

[SQUEAKING]

[RUSTLING]

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JOE HADZIMA: We come to our final session-- the last part of the final session. Marina Hatsopoulos, who's going to be presenting next, turned to me and said, I've got some of the same things that Bob had on his slides, and I guess I could skip over them. And I turned to her, and I said, I should let the people know, we don't coordinate what the speakers say.

And the fact is, you'll see common themes coming from a variety of people. And maybe that means there's something there that you ought to pay attention to. And in fact, it was only about 15 years into the course that we got all the presenters together for a dinner. And it was an interesting evening. And they started to share stories. And they were very consistent themes. So it's well orchestrated, but not scripted.

So with that, Marina Hatsopoulos, who I first met in the 3D-printing world a number of years ago, and I think she'll tell you about that. So I'll turn it over to Marina.

MARINA Great.

HATSOPoulos:

JOE HADZIMA: Yeah. Thank you.

MARINA Thank you. Hi, everyone. OK, so we're getting to the end of this, and I'm sure everyone's kind of tired. Anyone out
HATSOPoulos: there in Zoom land, if you want to open your cameras, we'd love to see you. For all of you, please-- this is much more interesting. If it's a dialogue, if it's engaging. So just raise your hand anytime, and I will start.

So my dream was to do a tech startup. I studied math and music at Brown. Those were my two passions. But I still had this-- I had this idea. My dad was a tech entrepreneur. So I decided I would try business and see how it went.

So I went to Chase and worked in banking for a few years. And that was actually a great experience. I learned how to analyze a business. I learned accounting. I learned negotiation. And I learned selling. And I actually think those three skills-- accounting, selling, and negotiating-- are three skills that are super useful if you pursue entrepreneurship, but, really, if you pursue any kind of a business career.

At that point, I loved being in New York. It was a lot of fun, but I wanted to get closer to actually producing a real product. What does a real producing company do? So I went to work at Thermo Electron in Mergers and Acquisitions. And that was a rude awakening because Thermo-- it's now Thermo Fisher-- is a technology business. And when we were looking at making acquisitions-- and this was for some of their small divisions-- it wasn't making an assessment of their financials, but making an assessment of their technologies. And I realized that I had a gaping hole in my background in terms of technology.

So I came to MIT, and I got my master's in Mechanical Engineering. And my only goal in doing that was not to ever be an engineer, but to manage engineers and communicate, to be able to speak the language. And it really opened my eyes to this world of science and engineering and opened an intellectual curiosity in me that has stayed with me to this day. It was a great experience.

But I needed an idea. And I actually spent a year looking for an idea or for a failing business to turn around and didn't know what to do. I finally went back to my thesis advisor, Woodie Flowers, and he led me to the TLO. So the Technology Licensing Office at MIT pays to patent inventions that are made by the students and the professors, and then they look for people and companies to commercialize those technologies and take them to market.

And that was really exciting because after looking at a lot of failing businesses that were failing because they had no product differentiation and they had to compete on price, suddenly I saw these really cool, patented technologies that they were looking for someone to commercialize. And one of them was in 3D printing. And this is back before 3D printing was a term. So nobody knew what 3D printing was.

There was a project going on at MIT led by Professor Ely Sachs. He had a team of 30 students, and they were looking to make a 3D printer that could go into outer space to make replacement parts for the stations that are out there. And it was a very ambitious project. And basically, they were designing their own print heads to be able to fabricate parts out of metal.

And there were two guys working on the project that were very frustrated designing these print heads. And they turned the technical problem on its head. And they said, instead of designing a print head around a given material system, let's design a material system around an off-the-shelf inkjet print head.

So they went to Staples, and they bought a Canon paper printer, and they butchered it. And they said, OK, what sticks together with basically ink or water? And they decided sugar and cornstarch, because if you've ever left your cereal bowl overnight, it turns hard as a rock.

So they built a prototype. And the folks at MIT didn't know what to do with it. And so I met these two engineers. They said, we're two engineers looking for a boss. I said, we're going to get along great. This is perfect. And that's how we started.

So we founded the business in 1994, in Kendall Square. We set a goal of delivering our first product within two years, which we did. And then we reached profitability two years after that.

In 2001, we were named the fastest-growing business in New England. We had no venture capital. And I make it sound like, oh, we did all this with no venture capital because we're so smart. No, we did it because nobody would invest in us. So that's just the hard reality of building a hardware business when the internet is taking off in the early '90s. Over 10 years, we grew to \$30 million in revenues with 125 employees. And we were one of the early market leaders with number 2 in market share. So I'm going to share a lot of stories from my Z Corp experience with you during the course of this talk.

We exited after 10 years. I'm assuming that everyone in this room is looking at entrepreneurship from a deep-tech perspective. So everything I'm going to tell you really relates to developing deep tech. Deep tech takes time. And so you should count on 10 years. That's a reasonable time frame.

We ended up selling the business. So I have four kids. But at the time, they were little, and I became really burnt out. I had twin girls in October, and we shipped our first product in December two months later. So it was just total chaos. I had the twins in the office for the first year of their life until they learn to reboot the computers, and then we had to kick them out.

So I got burnt out. We sold the business. Since that time, I've been a corporate director, an angel investor, an advisor to startups and then increasingly, over the last decade, have gotten involved with the startup ecosystem coming out of Greece. So they were decimated by the economic crisis, but they have really smart engineers. They saw what was happening around the world and particularly in the US, and they started making startups. And now it's really taken off. It's really exciting to see.

And I just got back literally earlier this week from Milan, where I was lecturing at the Polytechnic of Milan and at Bocconi about entrepreneurship. So there's a growing interest in entrepreneurship in these tech hubs around the world where engineers are looking at not just pursuing research as a career or academia as a career, but taking their research and commercializing it and making it into a product. I am also a writer. So I write business articles, short stories, personal essays. And you can see them at windystreet.com.

So Bob did a great job talking about this. Why would you create a startup? You might have a passion for particular technology. I mean, that's what I see a lot of at MIT, is people are working on technology, and they just want to continue working on it. It could be a desire to solve a problem, any problem.

That was part of my desire, is I wanted to make the world a little better place, but I didn't have any specific goal. But sometimes somebody has a specific problem they want to solve. So I've invested in a startup in Greece where the entrepreneur is a brain surgeon. His mother died of brain cancer, and so he has a technology that can help diagnose brain tumors.

But it can also be independence. You want to be your own boss. You want the sense of teamwork and community, or you want to make money. And there's no right answer, no wrong answer. But it's important that you articulate your goals because, otherwise, you won't know if you're successful or not.

So for example, the guy that I mentioned who wants to help diagnose brain cancer-- well, the finances of his business are not the number-one criteria. As long as he can get a product out to market, be solving people's problems, he will feel successful. And hopefully, the finances will follow. But if your goal is to make money, then you're going to build a business a different way. And so it's important you know what that is.

When you're doing a deep-tech startup, it really all should start with intellectual property. So you know about patents. Patents are a great deal. The deal is you're going to share best practice about how to do something. And in exchange, you get 20 years of a monopoly on that technology. That's just a phenomenal deal, and it blocks competition.

And I'll tell you a story. In our company, one of our early customers was a very large, well-known name brand I'm not going to mention it. And we had heard rumors that they had taken apart our machine. And we got nervous that they were going to compete against us, and we would have no chance because this is a huge company. Well, they didn't until the MIT patents expired. And then they came to market with a competitive product.

So that made a huge difference. For us to have that protection over that period of time allowed us to invest the money. It takes time to create an innovative market. The more innovative what your product is, the longer it takes to educate the market and to grow the market. And so that allowed us the time to do that.

But you should also keep in mind and keep a hold of your trade secrets and your know-how, and be thoughtful about what those are, and make sure you are careful to protect that. And when people say they're worried about sharing with the outside world, that someone's going to steal their ideas, that's up to you to make sure you're protecting what is most secret and make sure your employees know it as well.

The next thing is publications. Now, a lot of times, if you're in academia, if you're a graduate student or a professor, you're going to be incentivized to publish. And publishing is great, and it gives you credibility and all of that. It's wonderful. But you need to first patent and then publish. The order of events is extremely important. Once you publish, you can no longer patent. So you want that protection from patents. And therefore, you need to patent first before you publish.

This picture here shows the proof of principle that was at the lab at MIT when I met the two engineers. This is an early prototype. What it proved was the fundamental science could work, because if someone had said, I'm going to take a paper printer and prove that it can build a 3D part, you'd say, you're crazy. There's no way that's going to work. And these two incredibly creative engineers proved that it could work. And so it's taking out that core technical risk.

This particular machine required people to stand over and pour powder. It had two or three computers. Obviously, we knew we could make improvements to that. But it proved the fundamental concept, and that's the important thing.

One mistake is part-time entrepreneurship. So this question came up earlier with Bob. And I want to be clear. I think Bob made a very good point that you need to validate before you're going to jump ship and give up your job and your salary and all that. But once you've validated and you know you want to go forward, time is of the essence. And you can't do this part-time. You can't do it on weekends.

I meet professors who say, yeah, I've got a startup on the side. And they do three hours on Saturday morning. That's just not going to get you anywhere. And so our entrepreneurship is not a part-time job.

Researchers need entrepreneurs, and entrepreneurs need researchers. So if you are a technical person and you're not that interested in the business side or you don't know what you're doing, you can pair up with somebody who's on the business side. And that makes a very powerful combination.

A strong name is important. This seems like a real detail. But actually, it's something that you should take a moment to think about when you start your business because it's hard to change later. You want to create a brand that you can use to grow a business. And you want to avoid legal issues. [COUGHS] Excuse me.

So instead of choosing a poor name, what you want to do is, number 1, do a website search. I invested in a startup where the name-- we spent so long trying to find a name. We found a great name. Then, it turned out, the website was not available. And so that just becomes a huge nightmare. You invest all this money, and then it's not there.

Another is you want it to be easy to spell and easy to type. I see with a lot of startups in Greece, Greeks love their language. It's a beautiful language. But a lot of times, they'll use long, beautiful Greek words that nobody else can spell. You don't want to do that.

You don't want a name that is so generic that if you do a Google search, a million other things come up other than your company. You also need to check that there's no trademark infringement. That's very easy. You can just go online and do a search. You want to make sure the name is legally available. That's also very easy to do.

But one last thing is you don't want it to be limiting for the future. So I'm an advisor for a conference that was called the Nantucket Conference. And this coming year, it's going to be moved to Chatham on Cape Cod. So the name becomes an issue because it's confusing if you say it's the Nantucket Conference based in Chatham. And so ideally, you pick a name that can grow with the business and move with you so that you can leverage off of the brand that you have built over many years.

One thing that entrepreneurs often are worried about is staying stealth. And I meet a lot of entrepreneurs. They don't want to share any of their ideas. They're worried someone's going to steal their idea. They might even not want too much customer input because they have a very strong idea in their head, and they don't want to get confused, or they don't want to get a lot of different opinions.

And also, it's really hard to find customers. Where do you find them? All of that. This is a bad approach.

Instead, what you want to do is get feedback. You have so much more to gain than to lose by getting feedback from the market, by talking to your customers. Keep in mind, if all you have is an idea, as soon as you hit the market, anybody else can copy it. So you should have more than just this idea.

But you can share the idea. You can share your concept-- what is the problem you're solving-- without giving away your secrets. So ideally, you have some trade secrets, some know-how that you will keep to yourself. Your customers don't need to know that. They don't care. They really, truly do not care about the inner workings of the product that you're building. They just care, do you have a solution to their problem?

So you want to find out, is the problem painful enough? How big is the market? And is it worth going into?

And one thing you'll see is that startup opportunities come when there's a disruption in the market. Disruptions might be regulations. It might be demographic changes. It might be new enabling technologies. So these are changes that come about that now, as a startup, you have an opportunity to go in there and do something new.

But beware of transient disruptions. So for example, I invested in a startup that was really leveraging off corporate investments in DEI initiatives. And that has just all gone by the wayside. And the company has gone under because those investments have gone away.

COVID-- I saw so many startups that were solving the COVID problem, various aspects of it. And where are any of those companies today? When you're looking at disruptions, you want to think about, how long is this disruption going to last? Because you don't want to invest in something that isn't going to be around in a couple of years.

But the reason disruptions are an opportunity for a startup is because startups can pivot. And I want to make sure you understand this, that large companies have every advantage over you. They have manufacturing. They have R&D. They have sales. They have marketing. They have distribution. They have money. They have experience. They know the market. They have relationships with the customers. They have brands.

They have absolutely everything except the ability to pivot. They are large ships that cannot steer quickly. That is why you always want to look for opportunities where that ability to pivot will put you in an advantageous position. That is your startup edge.

And startups are always pivoting. That's not a one-time thing. The more innovative your market is, the more unknowns there are. There's no playbook to follow, just like Bob was saying. And so you're always making decisions with incomplete information. And that's why, when you're in a startup, you often feel like it's a big fog because you're making these decisions, and you have no idea because you don't have enough information.

It's not like when you're given a nice problem set, and they give you, OK, the train is going at this velocity and blah, blah, blah. And you're given all that information that you need. No, you don't have the information you need.

So you're going to make a lot of mistakes. You have to try a lot of things and then shift direction. As soon as you learn, you have to learn from your failures. You're going to have a lot of failures. And you pivot. So all when you get the new information, you need to change direction.

While you're building a deep-tech startup, you want to think about these two concurrent paths that you're working on. One is on the technology development. So you want to run a lot of tests to try different technology paths without investing too much. So you want to de-risk the areas of greatest technical risk without building a complete solution.

So you might say, yeah, I know how to build a box, and I know how to do the electronics and how to do the software, but I want to de-risk this one particular element. So I'm just going to build in the lab a little subcomponent to de-risk that one element. And you want to test these elements of highest risk and see what works, what doesn't, and then pivot and change direction.

Simultaneously, you're looking for the market opportunity. So you want to articulate, what are the technology advantages based on your intellectual property? Consider a wide range of industries. Speak with experts. Try for 20 ideas. Test the ideas quickly.

And this is like speed dating. It's not dinner. It's coffee. So you just want to do something really quickly. Talk to customers. Get their feedback. And then based on that feedback, you may say, oh, well, if you could do something slightly different than what you're proposing, the customers might have an interest. OK.

Now you go back to your technology team, and you say, can you do this slightly differently? And this is an iterative process where these two components-- the market side and the technology side-- are working side by side. You're developing both of them together to find that perfect product market fit. But always, you want to be going back to that intellectual property. You don't want to be finding perfect product market fit where you can't protect it. So you always want to be driven by that IP-enabled technology differentiation.

So when you're looking for this product market fit, the answer isn't in-- there's no book to look at. It's not on the internet. Basically, what you need to do is articulate what is special and different based on the IP. Identify those potential markets. Create a prototype or a storyboard so you can communicate with customers. And then get your customer testing. Get their feedback. And that will direct the technology development.

So an example of this is we had a 3D printer that could make parts this big. But a lot of our customers wanted larger parts. So we went back and made a larger 3D printer for those. We talked to the team. They said they could scale it. And we made a larger 3D printer to satisfy the needs of the customer. And yeah, so that's how that worked.

One mistake entrepreneurs make is to seek a popular market. Everyone's talking about it. A crowded market must be promising. There's room for Pepsi and Coke. Incumbents are slow and stupid. We can be really nimble. And we don't need a lot of product differentiation. There's space for us. And if worse comes to worse, we can always lower the price. This is a terrible idea.

Instead, you want to look at the opposite. You want to be differentiating. Lowering the price is not the answer. In fact, you should aim to price at a premium to whatever is in the market. You should look to do something that is so much better than anything else on the market that you can price it higher. So the state of the art for you should be the starting point, not the end point.

Differentiate so you can sell at a premium. And always go back to what makes you uniquely positioned to do this. Ideally, your IP will enable you to do something better than what anyone else has.

Consider your competition. So competition is a double-edged sword. Competition can actually be useful to you. In our case, it was useful because they were not huge companies. They were a lot bigger than we were, and they were public. They had more money than we did, but they were not Amazon or Google. And they educated the market. And in a case of something very innovative, like 3D printing, there was a lot of time and money required to educate engineers on why they would want a 3D printer.

So what we found is that we liked having them out there because once people were educated on why they needed a 3D printer, then they would come do their research, and they would find us. Or if somebody already had a competitive 3D printer, we didn't mind that. They understood the value of 3D printing. Now we could sell against them and say why we were better. We were faster, and we were cheaper, and we could do color. And so we preferred a situation where we could compare it to something versus the do nothing, where they said, why do I need to do anything at all? I'm happy just looking on my computer screen.

However, competition from huge incumbents-- like Amazon, like Google-- is really tough. They have everything. They might be slow, but they're not stupid.

So you have to think about who's well positioned in terms of customers or technology. Why haven't they done it already? Maybe the enabling technology didn't exist. Maybe regulations didn't exist-- not that they're stupid, were smart. That is generally not a good answer. You want to think about, what will keep them from entering the market? Will they have an unfair advantage or will we?

Then you want to create your minimum viable product as soon as possible. Get customer feedback and iterate on the design based on that feedback. One of the mistakes entrepreneurs make is striving for perfection. They want it to be perfect before they'll show it to the customers. And if they get poor feedback, the conclusion is, well, they didn't really understand. They didn't get what we're trying to do. They're not as innovative as we thought because mom and dad love it. Mom and dad think it's great, so it must be a good idea. No.

Everyone's going to tell you they think it's a good idea. That doesn't mean anything. You need to talk to customers. You need to try to get their money. Until you get somebody to give you money for your product, you don't know if you have anything at all.

So you want to get feedback. If you wait to get a perfect product, it's too late. You've missed the market. So you need to get-- no matter how smart you are, whatever you think is the right product to develop, when you get that customer feedback, you'll see that you're close, but you weren't quite on the market. And so when they give you that feedback, you'll use that to steer the development slightly differently.

You want to speak with customers every day. Get their feedback. Get something into their hands that they can try. And try to sell the product before it even exists. Prospects will guide the development.

The customers may even fund your R&D. This happens all the time. And that's hugely validating. If you can get your customers to pay for the R&D, then you are onto something. This has real potential.

And don't worry if a lot of people think your idea is bad. You are not trying to create a solution for everyone. You just want to identify a niche that really gets excited about it. And then you can grow from there. So you need to find that first beachhead market that's going to get very excited, and then you'll know you're onto something.

The next mistake is creating buzz. So focus on awards. Especially if you're coming out with some innovative, new product, people love covering that. They might write articles about you or give you awards. You might say, oh, I'm going to invest in a video that's going to go viral. Track your social media. Track the press. No. This is a losing bet.

Instead, you want to focus. Stay focused. Ignore distractions. Disregard your awards. Disregard press coverage.

What you want to do is focus on your revenue, your growth, and your customer, and your profits. So always be thinking about your customers. Are you solving their problem? Is that generating revenue? Is that generating profits? And make sure you look at the trajectory of your revenue growth. You want that to be as steep as possible.

Another mistake is hiring people you trust. That seems so logical. Why wouldn't you want to hire people you trust? Well, loyal teams made with friends and family may not be the skill that you need. Having a famous board chair who's on a million other boards may not actually be particularly helpful to you.

What you want to do is find founders with specific skills. So this is our founding team at Z Corporation. Jim Bredt is on the left. He got his PhD at MIT just as we were founding Z Corp. He was one of the crazy inventors who came up with this product.

Walter Bornhorst was our chairman, who guided the strategic direction of the business and made some absolutely critical, strategic decisions that I'm going to talk about later in the presentation. That's me and then Tim Anderson, who was another crazy inventor who was working on the project at MIT and had the idea of using a paper printer to make 3D-printed parts.

So you can see we had very different skill sets, business strategy, business execution, material science, and software, and hardware. So we were coming at it from very different perspectives, very diverse backgrounds, talents, interests, experience. And that's what you want to do, is build a diverse team.

Leverage your network, but don't hire anybody that you can't fire because people make mistakes. This is part of what happens. Ideally, if you can work with people before hiring them-- if they don't have a job, then bring them on for one week. You'd be amazed how much you learn about a person after one week, much more than from an interview. And you'll pay them for that week, but you're not committing to anything until the end of the week.

Empower the stars. So Jim and Tim were the most extraordinarily talented creative engineers I've ever met. They were unbelievable. But would they show up at the office at 8:00 in the morning? Mm, sometimes, maybe. There were certain things that just didn't-- it wasn't their thing. And yet, they could leapfrog the technology by coming in with a new idea and just blowing us away with what they had done. So when that happens, you want to empower those stars and fill in the holes with other people who can do the other elements-- organizing and keeping paperwork straight or whatever.

One other thing is having a salesperson on the founding team is often a very strong indicator, something that will help you get successful because they will have a very strong communication with the customer and have a finger on the pulse.

Sometimes senior executives will be out of touch and may not be that helpful. And busy board members may be disengaged. So they may be doing too many other things during board meetings, and so they're not going to be that helpful.

Be nice. What's wrong with being nice? Maybe you avoid conflict. A lot of people avoid conflict. Sometimes negotiating feels-- my mother hates negotiating. She just refuses to negotiate. She buys cars at full price because people are nice. People are fair.

But actually, no. Instead, embrace conflict. Conflict is the answer. And I really take great pride in the culture that we built at Z Corporation-- and it wasn't just me, it was our team-- in embracing conflict. So we argued about everything, like, everything. We were arguing all the time. Didn't take it personally, never got angry, never held a grudge.

But we would debate things to the end. And as a result, we reached such better decisions because everyone was coming at it from a different perspective, with their different background, their different experience. And that is what led to better decision-making. So encourage diverse opinions at every level of the organization, and you'll find you make better decisions.

And also, learn to negotiate. I guess there was some negotiation earlier in the week, and that is something you can definitely learn. Read a few books and practice every opportunity you have. And you have many opportunities just in your personal life. If you're in a relationship, you have to learn how to negotiate. And it's a good skill.

Another is working hard. So sometimes people think that creating a frenetic work culture is helpful to the business, that you should sweat and slog 14 hours a day, use TV as a model, have an abusive boss. You're fired, that kind of thing-- or lingering wine lunches or this mistake-- by the way, I forgot to say at the beginning, every mistake I talk about here, I have made every single one of these mistakes. So I am talking from personal experience.

This one is very personal to me-- checking off to-do list instead of strategic contemplation. I generate so much personal satisfaction from checking things off to-do list. And that is just not how you should really spend your time.

What you want to do is have a healthy culture, a strong team. These are critical to your success. If you ask VCs when they invest, what's the number one factor, it's about the team. And the team is only going to be effective if it's got a healthy culture supporting it. So you want to focus. You can't do everything. As an entrepreneur, you have 1,000 things on your to-do list, and you need to prioritize.

Every day, you need to reprioritize because things are going to move around. And you may only get one thing done on that list. But if that's the most important thing, then you need to do that number-1 thing. And don't do number 17 because that's easier and that's faster to get done. Start with number 1, the most important thing on the list.

At the end of the day, all that matters is results, not face time. Time to market is absolutely critical. So there needs to be a sense of urgency in the company without this frenetic chaos.

Focus on strategic decisions. So these are things like, what business do you want to go into? Who should you hire? Who's on your team? What are you going to build? How are you going to price it? And then when you get new information, remake those decisions. This is how you should be spending your time, not going to number 17 on your to-do list.

But then there are going to be a lot of non-strategic decisions. And again, this is a mistake I made often. Non-strategic decisions-- just delegate them. Let somebody else make these decisions. Who gets which office? What furniture goes where? What accounting software should you use? How many conference rooms should you have? Things like this. Other people can make those decisions. Don't waste your time on it.

Strategic decisions ultimately determined our future. So no venture capital-- now, that wasn't really our decision. That was because nobody wanted to invest. But at the end of the day, it had very big strategic impact. Because we didn't have venture capital, we had to bootstrap. And so we had to reach profitability quickly. And that actually was very helpful to us in the long run because at the end of the day, we were able to build a \$30 million business using only \$2.5 million of capital. And so it ended up being a very positive thing for us.

It gave us a focus on profitability, on getting to revenues quickly and then profits quickly. And then product pricing-- it turned out that our product, as with many hardware products, was much more expensive to produce than we anticipated. It was also much more expensive to sell and market than we thought. Because it was so innovative, educating the market took about a year. So in other words, it took about a year from when we would introduce our product to a customer to when they would buy it. Over time, that shortened, but that's a long time.

And obviously, as we got new information, we had to remake our decisions. So I want to give you an example of this. And our 3D printing market was ripe for disruption. So at the time that we founded Z Corporation, there were high-end machines. They were actually called rapid prototyping machines. And they sold for \$100,000 to over \$1 million. And we said, we're going to come out with something that's much less expensive. We're going to sell it for \$20,000.

And instead of going after higher-- because all these equipment manufacturers were going after higher and higher accuracy. They were becoming more and more expensive, more and more accurate, nicer-quality parts. But they were very difficult to use. They required a special lab environment.

And so we said, there's a hole in the market. And one of the books I highly recommend is *innovator's Dilemma*. And it's a terrific book. And that's basically what we did.

We came in the very low end of the market where nobody was really addressing the needs of that part of the market. And we said, at the early stage of the design cycle, we're going to make 3D-printed models very quickly and inexpensively in an office environment for equipment price of \$20,000. So that was our vision.

It was going to be easy to use, and it was going to take us two years of development. But we had a plan. We had a vision.

Then one year in-- so we still hadn't released the product-- we went to a trade show that almost killed us. The two incumbents, the two large public companies in our space, announced that they were making office-compatible 3D printers. They were going to sell them at \$60,000. And so we went to the trade show and interviewed the sales guys there.

And this is something to note. We never lied about who we were. We were very straightforward. Sales guys love to talk. They will tell you everything. Ask a million questions. They will tell you all the answers.

And we noticed a few things, that they had their sample parts under glass. So they would have three precious sample parts under glass. And we said, why is that? Well, they took a long time to produce. And we thought that was really interesting. And we asked, how long did it take to produce? Well, what we learned was that our technology was 10 times faster than their technologies-- over 10 times faster. So that's the difference between getting a part like this in 20 minutes or waiting till tomorrow to get it.

So you can imagine if you're a design engineer designing a product, you don't want to wait till tomorrow. You want to see it in your hand. And you don't care that our parts are kind of crappy. Our parts had poor surface finish. Our parts were weak. They were not great parts. They were not made out of plastic. But you would have something in your hand as opposed to seeing it on the screen, or waiting till tomorrow. And that really resonated with customers.

And that's where our chairman came. We had an emergency board meeting because we were talking about perhaps closing down the business if we didn't get good information from this trade show. But based on the fact that we were over 10 times faster, despite having some other shortcomings on our parts, our chairman said, why don't we price it at \$60,000?

Instead of \$20,000, Let's price it the same as the competition, and we'll see where we go from there. Of course, our team was really upset because they said, we've been telling everybody we're going to sell it for \$20,000. We can't sell it now for \$60,000. Well, no, yes, we can. We hadn't released it yet. And you can always lower the price. You can't raise the price. So let's just try it out. And if nobody wants it, we can just lower the price. So we decided to try it out.

It turns out, in the end, that \$60,000 is a sweet spot in the market. It's a great place to have a product. And we had a product in that price point for many, many, many years. And it turns out, it saved our company because it was so much more expensive to sell the product and to market the product than we expected that that ultimately saved our company.

So product differentiation is critical. It needs to be something that the customer really cares about. But you don't need to be better at everything. You just need to be better at that one thing that they care about.

So I just want to pause here. Does anyone have any questions or any feedback? Yeah?

AUDIENCE: Thank you for sharing. I actually have a question. So how did you validate the market before you have to advocate that? Do you get where I'm getting at?

MARINA Yeah. So I took sample parts to the first trade show-- we didn't have a booth-- and I showed it to customers. So I **HATSOPOULOS:** just walked around and I said, here's a sample part. What do you think about it? And I got feedback.

Interestingly, most of the feedback was pretty negative. So they said, your parts are really crappy. And I said, yeah, OK, they are. But you can have this in 20 minutes instead of waiting till tomorrow. And they were like, hmm, OK, maybe, interestingly.

And I don't know where I land on this. As an entrepreneur, you need to listen to the market, definitely. But you also need to stay true to your vision as well. And if you listen too much, sometimes the customer can't really see the future so well. And they can't visualize.

So for example, what we found with customers is we would 3D print their sample part. So we'd say, what are you designing? And they'd say, a new pump. And so we'd say, OK, send us your design, and we'll 3D print your pump.

And it was only when they saw their own design in a physical form that it resonated with them. And they were like, oh, wow, this is so cool. And then sometimes they would say, your 3D printer is flawed because this wall is too thin, and we could never manufacture it. And we'd say, well, go check your CAD design. Our 3D printer is only making what's in your CAD design. And so they would see things that they hadn't seen before.

But in answer to your question, it's an iterative process. So you build a very crude prototype. You get some feedback. Based on that feedback, that guides your technical development. And so it's an iterative process, and it gets better and better.

AUDIENCE: Thank you. So would you say-- in a word, would you say you have a vision for it before you get any feedback of what [INAUDIBLE]?

MARINA Exactly.

HATSOPOULOS:

AUDIENCE: And then you get some validation that reinforces that.

MARINA Exactly. Yeah. So for example, one of the two markets that we had identified, Tim had identified industrial design, **HATSOPOULOS:** and I had identified architecture. And those were two of the big markets we were going to go into to start. What we learned was that in those days, in the early '90s, industrial design and architectural firms, number one, were small firms; number two, were designing in 2D CAD, not in 3D CAD. And without the 3D CAD data, you can't use a 3D printer. And they all cared about surface finish. And our parts had pretty crappy surface finish.

So what we learned were that those were not good markets for us. And we migrated to design engineering across multiple verticals. Yeah, there's another question there.

AUDIENCE: Hi. I just wanted to ask you, how would you define yourselves in a market like software, artificial intelligence, where the cost of prototyping is very low? Software contains models. There is a lot of open-source models and foundational models. And then when you truly create something new, it feels like it's just combining the technologies together. So do you have-- I don't know. One example of that is that you have ChatGPT against DeepSeek that's in a couple days.

Essentially, DeepSeek is combining reinforcement learning with language models. And ChatGPT is just learning out of users. So the difference between those models is that you just have a combination of two different parts. And then my question is, if you're ChatGPT, you're trying to do something like ChatGPT, but you're a startup, then you have Google, you have Amazon, you have Facebook, all of these companies with immense resources. And new technology is like-- it could be modified and easily create a similar. So how do you defend that?

MARINA I don't have the answer to that. My experience is in hardware, where you can patent it. The whole open-source **HATSOPOULOS:** thing-- honestly, I just don't really understand that, so I'm just not the person to ask on that. So I apologize. Yeah?

AUDIENCE: It could be a weird question. Everybody says team is very important. I mean, yesterday a venture capitalist. Yeah. All investors say the team is important. Well, I'm a very bad team player.

MARINA [LAUGHS]

HATSOPOULOS:

AUDIENCE: And when you said, oh, I found two engineers to manage, oh, that's good. I started that, sparked something. But I don't have two engineers to manage where I had seven or more when working in consulting and multinationals. I mean, I get things done, but very bad team player. So how do I fix that?

MARINA Wow, that's a tough one. I mean, your self-awareness is a good start.

HATSOPOULOS:

[LAUGHTER]

So, I mean, you have to find the right people that you want to play with and who understand you, and you understand them, and you respect them. I mean, I think one thing in our team that worked really well was there was real mutual respect. I looked at the stuff that Jim and Tim were doing and, how did they do that? That was just remarkable. And so appreciating what each person is bringing to the table and the fact that thank God for them because, otherwise, you'd have to do it. And so maybe some maybe appreciating what each member is bringing to the table. I don't know if that helps. Was there something up there? Yeah?

AUDIENCE: So my question is about ignoring-- you said you have to ignore some input from the customer. And I've been in a position where I've had to ignore advice. So would you say that you ignored that customer input because they didn't actually understand what you are saying, and you have to go all the way and deliver a model? And finally, then they understood. Do you think this is a good rule, that rule of thumb, where if they don't understand what I'm saying and they're giving me advice, maybe I can ignore it?

MARINA It's really tricky. So I don't have an answer for that because I think entrepreneurship is this constant thing of **HATSOPOULOS:** listening and not listening because a lot of people are going to tell you it's a stupid idea. And the people who have been in the industry typically dismiss anything new. And oh, that's stupid. That's dumb, blah, blah, blah. And so I don't know how to tell you what's the magic as to when to listen and when not to listen.

I think one thing I would say is that the closer you can get to having a customer using your actual finished product as it's intended to be used-- the closer you can get to that, the more useful the feedback. The further away you are from that and the more visionary it is and la-la-la, their feedback is a little less useful. And so that's why you want to get to-- when I said the MVP, you want to get to that as soon as you can because that's going to give you much more useful feedback than describing your idea.

AUDIENCE: Thank you.

MARINA Sure.

HATSOPOULOS:

AUDIENCE: Marina, I've seen discussions with customers. Actually, you should do that. I've seen situations, though, where people you're talking to are what I'll call power users. And I've gone down rat holes where they'll say, oh, if you could do this, oh yeah, we could do that. And then before you know it, you make a product for only 1%. Have you ever faced that? How do you deal with that?

MARINA So actually, the large machine that I talked about, we built that because a lot of our customers were asking for it.

HATSOPOULOS: And by a lot, I mean, like, a dozen of our customers. But if you only have 40 customers, a dozen seems pretty significant. And we built them, and we had a dozen customers. No, I'm exaggerating. We had more than that.

But basically, there was not a huge market for it. It was this very limited market of power users who used it a lot. But there wasn't some big market for it. And the technical challenges turned out to be much greater than what we had anticipated. And so that whole project turned out to be a real derailing of our technology development.

If you looked at our technology development path, which was to make the machine less expensive, easier to use, that one was just going in the opposite direction. And so I think you also always want to be cognizant of, where do you see this going in the long term? And is this on our path? Or is it off our path?

AUDIENCE: How do you define deep tech? And then from an entrepreneurial perspective, do you recommend going out and seeking technology like how you did or have an idea, and trying to find the technology that solves that?

MARINA Yeah, so great question. So deep tech, I define as something that is going to take a long time to develop. And so

HATSOPOULOS: the core science is there. But it's going to take a lot of development to actually make it. And often, it's technology that has come out of research-- let's say, at a university. And so it's a solution looking for a problem as opposed to the second scenario you mentioned, which is a problem looking for a solution.

And that's where you've got of dual paths where you're developing the technology, but you don't know quite what for. And so then you have to figure out who cares. What is the market we're trying to serve? And so then let's develop it along a path that makes sense for this problem that we're trying to solve. And so that's why the two have to be done in parallel iteratively because you don't know exactly what direction you're going until you know what market you're going after. Any other questions? How am I on time? OK.

AUDIENCE: You mentioned that the name of the company is very important. And you mentioned a few things about it. I'm just curious to know the story behind how you came up with the Z part.

MARINA With the name?

HATSOPOULOS:

AUDIENCE: The Z part.

MARINA Oh, yeah. So it was because we're a 3D printing company, it's the z-axis-- so the third dimension. There were

HATSOPOULOS: some really bad names we tried before that-- Shape Shooter, Speedy 3D. Yeah?

AUDIENCE: Hi, Marina. Very good presentation.

MARINA Thank you.

HATSOPOULOS:

AUDIENCE: It seems to me you had the benefit of actually having an operational prototype that did some things, at least to demonstrate the concept. What would you recommend if you were actually in a situation where you've actually got an idea and you've certainly got some technological basis for that idea, but we really have an operational prototype, and you're looking at possibly some significant investment just to get to that first stage? Any thoughts on that?

MARINA I'm not sure if I understand the question.

HATSOPOULOS:

AUDIENCE: Hardware products-- so you had an operational prototype of some sort, what if you actually just have a concept of a product that is based in good science, but you haven't been able to prove it yet. Do you have any thoughts on that?

MARINA Well, I think you build, first, the proof of principle-- so that ugly machine that I showed-- to de-risk the technology.

HATSOPOULOS: And then you can build a crude prototype just to pull it all together as quickly as you can with low investment. But, I mean, hardware does take a lot of capital, and it also takes time.

It always blew my mind. From when it's an idea to when it's actually a product is two years. And so you better kind of know-- yeah, better be going in the right direction. In this process, as I said, you're often making decisions with incomplete information. You feel like you're in this murky, muddy waters. You have too many decisions to make, too many directions, like, where to take the technology, where to take the product, which markets to pursue. And you're getting conflicting opinions because everyone has a different idea.

And when you don't know what to do, the north star you always need to go to is your intellectual property. What is it that makes you special that you can protect? This is what you have your moat around. You have this monopoly. So go back to that.

And so in our case, it was speed. So when we were thinking about what markets to pursue, architecture didn't make sense because our parts were never going to be beautiful. They were just going to be fast. So then we looked, OK, who cares about speed more than part quality? That really gives you the answers to your questions. And customers also help give answers to your questions.

Another mistake is always staying positive. Now, this is a very entrepreneurial characteristic. Put bad news out of your head. Believe the engineers in terms of time and cost, regardless of history. This time will be different. No.

Instead, learn from your failures. Do things differently. Assume the time and cost will be at least double what anyone says. It's a rocky road. You're going to make lots of mistakes. You're going to hit these bumps. But every time you get new information, every time there's a failure, reconsider your decisions and re-steer things based on that information.

Another is to focus only on growth. Now, I said, you should focus on growth. Growth is a good thing. But don't assume the cash will be there when you need it. Sometimes people get into this mode. Who cares about profitability? We're just going to grow. Growth will bring an exit. That's not always the case.

Companies go out of business because they run out of cash. It's as simple as that. So you always want to have your eye on your finances. Track your cash. Track the time and investment it's going to take to get to revenues and to get to profitability. A strong company will be an acquisition target, so don't worry about that. Just build a strong company, and the rest will take care of itself.

Another is to put on a great show for investors. Use industry acronyms that nobody knows what they mean. Have complicated diagrams. Have entire paragraphs with teeny, tiny fonts that nobody can read. Have projections with revenue skyrocketing. Talk about exit in your pitch. Demand a demanded confidentiality agreement. Add AI, no matter what business you're in. No. Don't do any of these things.

Instead, simplify your pitch. What is the problem you're trying to solve? You'd be amazed how many people send these pitches. And you're going through it, and you're hearing all about this technology. And they haven't told you what is the problem they're solving?

What is the problem? What is your solution? Remove the acronyms. Remove the diagrams which don't add information. Limit the number of your words. Quantify the value proposition, and definitely practice your pitch in front of people who don't know anything about it. And have them repeat back to you what they understood because what you'll find is they have no idea what you're talking about.

Try to complete the sentence, we make blank for blank to blank. So we make, what is the product, for blank, who is the customer, to blank. What is the problem you're solving? If you can identify who the customer is, what the product is, and what the problem is you're solving, people will understand your pitch. Then you can get into the detail. But that should be your starting point when you're doing a pitch.

When you're looking at fundraising, consider all your options-- friends and family. That's typically where it starts. Then it goes to angels. Could be as little as \$25,000 up to \$1 million. Equity crowdfunding is sometimes an option.

Do not forget government grants. I've worked with startups that have raised many millions of dollars from government grants. When you're doing deep tech, there's a lot of funding out there from the departments of the US government. And also, in Europe, the same holds true.

Also, don't forget strategic investors. We had several strategic investors. They are great. They're patient money. You can get from them either equity. That's what we did. You can get R&D contracts. So those are non-dilutive. And they're hugely validating. If a customer is willing to pay you to do R&D, that means you're onto something.

Or we required customer deposits. So at the time that you placed the order, you had to give a 20% deposit, and then you'd get shipped the product in three to six months. So that helped us fund the business. We needed that cash. And then, obviously, venture capital, which could be equity in the early days or, as you get to revenue and profitability, can include venture debt.

You want to identify strategic investors. So this is anybody who's invested in your success. It can be your suppliers. It can be your customers. It can be your distributors. In our case, one was a customer, and one was a distributor. And they were so patient. They didn't take a seat on the board. I just sent them a quarterly report, and that was the end of that. They were great to deal with. And so I highly recommend it.

There's some things to be careful about. You want to be careful, if it's your customer, about what information you share with them. You may not want them to know the gross profit of your product, especially if it's really high, because you don't want them to use that against you in negotiating the price. You may be concerned because if Ford is a customer and they're investing, then maybe BMW doesn't want to buy your product. So you have to be sensitive, but it can be navigated.

A common mistake I see is underestimating fundraising. Fundraising is a very big job. It takes dedication. It takes time. It takes a lot of research and a lot of energy. You will face rejection and failure. And that's one of the reasons you should do research on these investors.

Check them out. If they only invest in AI software companies and you are a 3D-printing company, don't pitch to them because you know what? And what I see startups do is they're like, well, I'll just give it a try. Who knows? Maybe this time. No. That's just one more rejection. Trust me. You're going to get so many rejections. You do not need one more rejection. So reject yourself. Do your research, and don't put them on the list.

And it's important to learn from every rejection. Did you not do your research? Should they never have been on the list? Or was your pitch not good? Did they not understand what you were saying? Maybe you need to improve the pitch?

Or maybe they had some really good strategic advice about your business model. Maybe your business model is flawed. Instead of selling 3D printers, you should provide the 3D-print service to your customers. So make sure you get some strategic takeaways from every failed pitch.

And I want to say-- because everyone talks so much about venture capital, and it seems to be a figure of merit-- that venture capital is not for everyone. It's not for all startups. For sure, it offers terrific opportunity. It gives you cash to grow the business. You will get great advice from experienced investors who have a strategic perspective, and they have visibility in a lot of related startups.

They'll also provide connections. They'll give you connections to customers, to other investors. And it's hugely validating. If you have a top-tier VC in your cap table, that's very validating. That can actually help you with your customers. So venture capital definitely offers opportunity.

And I also should say, it may be necessary. So some startups have to grow big and fast, or else they're going to fail. High growth might be necessary to capture market share. So for example, Uber-- creating a two-sided market, growing around the world-- that really needed to grow fast. It required a lot of capital. That was not a bootstrap kind of a business.

You might have high capital costs that if you're building a satellite company, you're going to have high capital costs. OK, that makes sense. But venture capital carries risk. Sometimes it can be easier to move forward without product market fit.

One thing I see out of startups out of Greece-- because there's less capital available-- is that they are finding-- they're honing in on their product market fit sooner. They're reaching profitability sooner. Sometimes it's easier to think everything is OK if you've got lots of money and you don't need to get to profitability.

Some businesses can't grow at the pace that VCs expect. I would categorize Z Corporation in that role. We needed to educate the market, and that just took time. And that wasn't something you could throw money at. You couldn't advertise during the Super Bowl and expect to grow a lot faster.

You give up control. So as a founder CEO, you may be kicked out. And so you need to understand that. The other thing is a higher valuation raises the bar, and it makes it harder to make a return for investors. So you better be sure that you can put that money to work and generate a return.

Consider your personal ownership. VCs want unicorns. They want a billion-dollar business, and it's pretty much all or nothing. So they're going to push for growth. And sometimes the startup will fail as a result of pushing. But it might have thrived on a slower path.

And so keep in mind that a very profitable \$10 million startup that you own entirely yourself might be a better outcome for you than pushing to grow a business too fast and having it fail. So you just need to know what business you're in, whether this makes sense or not. And the only reason I mention this is because sometimes people think venture capital is the only path, but it really depends what business you're in.

Beware CEO syndrome. So I'm going to go through this quickly. Startup CEOs face failure and rejection every day. They get a lot of skepticism and doubt from engineers, from their customers, from their employees. Their ideas are challenged every day, and they slam the CEO.

And that leads the CEO to have to rework. They redesign the product, redefine the product market fit. And they get a strong articulation of the value proposition. And then they go back. And this rework leads to success.

And the success convinces everyone that the CEO is a visionary. And the company gains visibility, and awards, and the CEO becomes famous, and everyone listens. And people stop challenging the CEO. And now the new employees come in who never knew the CEO in that early stumbling stage, and they only see this very confident CEO.

And now the CEO starts to feel invincible, and the CEO is liberated from self-doubt. No more conflict with others, stops asking others for opinions, stops listening to other ideas, dismisses competitive startups. Not to worry, we're doing great. And it's this primal urge for validation that ultimately causes the CEO toward blindness.

And this success can trigger CEO syndrome, where the CEO becomes insulated from honest feedback, stops getting a diversity of opinions. And this leads to poor decision-making. And poor decisions can be fatal. You might develop the wrong product. You might demoralize employees by micromanaging and not listening to their opinions and their ideas. Or you might get blindsided by competition by that new startup that you dismissed just the other day.

So the antidote to losing touch is to be challenged. And Bob talked about that, about having that advisory group. You need a group like that that's willing, even eager, to challenge your ideas. And you need to ask questions. Listen and learn from these people. They feel like small failures. It feels like small rejections, like you go to them with what you think is the greatest idea, and they knock it down. But it's that process that will lead you to better decision-making and your ultimate success. So beware of this, and you will do well.

So commercializing deep-tech hardware is tough. Focus on key elements for success. That's defensible product differentiation, a strong, diverse team, encourage debate and a healthy culture. Talk with your customers. Get a prototype in their hands. Focus on revenue and profitability within your beachhead market. And make decisions whenever you get new information.

Entrepreneurship is rewarding. You have to persevere. It's hard. There's no life balance. But find something you believe in. It's going to be a long road, for sure.

Celebrate your milestones and have fun along the way. They are risky. Startups are risky. But controlling your destiny is incredibly enriching.

It's really fun. It's great fun to take a blank sheet of paper and develop a plan, develop a budget, develop a brochure, develop a sales contract. Every aspect, you're starting with a blank sheet of paper, and you are creating something that did not exist before that would not have existed without you. And that's really, really exciting. So as Steve Jobs said, "It's more fun to be a pirate than to join the Navy." And I couldn't agree more.

So there is a class that's going to be taught this spring-- I'm co-teaching with Ken Zolot-- called Founder's Journey-- Launching and Scaling Hardware Startups. It's going to be Tuesday and Thursday afternoons at 3:00 to 5:00 PM. And feel free to join, and I hope you'll sign up for it. It's a special class. So thank you.

[APPLAUSE]

JOE HADZIMA: A lot of information there. Thank you very much.

MARINA Thank you, Jeff.

HATSOPOULOS:

JOE HADZIMA: I'm just going to do a quick wrap-up here of the course. Let's see if I have it. OK, oops. There we go. Do you have the clicker?

MARINA Oh.

HATSOPOULOS:

JOE HADZIMA: Oh, you left it up there. OK. OK. Well, time for a quick wrap-up of the course. Whoops. Time for a quick wrap up of the course. So the question is, what did you learn in the six sessions? In the first session, I put down a whole bunch of questions you needed to answer. And you remember all of these? I want you to look at those and think about the different sessions we have and see if we've covered all of those things.

You can go back and look at the first slides. And remember, I said, why this course? I said, it wasn't going to be about theory. I think you realize it has been about doing. You just heard a great set of experiences conveyed to you about what actually happened.

Remember, our goal was to increase your probability of success. And we talked about it being a lifetime, incurable disease, not fatal, but it's a full-contact sport. It's not an academic exercise.

So did we deliver? Did you get all of these enhanced skills? I think if you go down, we've covered pretty much of those. If you don't think we did, please let us know.

We ask personal questions, and tonight's been really about having you think those through. And the question is, where do you go from here? This is a graphic about the different programs, classes, et cetera at MIT. It comes from the annual report at the Martin Trust Center. And these are things, if you're part of the MIT community, you might consider. You can find them there. You can go to the Open Innovation website, which tracks events and all the other programs.

And we've mentioned a couple of times Venture Mentoring Service. Now, just to give you a little more thought on that, Venture Mentoring Service tries to train the entrepreneur, help the entrepreneur see through things. If you go there, you're going to be given homework. You'll have a mentor team that will be assembled to meet the kind of things that you mutually decide you need. And you'll meet with them on a pretty consistent basis. It's a great resource. If you're ready to really get into starting a venture or starting, really, to push an idea, I would recommend you go to Venture Mentoring Service and check that out.

So with all of that, we have a couple of assets for you. If you found this useful, let us know. Certainly, let us know now, but even more so later. The reason somebody said to me the other night, why have you been doing this all these years? It's because five or 10 years later, somebody will send an email and say, I remember something that was talked about in the course, and I just wanted to say it was very helpful to me. You heard that on our founder night when one of our panelists said that dealing with the Technology Licensing Office, they looked at stuff that I had put out there. That's very encouraging for us. So let us know.

Let us know about other topics we should cover as you go on your journey. Be sure to give back and help others. That's a really important thing. As Bob said tonight, it can be lonely. Be part of a community that helps each other.

And remember, H equals R are divided by E . So think about everything you do. Can I create a reality that exceeds expectations that people have and make people happy?

And so with that, I'm going to declare this six-session journey through Nuts and Bolts of New Ventures to be concluded. And I wish you well on your journeys going forward. Thank you.

[APPLAUSE]