerful stuff. They have the power, if properly used, to help you avoid The Wrong It, find The Right It, and thus flip the odds for success in your favor. And that's no small thing. In fact, it's a *huge* advantage!

But, as they say, with power comes responsibility. How will you use that power? Or more precisely, what kind of new idea would you create and bring to market if you were reasonably confident (say 80ish percent) that it would succeed?

That's a big question. An existential question. One that deserves a lot of thought, because even if you are sure that you have The Right It, it will still take a lot of time, investment, effort, sacrifice, and commitment to build It right and to keep It going. A full and proper answer to this question is beyond the scope of this book and probably deserves a book of its own. But I will briefly address two key aspects of this question:

Is this idea The Right It for you?

Is this idea The Right It for the world?

Make Sure It's The Right It for You

The best ideas—the ones to which the market responds with the greatest interest and enthusiasm—are often the ones that require the most effort and commitment. Before you know it, you may find yourself with

thousands of eager customers knocking on your (fake) door. It's exciting, but it can also be overwhelming. And unless you are fully committed to your idea and prepared for all the work ahead, that kind of success can create more pain and headaches than a failure.

Early in my career, one of my managers had an expression for that kind of situation; he called it a *success disaster*. He was fond of saying, "If you think failure is hard, you haven't experienced real success." I didn't fully understand what he meant until I found myself in a success-disaster scenario myself. He was right! Failure is a beast, but success can be a beast too. The experience of finding and latching on to an idea that is The Right It can often be compared to having the proverbial tiger by the tail. Just ask Tesla's Elon Musk.

As I am writing this, Musk is experiencing just such a success disaster. In 2016 Tesla announced the long-awaited Tesla Model 3—the company's most affordable car, about half the price of the previous models. Following the proven Tesla playbook, the company required people to put down a \$1,000 deposit (skin in the game) to get in line for the car. Pre-orders poured in by the hundreds of thousands (talk about YODA!). The first year of production for the Model 3 sold out in a matter of days, and the orders kept pouring in. Eventually Tesla found itself with approximately 500,000 preorders—half a billion dollars of skin in the game, an unprecedented and unqualified success by most automotive-industry standards. Now all that Tesla had to do was to build and deliver a few hundred thousand Model 3s. That's a lot of cars for a fledgling and relatively small auto company.

Fast-forward a year or so and, not too surprisingly, a series of production issues resulted in significant delays and uncertainty in delivery dates. Many of the people who had put their \$1,000 deposits in Tesla's bank account were getting upset and wanted to know if and when they would get their cars. Elon Musk responded (via Twitter) as follows to one of those people:

You'll know as soon as we do.

We are deep in production hell.

On another occasion, Musk tweeted the following about the situation: "The reality is great highs, terrible lows, and unrelenting stress. Don't

think people want to hear about the last two.”

But people did want to hear more about the “last two”—and how Musk handles terrible lows and unrelenting stress. He replied as follows (emphasis mine): “I’m sure there are better answers than what I do, which is just take the pain and *make sure that you really care about what you’re doing.*”

Thank you, Elon, for providing us with the inspiration, and even the wording, for the key message of this section—an important qualification to our main message:

**Make sure you are building The Right It
and make sure that you really care about It
before you build It right.**

As we’ve learned, finding The Right It is not easy; it takes a lot of work, creativity, and persistence. Unless you are very lucky, it will take several rounds of testing, failing, and going back to the drawing board before you land on an idea that survives our market validation process. So when you finally discover that winning idea for a new product, be prepared to do it justice—and be prepared for the hard work ahead. Finding The Right It is the end of one quest and the beginning of another, longer and harder quest: building It right, and then marketing It right, selling It right, servicing It right, and so on, all the while fighting the competitors that inevitably pop up when a new idea that is The Right It is discovered.

It’s unlikely that you’ll ever find yourself in a success-disaster situation of the same magnitude as Elon Musk’s, but I guarantee you that you will have to deal with a wide range of problems, complications, and mini-disasters of your own. And unless you really care about that idea of yours, you will not have the motivation necessary to handle those obstacles and see it to fruition. In other words, in order to ultimately succeed with an idea, it’s not enough to know that the idea is The Right It for the market; it has to be The Right It for you. It has to be a two-way match.

How can you know ahead of time if an idea is right for you? Once again, I can give you no guarantees. But leaving Thoughtland and going through the prototyping process will teach you not only about the market’s actual response to the idea, but also about *your* response to working with the idea. And that’s another great reason to do prototyping. Let me

explain.

Being clear about your hypotheses, putting them to the test, and analyzing the results objectively is the most effective and efficient way to validate your idea against the market. It requires commitment and quite a bit of work, but it should also be fun, exciting, and engaging. If you are not experiencing at least some fun and excitement working with your idea at this early stage (e.g., devising, creating, and running pretotyping experiments), either you are doing it wrong or the idea and market you are testing are probably not a good match for you. And that's something that you should not overlook, because it's a strong indicator that the idea you are working on may not be *The Right It* for you.

If you are not enjoying the pretotyping process, at some point you have to ask yourself some tough questions:

Even if this idea that I hatched in Thoughtland turns out to be *The Right It*, is it likely to be *my thing*?

Am I cut out for this kind of job, this kind of product (or service or business)?

Do I really want to be in this market for the next few years?

If you cannot answer these questions affirmatively, you should reconsider pursuing the idea—even if that means abandoning an idea that is likely to succeed in the market. You don't want to be stuck working on something you don't care about. Sooner or later (and probably sooner rather than later) you will get fed up with it and stop giving it your best—you'll stop building *It* right. And that's not fair to your idea, your investors, your customers, or yourself.

Many people, including myself, dream of starting a restaurant. My wife and I talk about it all the time. In Thoughtland owning a restaurant sounds great: you get to have fun designing the dining room, creating the menu, hobnobbing with the customers, and making lots of money along the way. The reality, as every restaurateur knows, is very different—more grungy than glamorous. Even if your restaurant concept is *The Right It*, you will be spending more time on marketing, personnel issues, supplier problems, accounting, and so forth, than coming up with the perfect steak tartare (yum!).

As you prototype your idea, you will learn not only if the idea is *The*

Right It, but if you are the Right Person (or Team) to execute the idea. And that's just as important. Let me share with you a typical example.

Two years ago, I received an email from a young entrepreneur; let's call him Darrell. Darrell had an idea for an innovative and eco-friendly diaper-delivery and -disposal service. He had read my preto-book *Pretotype It* and thought that two of the pretotyping techniques I described were perfectly suited for testing his idea. To make sure he was on the right track, he contacted me to explain his concept and to get some feedback on his plans for pretotyping it. I suggested a couple of minor tweaks to make his tests more objective, wished him good luck, and asked him to keep me posted. And since my children are now in their twenties, that's the last I thought about diapers for a while.

A few months later, Google asked me to teach a seminar on pretotyping to the diapers division of a major consumer-goods company. While preparing for the event, I was reminded of Darrell's diaper-delivery idea. I was curious to know how his pretotyping experiments turned out, so I contacted him to get an update. The next day I received an email from him:

Sorry I didn't keep you posted as promised, Alberto.

I ran the experiments we discussed and got good results—enough to convince me that the idea had a really good chance to be The Right It.

But in the process of doing that I realized that I don't really want to be in the diaper business. It's not my thing. I am now pretty sure I can make my idea for the business successful—perhaps even very successful—but I had no fun working on it. To be honest, I hated it! It was a good idea, and someone else should do it, but it won't be me. I learned that I don't give a darn about diapers. I don't even have children. I would just do it for the money, and that's not good enough. My real passion and interest is soccer. So I will try to come up with a soccer-related idea and use your techniques on that.

I am sorry for wasting your time and sorry to disappoint you.

Darrell didn't waste my time (or his), nor did he disappoint me. Far from it. He didn't find the diaper business sufficiently, ahem, absorbing. Good for him! He discovered that this industry is not his thing—and that's a great thing. Better that he discovered that now rather than later, when

he would be knee-deep in it.

When you use prototyping to test an idea against the market, the idea and the market are also testing *you*—sort of. So while you are collecting market data about your idea’s market *desirability*, also keep an eye on its *enjoyability*. Would you enjoy working with this product, in this market, for the long term? Running a business based on your idea may sound great in Thoughtland, but as Darrell and many aspiring restaurateurs have discovered, the reality is often very different.

Make sure that your idea is not just a good match for the market, but also a good match for you.

Make Sure It's The Right It for the World

I saved this section for last not because I consider it an afterthought, a postscript, something relatively unimportant. On the contrary, I saved it for last because I consider it to be extremely important. These are the words with which I want to leave you.

Everything I’ve shared with you up to now has been factual, practical, logical: tools, techniques, tactics; metrics, markets, money; testing, trying, tweaking. Time to shift gears and raise the level of our discourse. Time to get a little philosophical—for your own long-term benefit and that of the world. Don’t worry, I am not going to go all preachy on you. I just want to make sure that now that you have the tools and know-how to help you succeed, you will feel confident to go after bigger, better, more worthwhile ideas. Not every idea is equally worthy of our efforts—even if, technically, it is The Right It.

Stay Away from Breaking-Bad Ideas

If we define The Right It based purely on market demand (i.e., lots of people wanting, needing, buying your product) and market success (i.e., a highly profitable multimillion-dollar industry), then crack cocaine, meth, cigarettes, and a number of other recently developed addictive products and substances, both legal and illegal, would qualify.

Where should you draw the line to determine if an idea is the *wrong* Right It, a product that will succeed in the market but will do more harm than good to everyone involved in it? I can’t tell you that. I obviously have my own set of totally subjective and biased criteria: crack cocaine, bad; microbrewed beer, good (and single-malt scotch even better). Let a

combination of your conscience, local laws, and customs be your guide. Asking yourself, “What would my grandma think of this?” should also steer you right most of the time. (I say *should* because I don’t know your grandma.)

With the tools and knowledge that you now have at your disposal, you possess the means to explore and test a wide range of ideas, so there is no reason to get involved in the kind of dangerous, shady, or illegal products and businesses that can get you and others in trouble. Hopefully, your idea for a new business does not involve cooking up a superior variety of meth in the middle of the desert. Don’t go *Breaking Bad* on me. On the other hand, if your new idea involves tasty microbrewed beer or single-malt scotch, feel free to send me samples.

Think Beyond Quick-Buck Ideas

Some ideas for new products may not violate any laws or customs; they are neither immoral nor unethical. But they are also not the best use of your knowledge, time, and effort. Such ideas may not make the world a worse place, but they won’t make it a better place either—they won’t make much of a difference one way or another. They are the kind of gimmicky products that are bought on impulse, used once or twice, and then tossed away or forgotten.

Soon after the original iPhone was launched, for example, the market was inundated with all kinds of really silly 99¢ apps: fart machines, simulated paper staplers, virtual cigarette lighters, “games” in which you compete against others to see how many times you can tap on a virtual button, and so on. (Full disclosure: I admit to buying one such app in 2008—but you’ll have to guess which one.) There’s nothing inherently wrong with pursuing quick-buck ideas—especially at the beginning of your career (say, to pay off your student loans) or when you have an urgent need for cash (say, to buy a Tesla . . . just joking . . . well, sort of). But if you have the programming skills to develop a mobile phone app, do you really want to use them to turn a smartphone (which is an amazing tour de force of technology) into a virtual whoopee cushion?

Think Beyond Business

Most of the examples in the book are based on business ideas, but all the concepts and tools you learned about can—and should—be applied to

other areas. Nonprofit organizations such as charities, hospitals, schools, and even governments are subject to the Law of Market Failure just as much as commercial ventures. Social entrepreneurs measure their success using metrics other than money (e.g., percentage of people with access to clean water, reduction in the number of deaths by malaria, number of freed political prisoners), but they still have to deal with the very high probability that their new idea or approach will not work as well as they think or hope. In fact, when it comes to many of the world's worst problems (including famines, preventable diseases, and all sorts of violence), we are still desperately looking for solutions that are *The Right It*. The Beast of Failure will not give you a pass just because you are working for a good cause, but our tools and tactics will help you *fight the good fight* and win.

Go for the *Right* Right It

What would you build if you knew that it would succeed? What kind of product, service, book, or company would you create?

The world is full of serious problems waiting for solutions and meaningful opportunities looking for champions. My hope is that what you've learned in this book will give you not just the know-how, but the confidence and courage to aim high and to build and bring into the world something of lasting value, something that will make the world better, something worthy—and worthy of you.

Finding an idea that is *The Right It*, building It right, achieving market success, and reaping the financial rewards that can come from it feels great. But accomplishing the same with an idea that is particularly meaningful to you and beneficial to the world feels *amazing*.

Furthermore, you will discover that if an idea is meaningful to you and beneficial to the world, your odds for success will increase dramatically for two reasons. First, if you really care about the problem you are solving and the market you are serving, you will be much less likely to abandon the idea when you run into your first (second, third, or umpteenth) obstacle. You will find the motivation and energy to keep going and overcome whatever challenges come your way.

Second, if your product is beneficial and valuable to the world, you will not only experience much less resistance to it, but also find all sorts

of unexpected people and organizations popping up to help you and cheer for you along the way because they want to see you and your idea succeed.

So don't settle on just any idea. Look for an idea that is the *right* Right It—an idea that, if competently executed, will not only be successful in the market, but will also be meaningful to you and beneficial to the world. Then do It justice and build It right.

I am rooting for you!

The beast is still there.

Still waiting. Still hungry.

Unchanged and unchangeable.

Ready to fight . . .

and so am I.

Acknowledgments

First and foremost, I want to acknowledge and thank Stanford University professor Tina Seelig. Tina has not only been an amazing supporter and champion of my work for many years, but she, more than anyone else, has encouraged, pushed, and prodded me to stop making excuses and to “just sit down and write the darn book.” The fact that Tina managed to publish two books of her own—and begin work on a third one—while I kept procrastinating provided me with the motivation and embarrassment I needed to get going. Tina, it’s safe to say that without you this book would still only exist in my own private Thoughtland. Thank you so much; *you are a gem.*

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The second group I want to acknowledge and thank are the many people who, in one way or another, helped me develop, refine, and share the material covered in this book.

The original set of concepts and tools for The Right It and prototyping were first developed and tested at Google with the participation of hundreds of Google team members. Unfortunately, I cannot acknowledge them

all, but a few individuals and teams deserve special mention. Eric Schmidt and Wayne Rosing, who not only hired me and mentored me as a young engineer at Sun Microsystems, but also gave me the honor and opportunity of a lifetime by hiring me again as Google’s first engineering director. Urs Hözle, for his wise guidance and support. Patrick Copeland, who got stuck managing me when I began to experiment with “that pretotyping thing” and who has been a great evangelist and supporter ever since. The original Unleash the Innovators team of pretotypers: Bob Evans, C.P. Lim, Stephen Uhler, and Deanna Chen. Cecelia Wogan-Silva, the first to bring me and my work outside Google and beyond Silicon Valley. Google’s Chief Innovation Evangelist and Stanford d.school adjunct professor Frederik G. Pferdt. A final—and very special—thank-you goes to Google’s amazing Executive Summits team, for giving me the opportunity to share The Right It and pretotyping with many of the world’s largest and most influential companies—thank you, Brandi Andrews Mihailovski, Alaina Beeman, Christina Carroll, Alan Eagle, Mike Emling, Matthew Hubbard, Kristie Lane, Michele Maiorino Kelly, Jessica Michaelis, Theresa O’Connell, Ivonne Tamariz, Kristin Villanueva, and Jessica Webb.

Stanford Graduate School of Business professor Baba Shiv gets the credit, and has my deepest appreciation, for being the first to invite me to give a lecture on pretotyping at Stanford. *Grazie, Baba!* Since that first lecture in 2011, I’ve had the pleasure and honor to teach, coach, and share pretotyping with hundreds of Stanford students and visiting executives through lectures, courses, and workshops. In addition to Baba Shiv and Tina Seelig, I would like to acknowledge and thank the following members of Stanford’s faculty and staff for all the amazing teaching opportunities and support they’ve given me over the years: Tania Abedian, Tom Byers, Leticia Britos Cavagnaro, Chuck Eesley, Mark Grundberg, Matt Harvey, Rachel Julkowski, Riitta Katila, Harjot Khara, Tom Kosnik, Michael Peña, Navdeep Sahni, Anaïs Saint-Jude, Nikkie Salgado, Eli Shell, Ryan Shiba, and Danielle Steussy.

The proverb “As you teach, you learn” proved to be true again and again. I cannot think of a single pretotyping project, workshop, class, or talk where I did not learn something new or find a way to improve on existing material. So, a huge thank-you to all my students.

In 2011, I invested one week to write and edit (sort of) the short booklet *Pretotype It*, a prototype version of this book, which I then released as a free PDF. I have no idea how many thousands of people have read *Pretotype It*, because over the years it has been shared on countless websites and translated by volunteers into a dozen languages. But the many emails I received from readers all over the world who loved the booklet and urged me to “write a proper book” with more tools and examples gave me the data and motivation I needed to write this book. Thank you all.

Some of my former students, colleagues, and readers took their interest in my work a step further and became master practitioners, evangelists, and teachers of prototyping tools and techniques. Thanks to them, these ideas are now being taught, refined, and practiced in many countries and organizations I could have never reached myself. A few of these invaluable partners in prototyping deserve a special mention. In the US: Jeremy Clark, Patrick Copeland, Rich Cox, Eric Alessandrini, Michael Thomas. In Europe: Leonardo Zangrando, Tim Vang, Dan Cobley, Elalami Lafkhi. In Australia and Asia: Leslie Barry.

I’ve benefited from the help, knowledge, and experience of many, many people over the past few years, and I have a terrible feeling that some of them who fully deserve to be acknowledged here are not. If that’s the case, please know that it’s not due to a lack of gratitude, but to a mortifying fiasco by my memory, on whose behalf I sincerely apologize.

Finally, a loving thank-you to my family and friends. I would not have been able to write *The Right It* without the encouragement and support of the Right Wife, the Right Daughter, the Right Son, the Right Parents, and the Right Friends.

Glossary

Beast of Failure: An imaginary merciless and insatiable creature that devours most new product ideas and bites and slashes those who pursue those ideas without first validating them.

Data beats opinions: A key rule you need to internalize and practice with no exceptions if you want to improve your odds for market success. Don't base your product decisions on opinions. Base them on market data, and not just any old data or other people's data (see OPD)—but your own (see YODA).

DTD (Distance to Data): A metric to help you quantify and minimize how far you have to go to collect your market data.

\$TD (Dollars to Data): A metric to help you quantify and minimize how much it will cost to collect your market data.

expert opinions: *See* opinions.

Failure is not an option: An inspiring but wrong and often harmful phrase and belief suitable for Hollywood movies but ill suited for most business ventures. Use it only if you are writing dialogue for an action flick with Arnold Schwarzenegger.

false negative: An idea that is mocked and knocked in Thoughtland but, when competently executed and brought to market, turns out to be a success.

false positive: An idea that sounds great in Thoughtland, is competently executed and brought to market, and fails miserably. Most market failures are false positives.

HTD (Hours to Data): A metric to help you quantify and minimize how long it will take to collect your market data.

hypozooming: The process of taking an XYZ Hypothesis and “zooming” it in with regard to scope, space, and time to derive a set of smaller related xyz hypotheses that can be tested locally, quickly, and inexpensively. For example, the xyz hypothesis for Second-Day Sushi: “At least 20% of students buying packaged sushi at Coupa Café today at lunch will choose Second-Day Sushi if it’s half the price of regular packaged sushi.” The principle is that if the XYZ Hypothesis is true, then the easily testable hypozoomed xyz hypotheses derived from it will reflect that.

If there's a market, there's a way: An important reminder that if there's enough market interest in an idea for a product, you (or someone else) will usually be able to find a way around whatever engineering, financial, legal, or other obstacles currently stand between the idea and its realization. If an idea is The Right It, one way or another it will usually find its way to market.

If there's no market, there's no way: If there is no market interest in an idea, no amount of brilliant design, amazing engineering, superior reliability, or marketing fireworks will help the idea succeed.

If we build it, they will come: An overly optimistic, ill-founded sentiment not suitable for business ventures, because they *won't* come, unless you are building The Right It. However, by transposing two words and adding a question mark, you get the critical question you should ask before building anything: “If we build it, will they come?” That’s the key question this book helps you answer.

Law of Market Failure: “Most new products will fail in the market, even if competently executed”; the stubborn hard fact that most new product ideas are destined to fail and that competent execution of an idea cannot save it from failure.

market failure: When the actual market result from an investment in a new product is less than or the opposite of the expected result.

market success: When the actual market result from an investment in a new product meets or exceeds the expected result.

MEH (Market Engagement Hypothesis): A high-level description that combines the fundamental premise behind a new product idea with a vision of how the target market will engage with it. For example, the MEH for Second-Day Sushi: “A lot of people who want to eat healthy and who like sushi can’t afford to eat it regularly, because sushi tends to be quite expensive. If we can find a way to make sushi as affordable as other fast food, a lot of fast-food patrons will choose sushi over less healthy alternatives.”

OPD (Other People’s Data): Market data collected by other people at other times, in other places, with other methods, using other filters, and for other purposes. Technically, OPD is data, but since it’s not *your* data, it can be as dangerous and as misleading as opinions. OPD is no substitute for YODA (*see* YODA), and given that OPD is neither necessary nor sufficient for evaluating your ideas, it’s best to not waste time seeking it.

opinions: Subjective, biased, and often baseless judgments about an idea’s prospects for success. In the quest for *The Right It*, opinions are worse than useless—they are downright dangerous and misleading.

pretotype: A specific artifact or technique used in prototyping. Some pretotypes are the Mechanical Turk, the Pinocchio, the Fake Door, the Facade, the YouTube, the One-Night Stand, the Infiltrator, and the Relabel.

prototyping: Using a group of tools and techniques to collect fresh, reliable, and relevant market data (*see* YODA) about an idea for a new product as quickly and as inexpensively as possible. The goal of prototyping is to help you *make sure that you are building The Right It before you build It right*.

Say it with numbers: A reminder to quantify whenever possible. A number, even if it's just an educated guess at the time, is more informative and useful than some fuzzy, vague term. For example, instead of "Our widget will be inexpensive," say, "Our widget will cost \$10," or "Our widget will cost 40% less than the competition's."

skin in the game: Something *of value* given to you by the market as evidence of interest in your idea. The simplest form of skin in the game is money (e.g., a paid preorder or a deposit), but it could also be someone's time, information, reputation, and so on.

Skin-in-the-Game Caliper: A tool to help you quantify and calibrate skin in the game, necessary because not all skin in the game is created equal. For example, a \$1,000 preorder for a product should count more than a \$100 deposit for the same product, and a commitment to attend a one-hour presentation should count more than, say, an email address.

Thoughtland: An imaginary place where ideas for new products hang around collecting solicited and unsolicited opinions. Like visits to Las Vegas, visits to Thoughtland are best kept short: most of what happens in Thoughtland should stay in Thoughtland.

The Right It: An idea for a new product (or service, company, initiative, etc.) that, *if* competently executed, will succeed in the market. The Right It is like Kryptonite to the Beast of Failure.

The Wrong It: An idea for a new product (or service, company, initiative, etc.) that, *even if* competently executed, will fail in the market. The Wrong It is like catnip to the Beast of Failure.

TRI Meter (The Right It Meter): A tool for mapping and visualizing the results of prototyping experiments to help determine how likely it is that an idea is The Right It.

XYZ Hypothesis: What you get when you apply "say it with numbers" to the Market Engagement Hypothesis. The basic form for the

XYZ Hypothesis is: “At least X% of Y will Z,” where X% represents a percentage of your target market, Y, and Z represents how that percentage of the market will engage with your new product idea. For example, the XYZ Hypothesis for Second-Day Sushi: “At least 20% of packaged-sushi eaters will try Second-Day Sushi if it’s half the price of regular packaged sushi.”

xyz hypothesis: A small, specific, easily and quickly testable hypothesis that is derived from, and consistent with, a broader XYZ Hypothesis. For example, a possible xyz hypothesis for Second-Day Sushi is: “At least 20% of students buying packaged sushi at Coupa Café today at lunch will choose Second-Day Sushi if it’s half the price of regular packaged sushi.” The process of going from a broad XYZ Hypothesis to one or more xyz hypotheses is called *hypozooming*.

YODA (Your Own DAta): Data about *your own* product idea, collected firsthand by *you and your own team*, by running experiments *you* designed to validate *your own* market hypotheses. To qualify as such, YODA must come with skin in the game (*see* skin in the game). Unlike OPD, YODA is both necessary and sufficient for evaluating your idea.

