

Masters of Scale Episode Transcript – Eric Ries

[Click here to listen to the full Masters of Scale episode with Eric Ries.](#)

NICK MEANS: They set this tent up next to a plastics factory, and it apparently smelled horrible. And when they answered the phone they weren't allowed to say what they were up to because it was a top-secret project. So one of the engineers on the project, Irv Culver, took to picking up the phone and saying, "Irv here, Skunk Works."

REID HOFFMAN: Our storyteller here is Nick Means, director of engineering at GitHub and a passionate student of aviation history. So it's natural that he'd be fascinated by the famous "Skunk Works," a top-secret project at Lockheed Martin. The story has a mythic status among engineers and entrepreneurs.

MEANS: So Skunk Works came out of Lockheed Martin in the midst of World War II. The Germans had created the Messerschmitt Me 262, the first jet fighter deployed in combat. The U.S. was looking for an answer to it. And we were way behind on jet engine technology.

HOFFMAN: The Germans having the world's first operational jet-powered fighter plane was an urgent escalation in the war. The Allies couldn't afford to lose this particular arms race.

MEANS: The British government offered us the de Havilland H-1B Goblin engine design and said we could use the engine, no charge. We just had to build a plane around it.

HOFFMAN: Sure, no problem. "Just build a plane around it." How hard could that be?

The Air Force handed the task to Lockheed Martin. But Lockheed was already cranking at full capacity for the war effort. They had no factory space. No spare engineers. They were tapped. But then, Lockheed's chief engineer, Kelly Johnson, volunteered to head up the project.

MEANS: He had been asking for an experimental aircraft division. And the Lockheed board largely gave him the project to shut him up, because they thought it would be too difficult to pull off.

He literally set up a circus tent on the grounds of the Lockheed factory in California – because there was no factory space – and mocked this plane up around a plywood mockup of the engine. They didn't even have the engine in house yet.

HOFFMAN: Under this circus tent, next to the rotten-smelling plastics factory:

MEANS: "Irv here, Skunk Works."

HOFFMAN: Johnson hand-picked a team to design in an entirely new way. He put engineers and draftsmen cheek-by-jowl with the fabricators, all under a big tent.

MEANS: Normally when you build a plane, you build it very carefully. You do lots of drawings, you do lots of test fitting. There's a strict change control process. But in this case, Kelly essentially waived all the rules.

He said that his engineers and fabricators were free to fabricate parts on the spot that would fit on the plane, and because of the flexibility of the rules, because of how quickly they moved, they were able to get the prototype built in 143 days, which is a remarkable time to build a jet aircraft.

HOFFMAN: So years before Silicon Valley adopted the concept of a "Minimum Viable Product," the Skunk Works used it as their cardinal rule. The tight feedback loops between designer, engineer, and fabricator allowed an idea to go from pencil sketch to tangible part in a matter of hours.

MEANS: If you worked in the engineering offices, you would probably be called to the production floor multiple times per day to consult on something that the fabricators were seeing as they were building one of these initial prototypes.

HOFFMAN: This feedback loop meant less time drafting blueprints and more time building things, collecting data, and iterating based on results. In aviation, the careful plan means nothing if the plane doesn't fly. And this plane? It flew. And it flew fast.

MEANS: That plane actually was the first American plane to fly 500 miles an hour in level flight. And it actually stayed in use for 40 years.

HOFFMAN: There are so many reasons why this should not have worked – maybe starting with the part where Kelly "waived all the rules." That's something you might expect from an improv teacher but not an aerospace engineer. But this was improv in a way. The Skunk Works team didn't have time for a multi-year plan. They couldn't afford to make any assumptions. They had an urgent question that needed answering. So they experimented. And so should you.

I believe every business is an experiment. For the experiment to succeed, you need to test your hypothesis, measure the results, and follow wherever they lead.

[THEME MUSIC]

HOFFMAN: I'm Reid Hoffman, cofounder of LinkedIn, partner at Greylock, and your host. And I believe that every business is an experiment. For the experiment to succeed, you need to test your hypothesis, measure the results, and follow wherever they lead.

Imagine you're in Pisa, Italy. It's the year 1589. You stand in the slanted shadow of the city's most famous tower. You look up to the top, just in time to see the scientist Galileo drop two balls, a cannonball and a wooden ball, at the same time.

First, get out of the way! Now tell us: which ball hit the ground first? If you remember this story from physics class, you already know the answer. And if you don't, keep listening. Because at the time, no one knew the answer. Even Galileo didn't really know until he ran the experiment.

And Galileo had a lot of proving to do. He was challenging almost 2,000 years of received wisdom with his hypothesis that the balls would land at the same time. You see, the Greek philosopher Aristotle had declared that the speed at which an object fell depended on its weight. A ball twice as heavy as another ball would fall to earth twice as fast. And that sounds right, doesn't it?

But Aristotle never ran an experiment. He never dropped anything from the roof of the Lyceum. He just thought about it – a lot. And he trusted his sense of logic to work out the conclusion.

“A heavy thing falls faster than a light thing” seems to make sense, especially if you compare, say, a feather with an anvil. We know the feather will drift gently to the earth, while the anvil... won't.

But Galileo ran a test. He knew that some heavy things, like an anvil, would fall faster than some light things, like a feather. But a feather and an anvil have a lot of differences besides weight that might affect wind resistance. In his wooden ball vs. cannonball experiment, he took out the variables to test out a very specific hypothesis. And if you don't know: Yes, the two balls hit the ground at the exact same time.

I think about this experiment a lot when I hear stories about new products that fail despite their solid business plans. There are plenty of circumstantial reasons why a launch might fail, like bad timing or just plain bad luck. But often, a product fails because someone made an assumption in their business plan that was simply wrong, as wrong as Aristotle was about cannonballs.

I wanted to talk to my friend Eric Ries about this, because Eric has made a career of applying the scientific method to startups. He's CEO of the Long-Term Stock Exchange, a new securities exchange built to encourage long-term growth. But you might know him as author of *The Lean Startup*, a book I expect many of you have on your shelves.

And I should add: If you'd like to share this episode with a friend, send this link: listen.mastersofscale.com/leanstartup. That's listen.mastersofscale.com/leanstartup.

With *The Lean Startup*, Eric introduced some ideas adapted from manufacturing to bring some new, some might say 'heretical', ideas to entrepreneurship. But as with Galileo, those ideas

didn't just come from Eric's brain. They came from real-life experiments, not all of which were successful.

At 25, he was cofounder and CTO of IMVU, that's I-M-V-U. They built 3D avatars for social networking platforms. And oh, the business plan he wrote.

ERIC RIES: The first business plan I ever wrote for a startup, it was beautiful. It was like 50 pages of just the most eloquent prose, the data in there was sourced from the U.S. census, and all this analysis. It was just like, you would weep to read it. And the small problem, the tiniest little detail, was the customers didn't read it. So they didn't behave the way it said, but the analysis wasn't wrong, the facts were wrong.

HOFFMAN: It's pretty funny to imagine a world in which new customers read the business plans of all their favorite products, to find out how they're supposed to behave. But in a way, every business plan makes that assumption. If I'm a game designer, and my research tells me that men 18-24 are likely to buy my game, I'll base creative decisions on how those 18-24-year-olds are likely to react. But what happens if I'm totally wrong?

Let's go back to Eric and IMVU. Their innovation was to make 3D avatars that people could add to their existing social media profiles without needing to sign up for a new network. Eric had pushed himself to the limit trying to get the code right.

RIES: We had spent six months building this software, which by modern standards is very slow, but at the time it was like 10X better than what we had done before. Still, we felt very fast.

I remember the night before it was going to become live, I was like, "Oh my God, tomorrow some journalist is going to download this software. It's going to crash their computer and the headline in the newspaper the next day, it's going to be, 'Idiots at that company don't know how to build quality software.' Subtitle, 'Never hire that guy again.' Big arrow pointing to my mugshot, and my career was over."

And what was so funny about it in retrospect is, good news, I was actually relieved when we didn't crash a single computer – because nobody downloaded the software. We didn't sell a single copy. Nobody even tried it for free!

HOFFMAN: This failure, while catastrophic in the moment, gave Eric the critical insight to develop his signature theory. His company had made a value proposition so wrong, six months of coding was wasted.

RIES: Could we have found that out a little sooner than six months? If the thing is so wrong – that product, I remember we couldn't even pay customers in a usability test to

use it. They would give us our money back. It was that bad. But if you don't get that feedback, you can't learn.

HOFFMAN: Eric and his team did learn. IMVU got data from those usability tests, the ones that, at first, they couldn't even pay people to complete. They learned what was driving their customers away and pivoted their strategy. In short: Gamers wanted to know what other gamers would find cool before they shared their own avatars. Eventually they got it right – their avatars caught on, as part of a standalone social network – and their company started to scale.

But those lost six months really stuck with Eric. It wasn't just that they got the product wrong. It was how much effort had been wasted building the wrong thing. It's as if he'd spent hours getting ready for a black-tie event – renting a tuxedo, getting a haircut, shining his shoes, the works – only to discover at the last minute that the gala is actually a casual pajama party. It's bad enough to show up overdressed. But even worse, when you're an engineer and efficient by nature, to think of all those hours down the drain.

RIES: Most of us just have this intuitive idea that if something needs to be perfect, then the way to accomplish that is, to be slow, careful, thoughtful. Take long blocks of time to do it. And that can work, so long as you really, really, really know what you're doing.

A master artist at the top of their craft can produce something truly beautiful if given sufficient time and energy. But even the very best, given too much money and too much time, we have seen that go catastrophically wrong too. So it's even that, it's like a risky strategy.

HOFFMAN: Pick your un-favorite movie.

RIES: But for startups in particular, if you don't know who your customer is, you literally don't know what the word "quality" means. So you can't make something higher quality by yourself. It's impossible, because you're wrong about what quality means, therefore you need to get this information.

So the second approach instead of locking yourself in a cave is to say, "I'm going to achieve perfection through iteration. You can make things perfect, but in order to be perfect, you have to pass through imperfect." This is something that artists have known for all of time.

HOFFMAN: This lesson taught Eric the value of something we already talk about a lot on this show: The "Minimum Viable Product." As in: the most bare-bones, least-polished version of your product that can be used to test a hypothesis.

As I often say, "If you aren't embarrassed by your first product release, you've released too late." But the point of releasing ASAP isn't 'speed for speed's sake.' It's to give you data from

your customers as soon as possible – and time to use that data to make improvements. Then build again, and test again, creating a feedback loop. Or as Eric would come to describe it: “Build, Measure, Learn.”

A great example of this is from 2007, when Samsung premiered the Galaxy Note 7. The story goes a little something like this...

SONG:

When the Galaxy Note 7 premiered
The worst thing that happened that Samsung had feared
Was that the phones would explode in a flamey fire
At 29,000 feet they would expire

So Samsung got to work right away
A recall of units and batteries was in place
They worked and they worked through testing muck and mire
And figured out just what caused those blasted fires

Batteries overheating
In the casing
Short circuit

When you're standing at an industry precipice
You need success with root cause analysis
Our listeners are smart so they won't be pressed
They know before you launch you have to test (test...test...)

Well Samsung changed the batteries
And every process of safety
First in its own factories
Which then reflected totally

Across the entire industry
So what's the wisdom that we earn?
To build (build measure and learn)

HOFFMAN: This idea would come to shape how Eric approached... everything.

RIES: I didn't know that that would turn into a philosophy of business. It was just a practical set of problems to avoid what had happened to me over and over again: you build a product that customers don't want and then you're sad about it, even though you made it perfect. And that just means you waited too long.

And you know, this is how the scientific method developed over centuries. We're not breaking any really new ground here, but just to apply those lessons to business.

HOFFMAN: Eric's got a point. The scientific method starts with observing the world around you. In business, this is where you identify a gap in the market or a common need. Then you pose a question. In our case: "Will my business or product address this need?"

Construct a hypothesis. This stage is, in essence, your business plan. It's going to have some assumptions in it, but that's OK. Because now it's time to test. Run experiments. Limit the variables. See if your hypothesis holds up. This is the part you can't fake. You can't run your experiment using old census data or generic market research. You need empirical data from actual users. If you're in tech, that means shipping code to your users as soon as you can.

Eric took this thinking to his engineers at IMVU. They started shipping code more frequently – sometimes multiple times a day – to get product updates in users' hands. Then his team would study the data on how users responded. The distance from hypothesis to experiment to results got shorter and shorter. His company started to scale. But one thing nagged at him.

RIES: I'm now hiring a bunch of people, and now I had this problem that we did things in this very unusual way that clearly, unambiguously worked, but no one can understand why. And it drove everybody crazy. It drove our employees crazy. It drove our investors crazy.

I converted like person after person to this new way of working. And they would still be like, "Okay, I see that it works, but why?" And I was like, really desperate to be able to answer that question, because it's very frustrating for the people that work for you to be doing something and not know why.

HOFFMAN: At this point Eric does something that takes him from startup founder to movement leader. He takes these ideas – about a new, more scientific method of feedback loops and frequent code-shipping – he builds them out, and he writes them down.

RIES: That requires a little bit of formalism from this vague concept of feedback to the more precise concept of experimentation. I started reading like crazy, everything I could get my hands on related to management, startups – and I mean really, I would read anything, if someone said it was halfway useful.

HOFFMAN: Eric pulled from a wide variety of sources: from management theorists and military strategists, to Toyota's "Lean Manufacturing" system, which identifies and eliminates waste at every step of the process. Then he applied these concepts to software development, the way a scientist might observe a drug's effect on one species and apply it to another.

RIES: I stumbled on lean manufacturing as a, like, concept and intellectual vocabulary. And I started using those concepts with my team. It was by no means the first thing I tried, but it was a metaphor that could more or less work.

HOFFMAN: After nearly five years of working with his team, converting them one by one to his developing theory, this approach finally took hold. With the company now up and running smoothly, Eric left IMVU. He headed for the VC world, as an advisor to other startups.

RIES: I started getting asked by these VCs: "I won't invest in your company, would you come work with some of my companies in my portfolio, and help them go faster?" Because I had this weird reputation that I could magically make engineers be more productive. I think a lot of people were just like, "He just somehow makes them work harder."

HOFFMAN: It wasn't magic, of course, it was science. It was the feedback loop created by "build, measure, learn" that was making the code ship faster. But this was over 15 years ago. And these concepts – now startup canon – were then heresy.

RIES: And I'd be like, "No, no, no, there's no magic fairy dust. I don't have any superpowers. This is just a system, it's a method for organizing your work in a more productive way."

And they'd be like, "Okay kid. I'm sure that's great, but could you please bring the fairy dust with you when you meet with my team?"

I'd go to these meetings – and you're laughing because you already know the answer. These meetings were a disaster.

HOFFMAN: Any meeting that starts with a client expecting Tinkerbell is probably destined to fail. But in Eric's case, the firms hiring him not only expected magical answers, they also flat-out rejected the real one.

RIES: The fundamental problem with these meetings is I would say, "Listen, here's how it worked for me. We would ship software to production 50 times a day on average." And people would say, "That's impossible." And I'd be like, "Well, that's not a theory. I haven't got to the theory yet. I'm just establishing the facts of what I witnessed with my own eyes." And they'd be like, "That's not true. That can't be done."

They would get really angry. These meetings ended badly, and I would get yelled at, and I often would have to say, "With all respect, you called me for this meeting, you asked me to do this as a favor and now you're mad at me. Just don't call if you didn't want to hear!"

HOFFMAN: If this seems like a harsh reaction, remember: Radical new ideas are not always instantly welcomed. Think of our old friend Galileo: the more he challenged the wisdom of Aristotle, not to mention the Catholic church, the less warmly his experiments were received. Thirty years after the ball drop experiment, Galileo was excommunicated and put under house arrest until his death for saying that the Earth moves around the sun.

Eric wasn't exiled for his ideas, but he was certainly tired of being yelled at. He considered a new experiment.

RIES: So I had this genius idea that as an emotional defense mechanism, I would write a blog post or two, writing some of these stories down. And then the next time someone called me for a meeting, I would first say, "Hey, why don't you read this blog post, and if you think I'm crazy, then maybe we don't have to have the meeting – and please don't yell at me." Honestly, I really did not like that experience.

Just so people understand the difference – the entrepreneurial environment has changed so much in such a short time. This is now 2008, '09, and nobody blogged. Startup people didn't blog, founders didn't blog. It was considered a distraction from your job.

I was so embarrassed about it, I did it anonymously. I didn't even put my name on it, because it was so disreputable. It was really not an acceptable thing to do. All these VCs were like, "Don't do that." So I did it anonymously, it was called Startup Lessons Learned – in the passive voice. The lessons learned themselves, not by anybody.

I called it *Lean Startup*, because I wanted to make an homage to lean manufacturing and draw that connection. The financial crisis was going on, so being lean, that vaguely seemed good to people. I had this just absolute certainty that someone had already written this book and I just hadn't read it.

HOFFMAN: But no one else had written it. And soon Eric's anonymous blogs became the basis for his best-selling book: *The Lean Startup*.

RIES: So the thing with *Lean Startup*, it really forces you to become rigorous and specific about what your belief is. Like, what is your theory of change? What do you actually think is going to work? And can you articulate it on a per-customer, per-human-being level?

I was like, "Okay, well, what if we thought about it as build, measure, learn? And we have this feedback loop? And what about minimum viable product? And of course, most importantly, the pivot – a change in strategy without a change in vision?" And all those pieces started to come together.

HOFFMAN: Eric's ideas came together at exactly the right time. In the wake of the 2008 financial crisis, entrepreneurs needed to achieve lift-off without huge amounts of capital or years of development time. Published in 2011, the book has sold more than a million copies. Eric's experiment was paying off.

And one of *Lean Startup's* most important ideas was the one Eric just alluded to: Once you collect data, how do you use it to improve? In the scientific method, this is when you assess the results of your experiment. Did it bear out your hypothesis? Or do you need to make adjustments? In the business world, those adjustments might be as small as adding a feature or as large as altering your entire strategy – while staying true to your vision. In other words, you might need to... pivot.

HOFFMAN: I think to my ear, you were the first person to use the word "pivot," which is now common diction in Silicon Valley and in general entrepreneurship.

RIES: Yeah, sorry about that.

HOFFMAN: No, that's good. We're not pivoting away from the question on pivot.

HOFFMAN: Speaking of the pivot, we are going to pivot ourselves to a word from our sponsor. When we return, we'll pick up this conversation, on how Eric invented the word, "pivot."

[AD BREAK]

HOFFMAN: We're back with Eric Ries, and we were about to dive into the question of how and when to pivot. It's a move that works best when it's motivated by evidence and data. Since Eric is widely recognized as having coined the term, of course I had to ask him about it.

HOFFMAN: So say a little bit about what the difference between changing strategy and keeping vision is, and maybe a little bit of the taxonomy of what are different kinds of pivots, and when to consider them.

RIES: In the history of tech, the pivots, there's only a few kinds, and some of them are real classics. Basically if you take every element of a business plan, you can pivot one of the major elements. "I thought there's going to be a consumer product, oops, it's for enterprise." That's a classic. Or, "I thought this would be B2B, oh no, it's B2C. I thought I would work for subscriptions. No, it's freemium."

HOFFMAN: But let's back up a moment – why this word? And why did it catch on so pervasively?

RIES: I didn't expect this to be like such a big deal in the world when I started talking about it, truly, but it's actually – it's just a necessary concept to talk coherently about

startups at all. In fact, I once had the honor, someone asked me to read someone's manuscript of a memoir that someone from the old – one of the OGs in Silicon Valley had written. It had never been published, but he had written about his experiences with some of the very early big tech startups. And he's trying to explain this phenomenon without the vocabulary for it. And it requires such convoluted language. He's trying to be like, "Okay, so these great entrepreneurs, they're always completely true to their vision. But they're super-flexible about how the vision is realized, but they wouldn't compromise on the essential things."

And it's just like four pages of trying to explain it. I'm like, he's missing a concept. We didn't have a word for it. And it makes being an entrepreneur so much harder, because when someone comes into your office and says, "I think we should just abandon the mission altogether for no reason. What do you think, boss?" You've got to be like, "No, we've got to be true to the mission no matter what."

But then that person walks out of there, and they're like, "Okay, if I ever have bad news, I'd better not take it to that guy." Right? He's like, "Mission first," and so that's like, "No, no, no, no. Actually no. It's not what I meant. If you do have any bad news, do tell me what it is, but don't be distracted from the mission." It's just like this very hard concept, it's like a total pivot: a change of strategy without a change of vision.

HOFFMAN: Eric didn't invent the pivot, any more than Galileo invented gravity. What he did was put a simple name to an elusive idea. And that helped entrepreneurs recognize and interpret the facts in front of them.

Scientists do this all the time: Think of "The Big Bang Theory" or "The Theory of Relativity." Giving names to big ideas accelerates them and makes them feel less threatening, which is critical to the scientific process. It also gives you a common language. A shorthand for referencing a complex set of assumptions.

In the case of the "pivot," it's a set of assumptions about changing strategy without changing your mission. Eric has a useful analogy for identifying when it's time to pivot.

RIES: You get in your car, you have a GPS. GPS is a robot, it's not smart. You cannot ask it, "Where should I go today?" It doesn't know. You have to tell it where you want to go. So I would like to drive from California to New York City, and it will be like, "Yes sir. Here's the best route to get there."

You start driving, and the road is closed. You're going to be like, "Oh, the road is closed. I'm going home, road trip over." But your GPS can help you. This is what it's for. It's gonna be like, "Hey, road closed." "Well, what other roads are there?" It's like, "Well, you could take this other road." You say, "Oh, let's go check that one out." And you try that one and that one.

And listen, now maybe you make so many detours. You're like, "Wait, I'm back in San Francisco, and I've really blown it." So like GPS is not in charge. You've got to be in charge. You're the one with the vision. But the data, the techniques, MVPs, split-testing, all that stuff can help you try to calibrate.

HOFFMAN: I really like the idea of the pivot as calibration. You don't need to wait until your company is on the edge of catastrophe. If you do, that pivot can turn into a wild swing, based on reactionary feelings instead of facts. But if you gather your facts and act on them the moment you can process the new data, a pivot can feel quite organic. Not a lurch, but a tweak.

Because the reality is, it can be hard to interpret data in real time. Try as we might, it isn't always possible to control for every variable. Real life is sometimes messier than a lab. It's the Zen koan of startups.

RIES: You'd never meet an entrepreneur who's like, "Boy, I wish I'd waited longer to pivot." Everyone always wishes they had done it sooner, but why don't we do it? The hard part about a pivot is not figuring out what to do next. Actually, most teams that I've worked with over the years, that's the easy – actually, they secretly had this idea, and they're like, everyone at the team's afraid to say it out loud, but they're like, "Do you think we should do enterprise? Is it okay to say?" The hard part is establishing the facts of what's happening right now: Is the strategy working? Is it taking us closer to our vision? Are we actually getting closer to New York or are we still in San Francisco?

HOFFMAN: So how would you build on this piece of advice that I usually give people when thinking to pivot is to say, just like *Lean Startup*, you have your hypothesis, you have your theory of the case about why your product, your service, your go-to-market strategy will work. And as you're encountering and it's not working, and you're trying new things, you try to track what the rough value of your ideas are. And if the value of your ideas is going down, e.g, you have less confidence in your new ideas than you had in your old ideas, that's when to seriously think about pivoting, right?

RIES: Yeah, that's exactly it. In *Lean Startup*, we have a mathematical discipline called innovation accounting. We've tried to formalize this way of evaluating the net present value at different experiments to figure out, during the flat part of the hockey stick, what's objectively more valuable than others?

You're running these experiments, and sometimes – every time you put an incremental effort, you get incremental results, and it's like, it works. And then you have these other times when the experiments are just not very productive. You're having to work harder and harder and harder to squeeze a little bit more optimization out of a lemon.

HOFFMAN: This idea of innovation accounting can get deeply mathematical very quickly, so let me summarize it this way. In the midst of your experimenting, and pivoting, and iterating, and learning, it's hard to know where you are on the growth chart. Is your revenue chart about to look like a hockey stick? Or more like an asymptote, a line that will approach a curve but never cross it?

If you've not yet reached your own measure of success – and if your experiments improve your product by smaller and smaller increments? You may never reach the threshold of success. The second you recognize this, it's time to pivot.

RIES: If you can just establish ground truth, facts about what's happening – the asymptotic growth, that becomes really obvious, and then you just say, "We are willing to really exhaustively try everything we can think of. So anyone got any ideas?"

And someone's like, "Let's make the button blue." It's like, "We already changed the button color 15 times. Okay, that's not going to get it done." They say, "Well, we just need to explain it better. Tell the comms guy to explain it better." It's like, "Well, we already explained it every possible way that we can. Is it possible that this is just not the right path, this is a dead end?"

And just being able to say that question out loud, and have people look at the data and be like, "Yeah, you're right." The sooner you can get to that moment, the better.

HOFFMAN: A lot of entrepreneurs have taken Eric's advice since *The Lean Startup* was first published. And Eric's written two subsequent books that show how companies of all sizes can take the science experiment approach. Build fast with data and facts. No fairy dust. No magic wands.

But speaking of magic wands, before we go on, we couldn't help taking a brief detour to a magic wand that demonstrates perfectly how iteration and experimentation can continue even as you scale.

PIERRE BOHANNA: For a stick, it's a very complicated stick.

HOFFMAN: That's Pierre Bohanna, a prop maker for feature films. He's worked on dozens of blockbusters, from "Titanic" to "Star Wars" films to "The Dark Knight." But there's a different, magical film we wanted to ask him about....

BOHANNA: What we do is, we make hero stuff like wands and brooms and Quidditch equipment right through to Dumbledore's office with the pictures on the wall.

HOFFMAN: That's right. We're talking to the man who made Harry Potter's wand.

BOHANNA: Wands are interesting because they are very reflective of the character that they're built for. With Harry's wand, it starts from the handle, it's a loose twig really. It's a very rough-barked twig. Then it becomes roughly hewn and then the tip is very refined. It shows someone growing from an innocent child and someone who knows nothing, to the final, to the important end of the tip where it's perfectly refined.

HOFFMAN: To create the very first onscreen version of Harry Potter's wand, Pierre and his team had to experiment. They built prototypes to test their ideas.

BOHANNA: In the first film, we made a range of sort of loose-styled wands from sticks we found in the woods of Leavesden with crystals tied on them. Right through to very modern, sort of, hewn hardwoods to periods, influence pieces. And they went for these very simple sort of high-quality wood, sort of precious woods, polished ebonies and things like that.

HOFFMAN: In the world of design, you can't always measure results in numbers. When you're creating the most iconic prop for the most iconic wizard in a generation, you have to be ready to experiment.

BOHANNA: Often part of the process of developing something is getting it wrong before you get it right. So often you have to make samples, and make samples, and make samples.

HOFFMAN: We talk about iteration often in tech, but iterative cycles happen everywhere, even the land of make-believe.

BOHANNA: Harry's wand actually, in the first two films, is half ebony and half Mexican Rosewood, but in the third film, a new director came along, Alfonso Cuarón, and he changed a lot of the visuals. He basically remade Harry's wand. Alfonso is an incredibly visual director, and he was really thinking in that way, so it was an interesting sort of slight shift.

HOFFMAN: This slight 'shift' in wand design did not go unnoticed by the extremely attentive Potterverse. You can find articles on many fan sites about the wand iteration under director Alfonso Cuarón. Wand 1.0 was smooth and simple, while Wand 2.0 has ornate carvings along the handle. From the third film till the end of the series, the art department had freer rein with all of the characters' wands, and they experimented with bone, crystal, metals.

BOHANNA: I love going back to wands and to the way of styling, it's still a very human aspect to feel how things look just for the pleasure of how they look. And that's not a magical thing, that's a very human thing.

HOFFMAN: Alright, that's enough magic for one day. As we come back to our featured wizard, Eric Ries, we're going to pivot to something rare: An idea in his book, *The Lean Startup*, that no one else seemed interested in.

RIES: I was literally on a plane flying from one *Lean Startup* workshop to another, and I was like, "Boy, wouldn't it be great if really long-term oriented companies and really long-term oriented investors could get together and support each other for mutual benefit? Almost like a new social contract?" And I was like, how would you accomplish that? And I'm sitting there being like, what we need from first principles is an institution with the regulatory power to bind companies and investors at the same time. And I was like, "Oh, a stock exchange."

HOFFMAN: It might sound like something of a leap from fast-paced iteration to a stock exchange that fosters long-termism. But to Eric, this wasn't a leap. It wasn't even a change.

RIES: In retrospect, the Long-Term Stock Exchange is a completely natural extension of the work I was doing with *Lean Startup*, so much so that I don't even really recognize when it shifted or when it happened, it was just this completely natural thing to me.

HOFFMAN: Same mission, evolution of strategy?

RIES: Yeah. That's really how it felt as it was happening.

HOFFMAN: Since *Lean Startup* had been inspired in part by the "lean manufacturing" system at Toyota, it's natural that Eric had that company on the brain.

RIES: I was studying the history of management very extensively. And I spent a lot of time on Toyota, and the research on what makes great companies truly great. If you've ever looked at this academic research, it is overwhelmingly clear that having a philosophy of long-term thinking is a prerequisite for doing anything great.

So, when I would do my workshops, I would always get this question, "Okay, hotshot, if being long-term is so important, why exactly are you telling us to build venture-backed companies and take them public, into the shortest-term governance environment on the planet?"

HOFFMAN: Great question, anonymous workshop attendee. When a company goes public, it opts into a system that relies on the markets to navigate the direction of the company. It's supposed to work in a feedback loop, not unlike the feedback loop in *Lean Startup*.

If a company makes a move investors see as positive – that's reflected in the markets. Confidence in the company rises, more people buy in, stock price goes up. When they stumble, people sell, stock prices fall, and the company sees they've made a mistake.

But: in the real world, stock prices rise and fall for all sorts of reasons – and not all of those reasons are due to company strategy. Specifically, Eric started thinking about decisions companies have made that might hurt their stock in the short term, but be much better for them in the long run.

RIES: The famous example of Johnson & Johnson and the Tylenol recall – it's still taught as a case in business schools – there was the idea that some of their products could have been tainted.

HOFFMAN: If you're too young to remember, or you never went to business school, in 1982, seven people died as a result of someone contaminating Tylenol bottles with cyanide. Panic swept the country. And even though the contamination turned out to have happened far outside of the manufacturing process, the company was still terrified its name would be sullied forever.

RIES: Just in case, they did a nationwide recall of that product. It was incredibly expensive, people thought they were crazy, thought this was a huge overreaction. But in terms of winning the public's trust, they said, "No, this is what matters, more than the bottom line." But actually, it was very good for the bottom line to do right by your customers, because you're trying to establish a position of trust with them.

HOFFMAN: Nearly 40 years later, Tylenol is still going strong. It's clear now that their decision was correct – not just morally, but totally. But back in 1982, if you were only looking at their stock ticker at the time of the recall, you might be calling them crazy, too. Remember: you can't just look at the results of a single experiment to judge what's good for your business. You have to look at a series over time. And that's really hard to do in our current, quarter-to-quarter environment.

What was becoming clear to Eric was that companies and their executive boards won't just automatically take the long-term view. There should be a way to incentivize corporations to make decisions based on more than just the next quarter's results.

Hence: the Long Term Stock Exchange, or LTSE, whose listing rules divide investors into two different groups: the Citizens and the Tourists. We'll let Eric explain how it works.

RIES: We call the long-term investors the citizens of the republic. If you think of a corporation as a corporate republic, a nation-state unto itself.

Our listing standards for LTSE benefit the citizens of the republic, they give them the primary role in partnership with the company, and we insist that the companies reward them for that long-term partnership.

HOFFMAN: In other words, the people making decisions for the company are the ones who are in it for the long haul. On the LTSE, short-term investors, or “tourists,” are allowed; they’re just not given the same voting rights as the “citizens,” or long-term investors.

RIES: So of course, the citizens love that, but the tourists like it too. If you go to a tourist, you're like, "Hey" – a real-world tourist that comes to visit San Francisco, who is like, "Oh, I love looking at the Golden Gate Bridge." "Hey, we're going to reform San Francisco's voting laws to have ranked-choice voting instead."

They're going to be like, "I don't care. Are you going to make it cheaper to go across the Golden Gate Bridge? Are you going to make it cheaper to stay here? Can you help me get an Airbnb?" That's what they care about.

They don't care about how San Francisco, the city, is governed. That's not really their thing. And the same thing is true in finance.

HOFFMAN: This experiment Eric's running is on a different trajectory than, say, an avatar company. The company has been live for about five years, with their first money raised in 2016. And in full disclosure, one of their early investors was me.

RIES: Yeah, it's been a wild ride, let me tell you. The company itself is five years old, and we first raised money in 2016, so you get a sense of the scale of it. We got the regulatory approval last year in 2019 to operate the exchange. The way you should think about that is it's like we acquired the world's most expensive taxi medallion. It gives us permission to drive the car, but we don't actually have the car yet.

HOFFMAN: But, with such a long-term trajectory, doesn't that make the Long Term Stock Exchange vulnerable to the same problems we talked about at the beginning of the show – a long-term project that can't be unveiled until years and years of work? How do you use *Lean Startup* strategy to build something so complex and tightly regulated? Is it even possible to make a minimum viable version of an entire stock exchange?

Remember the rule: Build, measure, learn. Eric is still doing that. While one part of his organization worked behind the scenes to pass SEC compliance, another part got to work. Eric returned to his developer roots, and led teams to create dozens of online software tools for companies that want to list on the exchange. The tools are mostly free, and they help provide startups with empirical data that will help them scale.

They have:

- A tool for equity management planning and ownership accounting;
- A head-count planner to structure fair compensation;
- A financial planner to manage your cash at the startup phase; and more.

Meanwhile, these software tools provide Eric with a sense of what his target audience wants. It gives him data that will make LTSE better before its first virtual bell even rings. Just like his very first company to scale, Eric is ready to gather the facts and follow wherever they lead.

Every business – even a new stock exchange – is an experiment. So make your hypothesis, test it early and often, measure the results, and take them to heart. The sooner you leave your assumptions behind, the faster and farther you'll go.

If you've enjoyed this episode and want to share it with a friend, an easy way to share it is by sending this link: listen.mastersofscale.com/leanstartup. That's listen.mastersofscale.com/leanstartup. This link will go right to the episode and will open the episode in whatever podcast app your friend uses.

I'm Reid Hoffman. Thanks for listening.