

URL: <https://ecorner.stanford.edu/videos/deep-tech-innovation-entire-talk/>

Margo Georgiadis is a CEO-partner at Flagship Pioneering and co-founder and CEO of Montai Health, an AI platform company enabling the predictable discovery of breakthrough small molecule medicines to treat and preempt chronic disease. In these roles, Georgiadis brings extensive expertise in technological innovation and high-performance business transformation and leverages her artificial intelligence and machine learning experience to advance biotech innovation. In this presentation and conversation with Stanford adjunct lecturer Ravi Belani, Georgiadis shares frameworks that Flagship uses to develop deep tech companies and her approach to addressing the challenges of drug discovery.



## Transcript

(inspiring instrumental music) - Welcome everybody 00:00:17,640 to this week's Entrepreneurial Thought Leaders seminar, the Stanford Seminar for Aspiring Founders. ETL is presented by STVP, the Stanford Engineering Entrepreneurship Center, and BASES, the Business Association of Stanford Entrepreneurial Students. I'm Ravi Belani, a lecturer in the Management Science and Engineering department at Stanford, and the director of Alchemist and Accelerator for Enterprise Startups. And today I have the pleasure of welcoming Margo Georgiadis to ETL. Margo is a CEO partner at Flagship Pioneering. How many people know about Flagship Pioneering? Okay, so those who know know, Flagship Pioneering is a singular unique biotech VC fund that both invests in and builds platform companies that change the world. Think of companies like Moderna, the COVID RNA therapeutic vaccine, that came out of Flagship Pioneering as one of its companies. Margo is also the co-founder and CEO of a Flagship portfolio company, Montai Health, which is an AI platform company enabling the predictable discovery of breakthrough small molecule medicines to treat and preempt chronic disease. Margo grew up in Chicago and she got her bachelor's degree in economics at Harvard, and then joined McKinsey as a business analyst out of undergrad. Two years after McKinsey, she went to business school at Harvard and then rejoined McKinsey for 14 years, rising up the ranks to become a partner in the retail and marketing practices, and then after that, after McKinsey, she had a dizzying array, for me, which is like an unprecedented diversity of leadership experiences, including being the EVP of USCard Services and the CMO at Discover Financial Services.

Then the President of Americas and the Vice President of global operations at Google. Then the CEO of Mattel, the toy company, and then also the President and CEO of Ancestry, the consumer genomics company. And then in 2022, Margo joined Flagship Pioneering and then also Montai Health. In a field of tons of failed studio models, which we're all hearing about right now, Flagship singularly stands out as one of the most notable successes, and that, sort of, is testified by the size of their most recent fund, which is \$3.4 billion. Margo also has received the Forbes Excellence Award in innovation, Chicago's Innovations Visionary Award, the Eastman Medal from the University of Rochester, just a dizzying list of awards including the executive of the year at Utah, and Crain's most powerful women in business and Fortune's 50 most powerful women in business. Margo also has three kids that are within your age. Her kids are between 20 to 29, including a son who's about to join the PhD program in the material science department at Stanford. So without further ado, please welcome Margo. Oh, actually before we applaud Margo, the agenda is that Margo's gonna give a quick presentation or talk, and then we're gonna follow it with a

fireside chat and then we're gonna have interactive student Q&A. So as things are progressing, start thinking about your questions, lining those up.

Just like last time, we'll try to pack in as many questions as we can. Okay, so now with that, you can give the applause. Please welcome Margo to ETL. (audience applauds) - Good afternoon everybody. It's great to be here, 00:03:32,970 and super inspiring to think about the next generation, as Ravi said, it's really fun for me. I'm actually excited to be here with Mo, we used to work together at Google for many years and built a lot of great things there, and it's really fun to see all of you excited about becoming entrepreneurs yourself. So as Ravi said, I spent about half my career working in more traditional industries, doing a lot of innovation, you know, it dates me, but at the early age of CRM I actually created the CRM practice at McKinsey, which was considered risky at the time. Did a lot of things in terms of the frontiers as we went into internet, mobile, video, and building these platforms. And what I'm really here to focus on today is what it means to kind of entrepreneur at the edge and reimagine what's possible. So I'm gonna share some of the principles that we use at Flagship Pioneering to repeatedly create these successful bio platform companies in life sciences and sustainability, and then we'll have a fireside chat to talk a little bit more about some of my experiences as a CEO, as an investor and an entrepreneur.

So I'm looking forward to your questions. So when I think about exploration and truly when you're leading at the edge, you think about yourself as being on that boat and truly losing sight of land. And there's this duality that exists, because on the one hand, we're so excited about the potential that's out there, but at the same time we have this trepidation of what we're gonna find. And as entrepreneurs, what makes people successful is the ability to balance that emotion, to have the vision and the conviction to manage through those stormy seas and get to the other side and then be able to capitalize on that opportunity that you find. One of the most important concepts that we talk a lot about at Flagship is the difference between risk and uncertainty. So when we think about risk, we think about things that are calculable. So if we're on that ship, what's the probability that we actually could survive different types of storms? That's something we can actually estimate, but uncertainty are those incalculable things, because if I'm going to a new continent that I've never seen before and I have no idea when I'm gonna get there and what I'm gonna find, there's no way I can actually do that, that's true uncertainty, and it's thinking differently about that duality that we found really enables us to be more effective pioneers. And I wanted to read a stanza from a poem that's attributed to Sir Francis Drake, who's a renowned explorer from 450 years ago, and I think it really highlights what we're all trying to do here as entrepreneurs, "To dare more boldly, to venture on wider seas, where storms will show your mastery; where losing sight of land, we shall find the stars." And I think that's what all of you hope to do as entrepreneurs. So let's talk about how you build the courage and the conviction to make that possible. So a lot of innovation focuses on the left hand side of this slide.

Things around businesses that we already know. How do we improve that business model incrementally, make it more efficient? Or how do we use innovations in data and technology to actually find adjacent opportunities? And these are spaces where we actually know the risk, but it's in areas like technology and biotech, with the transformations that are happening in these industries, that we're able to increasingly find these amazing red dot outliers. And that's, at Flagship, what we exclusively focus on, as leaders. And why do we operate beyond adjacencies? Well, we do it because there's the opportunity for a much bigger value pool. You also have less competition and you can define the rules of the game. You have a much more wide open space for intellectual property and you have the ability often to partner more easily with incumbents, because instead of replacing what they're doing, you're fundamentally additive, you're doing something new that they're not already doing. So they're really interested in identifying those new areas, and thinking about that is really important in terms of creating a new idea. But there are unique challenges to this pioneering approach. You have to deal with the fact that a lot of people will tell you it's impossible. "Oh, I tried something like that before, it's not gonna work." You have to overcome the dogma and you have to do it systematically and be okay with that.

It can feel really lonely at times because you can't just go to a KOLs tried something before, there's no playbook. You have to actually create it from the ground up. But I truly believe, from my experiences at places like Google and Flagship, that this is the best sea to chart. So let me unpack that a little bit more. So as I said, most innovation tends to be on the left hand side of this page. It tends to focus on adjacency, where there's less uncertainty and the risk is calculable. We can think about diversifying our bets and having a few shots on goal that we can actually estimate the probability of. But the problem that most people don't think about is you have the risk of innovation commoditization, because other people can see that opportunity because it's adjacent. So you're gonna have more people that are structurally going after that, both incumbents as well as new companies, and so actually, it ends up being riskier than you think. So just because you can calculate the risk doesn't mean it's less risky, because your ability to capture value from those investments is then lower, because there's more people going after the same opportunity.

And that's like a fundamental simple concept a lot of people don't think about. On the other hand, what pioneering and the way we approach it, enables us to make leaps to high uncertainty, but do it in a way that you can go after that greater value at lower risk. So it actually becomes less risky to pioneer. And that's because we think really, really hard about how do we do this differently? How do we take the risk out of the approach? How do we resolve uncertainty? And we do that by systematically identifying scientific proof, creating new inventions that enable us to make those leaps, and by building only multi-product platforms that take shots on goal. But the first shots we take, we try to take them in lower risk initial areas, that can then show the candy in the road to get to the bigger opportunity, and by doing that, we have systematically found that we can unlock much more value with actually the same or less risk. And that's what we do at Flagship. We've created an

institutional approach to innovation, where we pioneer new ideas, we entrepreneur these new capabilities to make them possible, and then we scale them into companies. And everything we do starts with a very simple leap of faith. What if the world could be dramatically different, fundamentally at a science level, in this area? So as Ravi said, my company is focused on something that if you're in life sciences, you know, is hard to say. How do we actually make small molecules predictable to develop.

Right now they have a 7% chance of success on average in the clinic. But what if we could think differently using AI and more human qualified chemistry to unlock the beauty in the molecules that nature has already generated, and use AI to match them into our pathways and make it much more efficient and predictable to make drugs that are more tolerable for chronic consumption. It's a hairy audacious idea, and then we have to break it down into its principles and pursue it, and we call that emergent discovery. We start, really, with an evolutionary process of the principles that it would underlie an idea. We generate different hypotheses for how you would actually prove that that's possible. And then we whittle it down and only the best ideas survive. And then as we build that platform, we reverse plan what is the story of the future, and we back it up and we build it systematically. And we've made this entrepreneurship into an institution. In fact, I was first introduced to Flagship Pioneering when I was at Google, because Google X was actually founded on the fundamental principles that Noubar had created at Flagship. How do you start with big what if questions? How do you create explorations where you can systematically understand the pillars of an idea? How do you take those surviving ideas through the process of evolution and experts, use those to create protocols, and then as you continue with very tight resources to evaluate your ability to succeed.

And it's really interesting, we actually don't give a company even a name until it becomes a NewCo, 'cause we don't wanna fall in love with it. It's just a number. Because it's about proving what's possible in the fundamentals. It can't have any emotion in it. It has to be about facts and data. And then when we get a NewCo, then we actually bring in a more seasoned management team to step on the gas and really scale that into a company and then it spins out and becomes a GrowthCo. And to do that, we've really built a culture that enables this to be possible. It's based on four pillars, being hyper-adaptive. So how do we have this immigrant mindset where we're going to these new places, we are paranoid optimists, that this idea is possible, and we are attacking it just relentlessly in terms of proving at a fundamental level and innovating to prove that. But then we are continuously learning in parallel.

So we often pursue the idea in three or four ways at the same time to make sure that there is an alternative hypothesis that could work equally as well. And then what we do is we actually grind that idea into a true platform that has scalability. And again, we're testing all those elements and this is what's enabled us to do this a hundred times. So we've created a hundred different companies, 30% of them have gone public. As Ravi said, probably the most famous that you guys all know is Moderna, but there's probably many other companies on this page that you've heard of. All of them were based on fundamental science that was created in Flagship Pioneering, innovative teams that came at these problems and put together diverse scientists and people from all different backgrounds to make them possible. So that is kind of the foundation and hopefully that's a helpful set of frameworks as you think about your pioneering journey. (audience applauds) - Before I dive into... 00:14:38,790 I wanna really click on just understanding how to build these deep tech ventures. But before I dive into that, I would be remiss if I also didn't put a spotlight on just the diversity and the depth of your background.

So I don't think we've ever had a speaker at ETL who has been the president of Google, the CEO of a toy company, Mattel, and now the CEO of a deep tech biotech company. And so I'm curious if you can spill your secret as to why you are such a singularity and in particular, are there any either mindsets or skills that you treasure, that you think are central to your ability to laterally jump into all these amazingly diverse fields and lead as a CEO? - Thanks for asking that question. 00:15:29,460 You know, I do think my career looks very random, but there is a golden thread that connects through all of it, which is, I'm a lifelong data and technology nerd, and I've really been focused on how do we reimagine ecosystems of all types. In fact, my first job was as an economist, building computable general equilibrium models of economies. So how did you think about all the macro and micro decisions? So I used to go around in paddy fields five miles from the nearest dirt road, as well as sit with the minister of finance and the food logistics agency to understand all those connections. So hopefully that makes sense. So if you think about what was happening in a lot of industries when I was at McKinsey, it was really about how do we better have traceability on understanding customers and the traceability of our economics, relationships and behaviors, and so we were reimagining ecosystems in retail and financial services and healthcare. And so I built a practice there that really was at... You know, we used to string together 12 work stations in the basement of these companies, 'cause it was before we had really sophisticated compute capabilities. And then I just kept following the tech into new areas.

You know, a role like Google just made sense, right? Because they were at the frontier of building these scalable platforms that are kind of proven. At Discover, it was my chops to turn around a credit card company using these principles, take it public, out of Morgan Stanley, but I was in search for like some bigger platforms, and I had some colleagues that had been long-term friends of mine that were at Google, had been trying to get me to go there since 2004. And so I decided, you know, to make the leap and be part of... We were just desktop search at the time, actually one of my early jobs was involved in launching one of the first Android phones and really building up that ecosystem. And it's hard to imagine that in 2009 that was new. And so we've come so far in this time period. And so each one of these had a theme around reimagining ecosystems. - It was reimagining ecosystems. 00:17:26,580 So there's this golden thread. You weren't reverse engineering this vision that, you know, you're gonna become the CEO of Montai Health- - No.

00:17:33,143 - Decades later. 00:17:34,710 You were following a thread, which was reimagining ecosystems. It sounds like it was also thinking about data driven decision making. - Correct. 00:17:41,010 - And then that just naturally 00:17:42,420 led you to each of these- - Yeah. 00:17:44,370 Think about being a translational executive in lots of different industries, because at Google you could have fundamental tech, but you still had to translate that to help companies reimagine how they did those things differently. So imagine if I'd been at McKinsey, doing that in all these different industries. I already knew those value chain economics. I actually knew a lot of those CEOs really well. And so helping people think about, you can create all this great technology, but you also have to help it get adopted, and you have to think about how can the ecosystem make sense of it.

And those choices become very, very important, that pattern recognition. - And you know, this has been a thread 00:18:17,700 for the last two ETL talks, on what should you do out of college in terms of a job that you should pursue. And I'm just gonna ask this question because you do have such a deep McKinsey background and a lot of the undergrads are wrestling with, "Should I take a consulting job out of undergrad or should I jump into a startup or should I jump into a scaling stage company?" What advice would you have for somebody who's wrestling with... They have all those options, what should they pursue? - You know, I think it's a very personal choice. 00:18:44,790 I think in my generation, you know, consulting really offered this opportunity to work and build pattern recognition across a lot of different industries. And I think you can still do that there. At the same time, I think the way in which businesses are being changed and a lot of the marquee companies of this generation, you can also learn that within a Google or some of these other companies as well, in terms of working on a lot of different problems at the same time, I just think the most important thing you wanna do early in your career, is you wanna see a lot of different business models, and you wanna work with a lot of really smart people. So I would follow your passions and I would follow people. So you know, when I waded around in those paddy fields, I was following two really famous professors from Harvard and Stanford, you know, over there, and I was the bag carrier, and I learned more by being able to tag along to that. You know, I went to run Discover Card because I knew the CEO of Morgan Stanley, Phil Purcell, and he took a risk on me to take me out of the firm and you know, have me manage 5,000 people.

I have to admit, that was baptism by fire, but I had to learn, right? And so in each case, I was going after, right? Google was about, right? A vision for the future. And I think when you do that, if you work around really smart people, they will open paths for you to do many other things, you know, in your career. And you know, that's my advice. - Okay, that's great advice. 00:20:06,360 Focus on people and bet on your own passion for learning and things will take care of it themselves. I wanna now click into this idea of how do you build deep tech ventures. Gang, I don't know if you understand how much of an anomaly Flagship Pioneering is, history is littered with people that have tried to do what Flagship has done and has failed, okay? So it's really, really a special gift that we have Margo here. I wanna understand, I love this distinction between uncertainty and risk, and swimming in uncertain waters, because it's more important to sort of not have... For all the reasons that conspire to your advantage when you are sort of the only game in town. At the same time, there's a lot of companies that get started, especially out of Stanford, that are research projects, that never actually take off.

So if a student is thinking about actually, you know, giving their life to a new venture or a new startup that is in uncertain waters, that's a big hairy audacious vision. How do they qualify that to know that it's hairy enough to be uncertain, but still tenable enough to actually succeed and take off? - Yeah, I think, you know, it's so situational to each, 00:21:15,060 and what an individual's risk tolerance is for the time, right? They're willing to put into something before they see it come to fruition. You know, we at Flagship, when we're launching a new company, we have a very simple framework. You and I talked about this the other day, which is, you know, why this, why now, why us? And it sounds so obvious, but it's actually not that easy to answer those questions. Incredibly thoughtful. So when you say, "Why this? Why is the world better five to 10 years from now because this exists? Why is it transformationally better?" Because by the way, most people forget, that in rare circumstance does something become really big, right? You look at YouTube, Google, so it took 10 years before these things were actually big. And so you have to really write the story of the future and convince people why is this fundamentally necessary. The second thing about why now is, you have to ask yourself, being too early is as good as being wrong, right? And there's a lot of things written on this. Why now? What are the fundamental things that are enabling this to be true now, that could not have happened? And then why us? Why are we uniquely the team that should be able to succeed? Who else could be doing this? And why are we the ones, why is this the combination of people, mindsets, capabilities, to make it happen? And I think that's how I would think about it. - And I wonder if we get just tactical on that, 00:22:43,170 just so that we can make that real for the students.

So because even on the why now, it sounds like there's a sweet spot where you don't want something that is immediate, because it's sort of readily evident to a bunch of people and you'll have a lot of competition, but also has to be within the strike zone where you can take off. So when you're looking at why now, how do you actually make that determination and how do you know if it's actually something that is within that sweet spot? - Yeah, so I think the why now is, 00:23:08,850 what are the accelerators? Because whenever you're going after an idea, if you think about I'm also an investor, you know, what is fundamentally creating velocity? What's creating wind at your back? Like what is the new consumer behavior or the new trend? So you know, when we... I'll give you a good example. When we came up with the concept of YouTube, we were really focused on replacing linear television, because we said, "Why do people sit and watch TV by appointments? Why do they have to be stuck in front of all these ads?" And you know, TiVo was kind of around, but you know, it wasn't that convenient and you still were stuck with when people wanted... and there was limited content, right? There's a few guys and a few places that were deciding what's... Who were arbitrating what kind of content we should watch. You had the huge adoption, right? Of the

web, of mobile, and all of a sudden we had the idea for people to be able to access content anywhere, anytime, and we were able to actually encourage that to flourish. And so, you know, I think you have to write the story of the future. Nobody should be watching linear television or content or ads. There shouldn't be a limited amount of content created, that just fundamentally makes sense.

But if those other things were not true, if the internet was not developing, if processing speeds were not getting better, if you wouldn't be able to access it at any time anywhere, right? It wouldn't have been as easy to enable that to happen. - But is there a certain sweet... 00:24:28,710 In terms of the timing of the roadmap, if you could implement YouTube in the next month, would that have been something that you would've said, "Oh that's too easily available." It has to take like 18 months or 36 months for you to actually get great, the- - I don't think it's a time to get to an MVP. 00:24:46,140 I think it's, how does it actually get better with scale? Like if you think about all the really successful companies, there's a flywheel effect, there's a network effect, there's velocity fundamentally enabling it to keep learning and get better. Otherwise someone in an adjacency will just reverse in, right? That's what everybody missed about Netflix, right? They started just distributing, right? All those cassettes everywhere, but they actually had a captive audience. So they were uniquely positioned to pivot and then invest into that business in a fundamentally new way, and the networks never saw it coming. - But, so that's assessing... 00:25:21,780 If you have an opportunity that's interesting, you wanna see if at scale, do you actually create more value? - Yes. 00:25:29,446 that has a network effect it actually increases in values, that creates lock-in. But even in the short-term then, like if I can, so like Elon Musk, you know, famously wants to go to Mars with SpaceX.

So if we just use that as an example, but that vision of going to Mars might be 30 years away. You guys can correct me if I'm wrong. And so what he's done is he's sort of separated that out into these intermediary markets- - Correct. 00:25:55,110 and thinking about, you know, these might be in like 18 month or 36- - Correct. 00:26:02,640 You know, he had cars, now he's opening up autonomous vehicle a marketplace. Do you think about that as de-risking or I guess what I'm- - That is fundamental to going after deep tech, right? 00:26:16,200 I think life sciences is no different, right? It takes 10 years to get a drug to market. So if I think about it in my own company, like how do I apply this. In our own, we have this vision of how do we make small molecule drugs more predictable, but it'll take me a really long time, it's not really proven. And to get all the way to the clinic, that takes a long time and a lot of capital. So just to put it in perspective, it's similar to building these big tech platforms.

Call it, rounded off, a billion dollars in 10 years to get one drug, one drug through the clinic, before you even make a dime. So you have to really think about, how am I gonna manage risk in that context? And the way we do it is a couple of things, right? The first thing we do is we prove that we can scale this new type of chemistry that's more diverse and prove that it's valuable. We have to prove we can build AI that can interrogate it, but that AI can be used for other things, right? I can actually fill up the drug pipelines of partners. The other thing I did was for the first shots I took on goal, I used only clinically validated targets. So I know that I don't have to take biology risk, I just have to find a molecule that works. And if I do that, even before I get to market, a pharma partner will be super interested in it. So they'll give me capital so that I can actually keep scaling the platform, and so it's really how you break the problem down. Just like Elon knows in going to Mars, he knows that we're under invested in our development, there's huge government funding, they need him to make these rockets and do these other things. There's real funding sources as he develops interim technology on the way there, that he can tap into, that can help fuel the development of the company in the future. That's why you can say, "I wanna replace linear television with YouTube." But you started just focusing on people watching...

A billion people watching as many minutes as possible. Then we figured out what content was really interesting to them, and then we stepped on the gas and attracting more of those content creators. And that was a very purposeful decision early on. Did we focus on minutes per session? That totally changes how you build the technology, versus as many people in as many parts of the world watching as much types of content and as many people creating content as possible. It completely changed the arc of the investment of the company. And so you have to make those choices purposefully. - Can we just dial into that first phase? 00:28:37,890 How do you decide what to scope for what you're gonna get done? And does that look differently than, you know, scoping out, you know, a project at sort of a steady state execution company? So I guess what I'm asking is, there is a school of thought that you take the biggest risk and you de-risk that first, and that really, venture capital is about taking the least amount of dollars to create the most de-risking. - Yeah. 00:29:01,800 or do you think about it in a different way, and how do you decide the one metric that matters in that first phase? - So I do think it's different in different situations. 00:29:12,870 I think if we just take deep tech and focus there, I do think people are looking for, risk, the fundamental principles on which the technology is based.

Sometimes it's one thing, as I gave in the case of Montai, it's multiple things, right? I have to prove that this chemistry can be aggregated at unprecedented scale, and that I can use computation to curate it, because only 1% of the molecules that we chronically consume are bioactive. So just 'cause I could say I have a lot of 'em, no one's gonna care if I can't find ones that are bioactive at scale, right? The second thing I have to prove is that this is not chemistry that drug companies are used to prosecuting, right? It looks a little scary, right? These are a lot of stereochemistry structures that are very different. They don't pass the Lipinski rule of five. How do I prove to them that this actually can be easily made into a drug, and that you can actually make it cost effectively. And so I have to prove that, right? And so there's like three or four things that I identified that I had to prove, and then I'm on a really rapid path to say, how do I get value creation proof that that's possible. And I think that's how you manage in these deep tech environments. You go to your first principles, and then you prove them

systematically and you try to do it in an elegant way that it becomes stacking, on the next one. - So I wanna make sure that everybody gets this. 00:30:33,870 So you identify the drivers, the key drivers, the fundamental drivers, and then are you thinking about the least risk that you can take to prove evidence against those drivers for the first phase? - I think you try to take as much risk off the table 00:30:47,826 as you can. - You do, okay, so you're trying to maximize the de-risking- 00:30:52,860 - Maximize the learning to the ultimate outcome, right? 00:30:54,000 In my case, I want to prove that I can efficiently turn this chemistry into a great drug- - Yeah.

00:30:59,940 - Efficiently. 00:31:06,090 And I think we don't spend enough time breaking that down. I think that's what we're really, really good at Flagship, is we have like rooms of people that will sit there and argue about that. We had the same thing at Google. - And so how does... 00:31:14,520 So just to make this really real, and I know we'll get to questions shortly, with a YouTube example, you have this vision, you know, the world's changing, there should be a world in which YouTube takes over, which is the world that we're in today. But you have to decide early on what's the north star metric. - Correct. 00:31:35,340 - Yeah, so you know, we were... 00:31:37,110 You know, we were a loved platform.

We had to re-engineer it, 'cause it fundamentally wasn't scalable to be distributed everywhere. A lot of people don't remember that YouTube was like a huge percent of the entire traffic of the internet. So one of our biggest problems to solve was actually being able to get that thing to people and have it not just like glitch on you all the time. And you had to be able to do that where people had access to very different types of speeds, of technology. So there was that, you know, and then we also had to get people to be willing to create for this thing, because it was just a bunch of random guys. And that was kind of... That would only take you so far. We had to get like more different types of creators. So we literally had to create studios and put people in front of cameras and, you know, to make it easier for more people to create better quality content. So the north star metric that led us to that, was an obsessive argument about what would ultimately convince the world that this was the better way to go.

And we took the reverse of what was obvious. If we could say a billion people were watching YouTube every day and billions of minutes, then people would have to say this is a thing. Like you couldn't deny that people actually wanted, and that we had to redefine what people thought great content was, because we realized as long as they thought it was a professionally produced, right? Hour long segment with 20 minutes of ads, we were dead, right? Because that wasn't what people really wanted, and it didn't allow consumption anytime, anywhere. So we went back and forth and we said, "We're not gonna think about programming, we're not gonna think about individual experiences and how long we can keep 'em on. We are literally gonna just focus on a billion people making this, and as much diversity of content as humanly possible, because we don't know what people really want." And then we can learn and reverse into, what is getting people to come back the most often, what do they want more of? And we actually would've been totally wrong if we would've guessed in the beginning. People loved those how to videos, like loved it. Like, 'cause they could go there and fix anything. People loved comedy, like little skits, and they loved like very simple things that were entertaining. They loved like the highlight reels, you know, from... And we could literally, if you were watching...

And you could do it today, if you're watching the Super Bowl, like whatever is important on that, you can see it literally being searched for and watched on YouTube in like within seconds. You know, it's really fascinating, right? You become a completely different way of thinking. - I think that's such a good example. 00:34:10,320 So I just wanna make sure everybody understands that. So you understood the drivers that were critical for this vision of YouTube and then you said, "It's a two-sided marketplace. There's viewers..." Or three-sided, multi-sided, but the most important... For that first phase, you could only focus on one, and you focused on, you said if you had a billion views, everything else would be solved for or you could then get everything else. And so that was the objective function initially for the first- - Right, and then everything 00:34:33,990 has to keep aligning to create velocity. So imagine, when we are trying to create ads on YouTube, the user was the king, the user and the creator. And so we had to make ads that were skippable, because if we didn't do that- - It would violate- 00:34:53,250 So you have to make sure everything aligns.

And I think that's not something... There's a concept actually I've talked about with a lot of entrepreneurs, which is, there's a difference between being a first mover and a first right mover. And I think we often don't actually interrogate that deeply enough. So think about Google, they were not the first search engine. They were not. Like, I think there were four or five. - I think they were 12th. (both chattering) - They were way down the list, but they were the first ones 00:35:25,577 who actually understood an ecosystem return, where everybody wins. There was no friction, right? They were not a portal. Portal fundamentally had decreasing value to the user.

But if you actually, with every single question that was being asked, I got smarter, I got faster, I got better, and I never bothered you. It was just a plain white screen, it was whatever you wanted, and it's still the same. That's the other thing people forget. Look at almost all the major platforms, the fundamental mechanic of YouTube, Google, Facebook, right? Instagram, they haven't changed very much. Snapchat. That simple core idea and how it could be, right? Enhanced, get smarter and smarter and smarter. But as the user, you don't have to deal with that. It's all in the background. It just gets better in ways that make it more immersive and valuable to you. And that sense of...

People forget about that, complexity kills speed. Probably the number one piece of advice I give to almost all the entrepreneurs I'm on the board of or invest in, is, you get that first version, it's really hard to get the MVP off the ground and

to get to some minimal scale, after that, like the simplest possible one or two things that are driving your entire momentum and let the rest go. And because complexity kills speed. - Complexity kills speed, that's so good. 00:36:53,220 Margo's on the board by the way of McDonald's, AppLovin, like such a wide variety of companies. I wanna open it up for questions, gang. So please, everybody raise your hands if you have questions. We're gonna grab a few questions at first, so if you might have a question just raise your hand, and then we'll try to get through as many questions as we can. Thank you. - Hello, thank you 00:37:12,090 for your talk and the insight.

So I'm an engineer, you know, so I've been... My whole career is engineering and I've been interested in bio just lately and I find it very impressive. But also I find a lot of questions and I also feel that it's very simplified. Well that's an outsider point of view. Like what we understand is not as much, is my feeling, right? So how do you deal with this uncertainty when you build these companies? Because I just feel that it's really hard. You know, like 90% of drug companies... Well, drug candidates fail, right? So that kind of like tells you, you know, the rate of success and what's going on. So I just wanted to get your insight into, you know, there's a lot we don't know about biology, and still we want to build solutions and how you deal with this high uncertainty as you said at the beginning, right? Because we're literally blindfolded, so I just wanted to get a little bit more of your insights behind the companies that are public or whatever is successful today. - Yeah, I think that most people 00:38:18,090 that are outside look into this industry have no idea how tough it is. Like most of the major drug companies, Pfizer, Merck, Sanofi, BMS, Bristol Myers Squibb, you know, they're based on like five or six drugs period.

Like that's what drives the vast majority of their revenue, and almost all the value in that industry accretes to the asset. That's what makes it so hard versus other industries to structurally amass the data, because it's really hard to get paid for that fundamental generation of data-driven insight the way... You know, think about Intel or Nvidia, right? The chips really do well in this industry. That has not been true historically in life sciences. All the value accretes to the asset that goes into the clinic. And so that has made it much more difficult to fund the stacking of knowledge, plus knowledge is very siloed. So when you think about the ability to look at all the data that, as you said, on the web and build these large language models, you had a lot of places you could go to get that data. You have a lot of walled gardens, right? In the biology area and less incentive, it's a very artisanal process. That's also for me, the reason I'm doing it, because when we think about the problems that we solve in humanity, that motivated me to do Montai, it turns out one of my kids has this really debilitating chronic disease to which there is no great solution. And I think there's no better motivated entrepreneur than a mom on a mission.

And for me, when I looked at this industry when I was working at, you know, at Ancestry, I said, "There has to be a better way," as you said, 10% odds at a billion dollars. Like just think about the value creation upside from thinking differently about how we approach this problem and use computation. But you are not wrong that we still do not know so much about human biology, and everyone in this room, we are very heterogeneous. There's not that much that differentiates our DNA, but how our bodies actually work, what we eat, the environment, right? All the other things around us that we're exposed to and how they respond, has a lot of intricacies that we're just beginning to untangle. And this is one of the reasons why we look at this as the biology century, because we're now able for the first time, to amass the fidelity of data at scale and be able to interrogate it, so that we can actually start to untangle some of these mysteries for the first time. It would be wonderful if all... I would love it, you know, as a tech person, right? If all the pharma companies in the world shared all the data from all their clinical trials and you are required to post that and share all the details from those trials, we would make a lot more progress faster. Unfortunately that's not required. So in the meantime we have to find other methods. And so you can imagine, that's exactly how we came up with our hypothesis at Montai.

How do we start with chemistry that's already been chronically in the human body? We already know that these scaffolds can be tolerated on an ongoing basis. If we're gonna create a chronically dosed medicine, I sure as heck would rather have something that I know and have already been in a person, often for a long time. But then I have to figure out, how do I precision match those into biologic pathways, and that's where AI gets really interesting in understanding the chemistry and doing that precision matching. By the way, it's actually how drug development started in the thirties and forties. We serendipitously discovered antibiotics and all these things, but it was serendipitous. What's awesome about AI is we can now make it predictable. So you have to take a really hairy problem like that and then try to find a way to break it down. And it's also why I only am pursuing clinically validated pathways in the beginning. We already know that there's, for example, a biologic medicine, unfortunately in chronic diseases, take just inflammation and autoimmune, we spend \$165 billion, billion, excuse me, billion dollars, 65 billion of that goes on biologics that go to less than 10% of the patients. And yet we have 60% of our country that suffers from at least one of these chronic diseases.

We gotta make pills, pill based format, so we can make these more affordable, accessible and help more people. That's what we're trying to do. And you have to like... You gotta break it down, you gotta cut some of that complexity off. - That's awesome. Next question. 00:42:34,440 Audience Member Thanks for the great talk. 00:42:38,220 I really loved what you talked about uncertainty, and from what you just described, there seems to be a lot of uncertainty inside of the health and drug discovery space, but also seems like this, like AI and drug discovery, this field is very, very hot right now. So I'm just wondering like what would be a competitive advantage that can help a company clear out uncertainty in this field? - Yeah, it's really interesting. 00:43:09,690 There's so many different ways it can be applied.

There is a lot of work going on right now to begin to solve these problems. There's everything from digital twins in clinical trials so that we make it easier to do trials. There's stuff we're doing on the front end to try to discover or optimize molecules

more efficiently. There's many, many different applications. I think if you're really interested in the space, right? It's thinking about what areas you really have passion for. And I think there's many applications of AI that are really just in my mind, automating what we do today and making it more efficient. What we really need is to do things that enable us to reimagine. Like what if we could combine two or three different things, like we're doing the chemistry with the AI and understanding of pathways, right? To fundamentally... And going back to the... My hairy, hairy audacious goal is that if we can amass the world's largest understanding, now you can see my Google coming in, of all this chemistry that's been in the human body and all these pathways and how all those things connect, in 10 or 15 years, we could actually build the knowledge to take your detailed RNA-seq data, know exactly what's wrong with you.

Because what I found really crazy, having gone through this in my own family is treating people as trial and error. Like you go to the doctor and they're like, "Well, if I look at your symptoms, I think this is what you have," especially in a lot of these chronic disease. Try this, if it doesn't work... They call it steps, you try that, if that doesn't work, they give you something else and you go through this, that's crazy. Like we should be able to take your detailed RNA-seq and say what's exactly wrong with you and what is most likely to get to that outcome, not have to go through all those steps. But unless we amass this knowledge, we have no chance of getting there. And so we have to think about... But if I said... If I went around to investors and said I was doing that, they'd think I was crazy. It's like if we'd said YouTube was gonna replace linear television, we said inside Google.

We did not say it outside Google. - And that's exactly... 00:45:07,290 I love this, and so if you have that vision, which is a beautiful vision, of how healthcare really should occur, and how do you solve the chicken and egg problem with funding? So if you go to VCs and you have a big hairy audacious goal, they're gonna ask you to come up with more validation points before they're gonna fund, but you need the funding to get the validation points. If you're not in Flagship Pioneer and you don't have \$3.4 billion of capital to use, how do you overcome that chicken and egg? - Yeah, so I mean, as I said, you know, 00:45:35,523 I am benefited by the fact that I have a benevolent long only lead investor. So Flagship will lead every round. They're still the largest investor in Moderna, after what, 15 years. So that is a stability creator when you're trying to do something, you know, really audacious. But it's not that they're not other people that are equally audacious in their thinking, but we still have to raise money from outsiders by the time we get to our B round. And by then we have to, you know, and that's one of the reasons... If I have clinically validated targets and literally every one of my targets is on the hot list for every pharma company.

So if an investor looks at that, they say, "Whoa." And then I can show comparables that any of those could easily be sold for 4 billion a piece. So I got none of these moving, you're taking a bet if I can't make... If you believe in what fundamentally I'm doing, you got nine shots on goal and any of those would be worth a billion dollars. Would you like to gimme a little capital? You know, and you've shown enough proof on the way that you can actually enable that to occur. So you do. That's why I said you have to take some risk off the table. If I said, "Oh, I'm exploring pathways and I'm finding these new innovative ways to treat drugs and I'm doing all this new chemistry into it," they'd be like, (yawns), "Come back. Come back when you've figured out a couple of these," right? And so there's a lot of good ideas that don't work just because there's so many things that have to go right - Yeah. 00:47:04,590 That they run outta money before they can prove that. - Okay.

00:47:08,250 I could keep spending like another hour, but I know we're down to our last minute, so is there a burner question? Is there somebody who's got a burner that really wants to get that question answered? Yes. Right there. We need to pick it up for the mic for YouTube. We only have 30 seconds, so if you can be concise, we'd be grateful. - [Audience Member 2] What's the biggest mistake 00:47:29,190 you've made in your career? (audience laughs) - Oh, good question. 00:47:33,333 I think for almost all leaders, I'll give myself, you know, two, that I relentlessly focused on. The one is, not making things simpler. You know, I've gotten better at this over the years, but I am relentlessly focused on simplicity, and how do we actually take friction off the table and take, right? Not make the goals too complicated. And as I said, you know, silos kill speed, right? Complexity kills speed and agility. And so making those decisions.

I think the second is waiting too long on people. You know, I think that's the hardest thing as a leader is how do you make sure you have a team that works really well together, and there's a lot of really smart people, but sometimes they just aren't great teammates, and you gotta make those calls quickly, because you can lose a lot of time if they're in a pivotal role. I would say those are the two big ones. - Okay, that's great. 00:48:27,803 Well, on that, Margo, I have to cut and end this week's ETL. Everybody, let's give Margo a big round of applause. (audience applauds) Gang, next week we're gonna be joined by Ali Ghodsi, the co-founder and CEO of Databricks, and you can find this event, other future events in this ETL series on our Stanford eCorner YouTube channel, and even more videos, podcasts, and articles about entrepreneurship and innovation at Stanford eCorner. That's ecorner.stanford.edu. Thank you, Margo. - Thank you.

00:49:01,543 (audience applauds) (gentle instrumental music)..