EPISODE 1527

[INTRODUCTION]

[00:00:01] SF: Ashmeet, welcome to the show.

[00:00:03] AS: Hi, Sean. Thank you.

[00:00:05] SF: Yeah, thanks for being here. You're actually my first ever video interview on Software Daily, so that's very exciting. You've been on the show before, but I don't think we've really dove into your background. You've been an investor for a long time, but what were you doing before you went into investing?

[00:00:22] AS: I love being first in anything. Before being an investor, I was at VMware running product management for ESX server. It was quite a big transition to go from a pure operating role directly into venture.

[00:00:41] SF: Mm-hmm. Then, what made decide to go down that path? Why become an investor?

[00:00:47] AS: The history of VMware is that, we had a wonderful start, sort of follow the venture trajectory in terms of growing very rapidly. But the company was sold early, too early, in my opinion, in its lifecycle. We became a subsidiary of EMC. So, I was a little frustrated working for a large company here, arguably running what was going to be a huge product, even bigger than what it was when I was running it. So I decided to leave with the intention of starting another company. I had some ideas in the back of my head, and I was just kind of thinking, "Okay. It's time to go try some things." In that context, I started talking with some VCs with the intention of saying, "Hey, at some point, I'm going to need to raise money." So I wanted to have a conversation with them.

The nice folks at Foundation Capital, Mike Schuh, Kathryn Gould, Bill Elmore, Paul Koontz, all of the folks who were there, they offered me a position as an EIR, as an entrepreneur in residence, to just work on my ideas and start a company. Then, surprised me with an offer a couple of months later, saying, "Hey, why don't you try your hand at venture?" It was a complete surprise. It was not planned in any way. But I'm very grateful to them, because in many ways, they changed my life.

[00:02:07] SF: Yeah. I mean, how's that transition, I guess, from someone in product as an operator, then you're thinking about starting your own company, and then you ended up becoming an investor? Imagine that's like a fairly different style work than being an operator of a business, for example. How's that transition or how was that transition for you, I guess?

[00:02:30] AS: It's a completely different way of thinking. When you're thinking as an investor as compared to when you're thinking as an operating person. So it is a transition, and it is something you have to think about actively. However, the raw material that you use is the same. If you think of your experience as consisting of many Lego blocks, you have some experience, you have some education, you learned some tasks, you went through some interactions. Those pieces are clearly the same, but you're going to assemble them in a different way for a different outcome. That's really how I described that transition. You have to think about it actively.

In my case, I had mentoring. Specifically, for example, with Kathryn Gould. For the first couple of years, I went to every single board meeting of hers I would just sit along, sit with her. As soon as we would come out of the board meeting, she would say, "Okay. What did you think?" It was this instant, "What was your reaction?" I would, of course babble on with a couple of ideas and say, "Well, I thought this. I thought that" or that's number one issue." Then she would look at me, and she would say, "Well, what about this?" It will invariably be this incredibly insightful single observation, which will encapsulate sort of the fulcrum of what I think the discussion as an investor was relevant to that point.

Investors have much more data, much more behind, much more information, much more analysis, much more ambiguity with which they are dealing. But they have many fewer decision points that they get to make. It's a completely different way of thinking about the problem, and approaching how you work. I was lucky to get that mentorship, and you slowly make that transition, and then it becomes just a natural way of thinking. The second point I will say about it is, that often things that are obvious in an operating role can have counterintuitive meanings, the so-called contrarian view when you're thinking like an investor. Here's the simplest example. You jump on a phone, you're going to hire someone, you're doing a reference call. The first thing the guy says is, "He's really hard to work with." That's usually a disqualifier if you're going to go hire someone.

Well, if you're an investor, that could be a very positive sign right there. A great entrepreneur is often someone who is very hard to work with. No one said that they loved working with Steve Jobs or he was the easiest person to work with. Obviously, one of the greatest entrepreneurs in the world. Things can often have the opposite meaning in investing from operating. That's another thing that you kind of have to reset your thinking on when you start thinking as an investor.

[00:05:13] SF: Is that something that is a skill that you just have to learn on the job from experience? Or is there another way to sort of acquire that skill, because, like you said, the types of decisions that you're making, the types of information that you're taking in as an entrepreneur, or even as an IC, or manager at a business is going to be very different. You're looking at those things with a different lens than as an investor.

[00:05:36] AS: Like any skill, I think it's a combination of education and experience. In other words, to become a great basketball player, it helps to watch Michael Jordan, or Steve Kerr, or whoever, who has played. But you have to eventually go play the game yourself. You cannot become a great basketball player unless you go and play basketball. Just like playing a game, I think it's a one to ten ratio. You have to work 10 times as much yourself, then one times on educating yourself, watching other people, learning from other people's experience. Overall, it's a game of experience. Absolutely.

[00:06:13] SF: Yeah. I mean, I think that's a great advice basically for anybody who kind of wants to do anything if you want to be great at it. You want to be a great engineer, you can't just read theory, and have a computer science degree. You need to go and actually be implementing things as well, and that's really how you kind of learn to navigate that world. I've raised money in the past from VCs and angels. I think, from my experience that, different people and firms have sort of different approaches to investing. What would you say is your sort of investment thesis?

[00:06:46] AS: Yes. This is really good to understand if you're an entrepreneur, because entrepreneurs seem to sometimes think that all investors are the same or looking at it from the same lens. In my particular case, I found a niche in Silicon Valley, investing in companies that are taking high-technical risk, and so that's what I focus on. At a high level, of course, I'm a VC looking like any venture capitalist at a large market, capital efficiency, high growth rates, high barriers to entry. So those factors remain the same. The real differences come in. How do you get those barriers? How do you achieve those outcomes?

In my case, I am focused on companies which take high technical risk. Here's how I describe that. Most VCs try to answer the question, "Will the customer buy this product?" At least at the early stage. I'm talking about early-stage investing, not growth stage or later stage investing. The question the VC is trying to answer is, "Will the customer buy this product? How much will they pay for it? What's the customer acquisition costs? What sort of gross margins will I get? How big is the market?" Et cetera. That's clearly an important set of questions, and that is the classical and I would say the most common way in which VCs invest.

I'm looking at a subset of the companies where the question is, in fact, quite different. Which is, can you build this product? We already happen to know that people want to buy it. We already happen to know that it's a large market. We already happen to know that it's an unsolved problem. But we don't really know how to build it efficiently, cheaply, usefully in a way that can be commercially viable. I would say that's a small subset of companies. It's not the large subset of what goes on in Silicon Valley. But that's why I call my firm engineering capital, venture capital for engineers. It's companies where there is a technical risk, a technical insight that a founder is going after.

[00:08:40] SF: How do you or other people in your firm, I guess determine that a particular company or investment meets your bar, I guess, from a technical risk standpoint? Because you can't be an expert in everything. How do you actually know whether this is a problem that is really, really difficult from a technical perspective?

[00:09:01] AS: What a great question, Sean. It also relates to the way we started the conversation in terms of thinking like an investor versus an operator. An investor never needs to be the best at the problem that has been solved by the company. Because really, if you are the best at solving it, then you are much better leverage going and actually building that company yourself. You will make more money, you will do it faster, you will increase the odds of your success. All of those benefits will accrue if you really are the best in the world of doing it. You must start from a position of saying, "Okay. I'm not the best at this, but am I talking to someone who could be the best at this? Who could really be one of the top 5, 10, 15 people in the world at doing this?" That's kind of the bar that you're looking for a venture style outcome.

If there are 1000s of people who know how to do what you're doing, it's clearly not an interesting problem, from a venture perspective. If it's hundreds of people, it starts becoming a question mark. And when it is 5, 10 people, then you really know you've got something very exciting. That's one way of thinking about the bar. The other way to think about it is, does it have disruptive potential? In other words, does it change the flow of funds, the market, the positioning, technology, et cetera in a way that would be fundamentally different? I mean, as I mentioned, I used to be at VMware. In the early days of VMware, I used to run around telling people, "One day, there will be one million virtual machines in the world." People would laugh at me, and they would say, "Oh! What have you been smoking today?"

Today, there are not millions, but there are billions, or maybe perhaps trillions of virtual machines running in the world. It was clear to me that it was a radical fundamental change that we could introduce, if we could make that successful. And you have to think similarly, when you look at a company, that's what excites a VC and say, "Wow! If this was to work, would it have real disruptive potential in terms of changing the dynamics of the market?"

[00:11:08] SF: Yeah. How do you balance having a real disruptive potential, and a problem that could legitimately be solved versus something that's just like too out there. Like I could say that I'm going to develop true AI. That obviously would have huge impact on the world. But that is probably, based on my understanding of where we are in the AI space, would require a tremendous amount of new innovation to get us there. We're probably a long way from that.

[00:11:38] AS: Yeah. That's a great example of something where I consider we have science risk. In other words, that's not technical risk. Science risk is that you have to do through invention. You have to do true science. You're better off doing that type of work in a university format, perhaps at a research institute, perhaps at a university. That's the right way to approach it. It's very rare for you to be able to do that in a venture-backed company. You might be able to do it in a large corporation, especially in the old-style way in which research was funded by corporations. I mean, if you think about the invention of the transistor. That was a very specific research program that was started by AT&T to solve a particular problem that they were having. It's a great example of a technical insight, of where the problem was well understood. They knew exactly what they needed to do, they just didn't know how to do it. So they started investing very heavily into Bell Labs. Many Nobel Prizes came out of it. Obviously, the transistor came out of it, which is arguably one of the world-changing inventions of the 20th century.

That's what I would call science risk. Venture-backed companies cannot afford to take risks like that. At the other end of the spectrum, you have the trivial examples. In other words, any good engineer could implement that idea. I call that enterprise software. It's a great idea. There's probably a market need for it. If you want to build the next Salesforce, great. You can build a very large business and a very large company if you can execute on other domains. But you don't have technical risks there. Somewhere between these two extremes is the sweet spot where you have an idea that is not so risky. Doesn't require so much invention that sciences, and yet it's not trivial, in terms of any engineer could implement it. That is the sweet spot for venture investing.

Another way, another thumb rule that I like to use is, can you get to revenues with the two-pizza team in a year or two? That's another way of thinking about it. Which is, 5 or 10 people working for a year, working for maybe 18 months, two years at the most. Could you get to real revenues? Doesn't have to be the final product. Doesn't have to be something that is the full vision of what you have in mind. But it will give you true honest to goodness market feedback that you have built the right product over there. Those are the sweet spots for me at Engineering Capital, and I believe for building disruptive venture-backed technical insight companies.

[00:14:05] SF: Yeah. I could come up with a great idea. Maybe it needs this framework of technical risk. But if I can't build a team, I can't make them believe in my vision, I can't have them follow me in the battle, then I'm going to fail as operator of business, like essentially, ideas are cheap. Startup successor is really about execution. How does someone who's pitching you, or how do you personally assess that the founders of the business are someone who can actually go from idea to execution?

[00:14:41] AS: That is arguably the million-dollar question or perhaps the billion or the 100-billion-dollar question, because it is subjective. By definition, it will have contrarian elements in it. In other words, you will get conflicting data. You'll get data as in, "He's hard to work with, but he's very intelligent. Or he can be stubborn, but yet he's very ambitious." Those things tend to contradict each other in terms of how people behave. My job and the art of venture capital is to figure that out, is to make that decision, to make that choice. Let's also recognize that we often get it wrong. In fact, more often than not, I am wrong. That is just the reality and the nature of venture capital in this very early-stage business.

Recognizing that we get it wrong, I have the luxury of making mistakes. I can err on the side of saying, "Okay, let's give this a chance. Let's give this person a chance. Let's give this opportunity a chance." If I believe in the technical inside, if I believe in the person, that's usually enough to get started. Then the market speaks for itself, if you can actually execute. You are absolutely right, Sean. Ideas are cheap, execution is expensive. It is really all about execution at the end of the day. The idea gives you an unfair advantage. The idea gives me a specialist practice where I have an unfair advantage as a VC. But it's not a prerequisite to success. You can build great companies without technical insights, with great execution and have an impact on the world. It's definitely not in necessary condition. It's not even sufficient, as we've just recognized. Because you have to pair even a great idea with great execution, if you're going to actually have an impact on the world.

You absolutely have to solve all the problems that a regular company solves in addition to solving the technical area if you're going to work in a company like this. It is an art, it is a package deal, and you have to approach it as a learning effort. The last thing I'll say on it is, it's also like playing a game. If you watch basketball, if you watch soccer, if you watch football, the person who becomes the greatest player is often underestimated early on. It's the same thing in venture capital, because that's human. That's human nature. We cannot understand another person's motivation, ambition, grit, perseverance, even intelligence from the outside. We think we can, we have some measures, but it's not a perfect science by any stretch. We are always surprised.

[00:17:14] SF: You're right. It could actually be the impact of being underestimate as an individual that pushes that person to ultimately be great. If you look at someone like Steve Nash, grew up in Victoria, BC, went to kind of a nothing College, Canadian NBA player becomes essentially MVP multiple times is an incredible story. He came from rather humble beginnings of, there is not a lot of superstar basketball players from Canada at that time. You mentioned this idea of how something like a technical insight can give you an unfair advantage or an idea can give you an unfair advantage. Can you talk a little bit about how essentially some technical insight could give a company that unfair advantage over anybody else that enters such space or is interested in solving some more problems?

[00:18:03] AS: Yes. There are many unfair advantages that accrue if you have a technical insight which you can pair with a market need. The first and most obvious unfair advantage from a venture perspective is that it gives you a very high barrier to entry. If you've truly solved some technical problem in an interesting way that nobody else has figured out. By definition, you've got a barrier to entry. Perhaps you'll get some patents, perhaps you'll keep it as a trade secret.

At VMware, when we were selling virtualization on the x86, nobody believed it was possible. There were peer reviewed journal published academic papers that had proved that you cannot virtualize the x86 when we were out there demonstrating that you can virtualize the x86. Again, I'm not taking anything away from Professor Popek and Goldberg's paper, which proves the opposite. Mathematically, they are correct. But commercially, we had built a solution, which was incredibly valuable. Today is a $50 billion company. The first thing you get is a barrier to entry. You get something which other people can't easily copy, which means, number two, you get multiple chances to succeed. You get to practice, try, iterate and figure out a go-to market multiple times. If you build a company with a low barrier to entry, you don't get many chances. Because if it is really a great idea, if it is a great market need, then other people will copy it immediately, because you have a low barrier to entry.

By having a technical insight, you get multiple chances to iterate and win with it. That's the second unfair advantage you get. The third unfair advantage you get is, because it has a high barrier to entry, because you have multiple iterations to succeed and find the success, you get very high valuation multiples if you play it right. At the end of the day, whether we go public, whether we exit in an M&A. As a VC, I'm looking eventually to sell. I have to do that. A founder may want to consider the life's mission and continue to do it forever, but I have to sell at some point. Therefore, I do think, how will this be valued? There also, you get an unfair advantage if you have a true technical insight, you build a company with a core technical advantage.

Last one that I'll mentioned and – I could go on and on on this topic. Is that often, if you have some good technical insight, and you've done that, you will automatically attract the best people in the world. The best people in the world who want to work in that area are obviously interested in that area. Therefore, it tends to have this virtual cycle which builds on itself, and it makes it easier for you to build that company. There are second order or tertiary advantages that come out of having a deep technical insight and really becoming the expert in some new area. Because you've invented a new space, you've invented a new approach, the people who invented are yours. They would work on it for yourself, with you. So of course, you would have an advantage in terms of having the best minds working on it.

[00:21:10] SF: Right, absolutely. I mean, I think A players attract other A players is a cycle that you tend to see. Do you ever see the situation where someone's essentially takes something that's really technically difficult, have some insight and they're actually able to solve it? Then, once someone has proven or shown that this can actually be solved, then that makes it so that the follow-on companies can actually figure it out. Like if you take something like from sports, the four-minute mile, for example. People believe that that was impossible for humans to run less than a four-minute mile, and then Roger Bannister broke the four-minute mile barrier. Now, I don't know what the four-minute mile record or the mile record is now, but it's probably under three minutes. It's like three minutes and 40 seconds or something crazy like that. Everybody basically breaks it, that at an Olympic level now. Is that something that a trend that we also see from a technology perspective?

[00:22:11] AS: Absolutely, yes. In fact, I would go so far as to say is that, the moment you talk about what you're doing, immediately, people will test it by saying I want – can I figure out a way around it? Do I want to do it? Et cetera. So yes, the game is not over just because you had one great idea, and you had one great implementation. This is a continuous game, where you have to keep playing better, and better, and faster and faster if you're going to win your way to the top of the mountain. So yes, people will come after you. Again, we could use as a technical example, your four-minute mile example is a fabulous one. Once the psychological barrier of the four-minute mile was broken, people push themselves harder, and they achieved greater things, and they were able to run faster.

Same thing, technically. Once you've described the idea, implementing it for the second time is sometimes actually easier than implementing it for the first time. For example, I happen to believe that in the quantum computer space, we will see this approach. I actually consider that a negative as an investor when I analyze the space and I go, "You know, that's a really powerful problem, if you could build a quantum computer." But whoever builds the first one, whoever's trying to build – that would be hard, and that'd be great. The guy who's building the second one is going to be able to infer so much from what the first one looks like, even if they don't have any of the patents, or information, or technology that they will get an advantage, and they'll be able to copy it.

Copying is often easier than being first. There are many advantages to being second. I mean, Microsoft built arguably one of the most valuable companies in the world by always being second. But being a fast follower and copying many of the ideas. That definitely happens. But those are the rules of the game. The rules of the game are, you will get copied, you have to get better.

[00:23:58] SF: Yeah. It also doesn't – just because you have that insight, it doesn't take away from the other part, which we talked about, which is the execution. You mentioned before this idea, as an investor that you're going to make mistakes. You're going to believe in something that just doesn't pan out. When you miss an investment opportunity, maybe you chose not to invest, and then it did become something big. Or you invest in something that doesn't work out. How do you actually learn from that? If you look at like engineering, or other teams that you're part of, maybe you're running post mortems or reflections on a quarterly basis. What went well? What didn't go well? How do we improve? Is that something that you're actively doing as an investor as well?

[00:24:39] AS: Absolutely. Absolutely, yes. You have to constantly think about how – I have to think about how I am improving my own game. In other words, my game is to invest. How do I keep improving that? Which means, I have to think about all the mistakes I made in the past. Also, all the things that went right in the past, and then see what does the future look like. Because future does not look exactly the same as the past. So it's really important to not over rotate on lessons from the past, but you still have to learn from them, you still have to apply them and do it. I'll give you a great example of that. Once, in my early part of my career, not as a part of Engineering Capital, we had a founder who committed fraud, who actually stole money. Now, I know there's a lot of press coverage these days about it also. This is not a unique instance. It does happen in venture capital.

I learned one of the greatest lessons in venture capital from my former partner, mentor, Bill Elmore, founder of Foundation Capital. Fortunately, it wasn't me. It wasn't my investment, I wasn't on the board, or it wasn't something which I had done. So I was just learning from other people's experience. But here's what Bill said, and he had done some numbers and said, "This is the 100th company or something on some basis that where this had happened." He said, "If one in 100 times someone commits fraud, maybe that's an acceptable rate of fraud that you must accept, to be able to trust people, and to be able to work in a business where we are looking with so much ambiguity on what the future looks like. Because you cannot over rotate to such an extreme that you never, ever, ever make a mistake." If you do that, you will fail. You are guaranteed to fail. Just like when we run our customs and immigration system, and you land at the airport. Certain amount, you have to let through. Otherwise, it will become a dysfunctional system. Just like when the police work, when our courts work, you have to allow a certain failure rate. So you have to think about what is that failure rate that is acceptable to you and how do you work with that.

Now, I'm not condoning fraud. I'm not suggesting fraud is a good idea. It's obviously a terrible thing to do. It is illegal. It is criminal. That person actually ended up going to jail. Fortunately, we have a great legal system and there are consequences to things like this. But as an investor, you have to understand that mistakes are part of the approach, and you have to figure out what is the acceptable error rate that you are willing to take?

[00:24:39] SF: Yeah. Imagine that's also something that's very different than necessarily being an operator. Although there is a certain amount of trust in mistakes that has to – you have to be willing to accept as an operator as well. You're never going to have perfect information. If you wait until you have perfect information, you're going to be too slow. I think Jeff Bezos said that they wanted to make decisions about product investments when they were like, 70% sure or something like that. Because if they waited until they had more, they are more than 70% sure. They were basically moving too slow. If you're only 70% sure, of course, 30% of the time, you're going to make mistakes, and you have to be kind of willing to live with that by optimizing for speed.

[00:27:58] AS: Yes. Sorry.

[00:28:01] SF: Go ahead.

[00:28:03] AS: I was just going to reflect on some of the more recent cases. I mean, obviously, FTX has been in the news with a lot of very famous well-known firms. I am sympathetic to the decision making that they had to go through. I am not sympathetic to what may potentially have been fraud over there. Who knows, we'll figure it out eventually what the true story is. But I am sympathetic to the decision making that partners have to go through when they are trying to make an investment decision like this. It's obviously much more expensive, much more painful, and arguably more responsibility on the investor side, but it's that later stage and investment. But it's a very hard decision to make as an investor. At the end of the day, we only succeed if we can trust. Venture capital only works when we trust the entrepreneur. If the trust doesn't exist, this business could not exist.

[00:28:54] SF: Yeah, absolutely. I'd like to transition to talk a little bit about some of your existing investments. I'm familiar with vFunction, which I mentioned before we went on air. The founders, Moti and Amir were on the show recently. I know they're trying to solve a really hard and important problem, which is, how do you essentially modernize legacy applications. But what hard technical problems are some of the founders you invested in trying to solve? I guess, what convinced you to invest, especially speaking to how you realize. These are not only the people who have that technical insight, but there are people who I believe can actually execute the vision of it.

[00:29:33] AS: Are you looking for examples of companies other than vFunction, or you want me to explain what happened in the case of vFunction?

[00:29:40] SF: Oh, well. We can start with vFunction if you have some insight there and then we can go and take a couple other examples.

[00:29:47] AS: So vFunction came about because I knew Moti is a great entrepreneur. I actually approached him when he was selling his previous company, Watchdogs. I told him, "I know you're going to do another company and whatever you do, I want to be your first investor." He kind of laughed and said, "Ha ha, get in line. There's a bunch of other VCs who want to give me money also." Because let's face it, he was a very smart, tech on undergrad, Harvard MBA, had built a company sold it for enough money that he had walked away by what in the rest of America would be considered a high-net worth individual. In Silicon Valley, people may be dismissive of the success of Watchdogs. But he had done reasonably well for a first shot.

But I knew he was a great entrepreneur, I knew he was young, intelligent, ambitious and he wanted to do it. I literally went to him and I said, "Here are some ideas. Here are some places where there are opportunities where you could start a company, and I will help you do that." I was very fortunate that he trusted me just like I trusted him with the ideas. We developed the idea for vFunction together. I'll take a little bit of the credit. He gets most of the credit, but I'll take a little bit of the credit for actually helping him shape that, think about the problem, think about the opportunity. He honored me by letting me be the first and only investor in the first round of vFunction. That's how that company came about.

So I knew the problem, by definition, since I introduced him to the problem. I keep a bunch of these problems in my back pocket at any given time. Whenever I meet a great entrepreneur, I share some of them with him. Now you know the secret of when I trust someone, and I like them, and I know that they're going to build an interesting company, how I start. Then, we met Amir, who of course Moti knew from before. He recruited him as his co-founder. We brought him in. We brought in Ori. Then we started trying to solve the problem. Our initial hypothesis did not work. Okay. I had an idea in my head, and I said, "Hey, why don't you try it this way?" Then Amir, to his credit, bashed his head against the wall for a while and figured out a different approach, a combination of static analysis, dynamic analysis, some AI applied to machine learning applied to it. Now, we have what I consider to be a commercially viable, incredibly valuable solution, which is deployed in multiple companies now.

[00:32:21] SF: Yeah. In that case, you had no prior experience with Moti, so you knew sort of what he was capable of. Can we take maybe another example of an investment that you made, where these were first time entrepreneurs that were able to convince you that they were solving a technical challenge, and also that they can actually execute it.

[00:32:40] AS: Yes, sure. I can give other examples of companies in the vFunction category. In other words, where I introduce the idea to the entrepreneur. But let's face it, I'm a VC. Many times, I meet entrepreneurs who have their own great ideas. I'm very happy to invest in them. A good example of that is Nexla. Nexla was started by Saket, who was a VP of Products at Rubicon. He had been with Rubicon through the journey from a very early days of Rubicon. He was not the founder of Rubicon, but he was early at Rubicon all the way through the IPO. He was an experienced executive, very smart guy, IT undergrad, Wharton MBA, knew how businesses work. Clearly, he had the education and the experience. He came to me with the idea, which I'll loosely described, as taking an ad tech style approach for data engineering and applying it to enterprise data. Another way of describing it is real time automated data engineering. I was like, "Wow! That would be radical, if you could do it." But it's really, really hard, and he was like, "Yep, we will run sequel on a pipeline as the data is coming through in real time and allow – because it's so easy, because the enterprise numbers are so small.

I'm used to – they're used to working in the ad tech world, with this very, very narrow 50 millisecond response time, sort of latency requirement, within which all decisions get made in the ad tech world. He was like, "Oh, the enterprise, it's easy. The data sizes are smaller, and latency requirements are less rigorous, but we're going to give a much more rigorous automation solution, where we guarantee provenance, we guarantee ACID properties, et cetera, on the data." I was like, "That would be amazing if you could build it. Here we are, I'm very proud to share that it's a very successful, large company growing rapidly. They grew over three times last year. They have customers like Instacart, and DoorDash, and J.P. Morgan, and Bed Bath & Beyond, all of whom are running commercial products on our platform. That's an example of a very nice technical insight to build a large commercial business, and they are profitable by the way.

[00:34:57] SF: That's fantastic, and also a very good place to be, given the current market dynamics. I guess, what advice do you have for first time founders that are trying to raise? A lot of people that listen to this show are engineers, technical backgrounds. I'm sure some subset of those folks are going to go and start a company, or they could be starting a company right now. If you could sit down with them, they have a coffee meeting with you, how would you help them figure it out to go out and actually pitch you or other investors?

[00:35:30] AS: Well, pitching me is relatively easy. Because I'm a solo GP, I work one on one with you, I don't have high expectations in terms of pitch, et cetera. Obviously, if you prepare a deck, that's always helpful, it shows me your quality of thinking, your ability to articulate ideas, but I will invest without a deck also. That's not a requirement for me. But what I will advise a first-time entrepreneur is, spend time with customers. You cannot spend too much time with customers and customers are people who actually pay you money. That's my definition of a customer. Often, first-time entrepreneurs are a little bit confused about who's a user, who's an influencer, who talks about it. They think of all of them roughly, especially if they come from deeply technical background, they think of them as customers. Customers are people who write checks, who will pay you money, and you need to spend time with them. You cannot spend too much time with them. That's point number one.

A corollary to that is, go and meet them in person, especially in a post-COVID world, go and meet them in person. It's easy to jump on a Zoom. Go meet them in person, if possible. If not possible, do a Zoom If Zoom isn't possible, do it by phone. And if a phone isn't possible, do it by email. But you need to understand that there is no substitute for high-quality, one-on-one time with a customer. People are shocked when they say, "I should jump on a plane and go to New York for a one-hour meeting?" I'm like, "Absolutely. Absolutely, you should." If it is a high-quality meeting that you can get with that person, that is time, and money, and effort well spent.

Again, this is only applicable by the way to the enterprise deep tech space. I mean, I'm not talking about a consumer startup, or someone who is building the next social network or something like that. I don't know how to start those types of companies. Number one, spend time with customers. Number two, spend quality time with customers as much as possible. Number three, sell before you build. Sell before you build. Most entrepreneurs, especially first-time entrepreneurs start building before they have sold. No. The correct sequences sell before you build. That is very counterintuitive to entrepreneurs, but that is how great companies are started and built. Because that's how you get momentum, that's how you get velocity.

[00:37:49] SF: Yeah. I think as a former entrepreneur myself. I wish we had this conversation before I started my business, because I made all those classic mistakes in my first year. We didn't talk to enough customers. We started building before we really had enough. We thought we were talking to our prospective customers. We were to some degree, but not to the level of depth that we needed to. We basically reached a point in our company where in the first year, we were going to run out of money, and we ended up laying off everybody in our company, and essentially started from scratch, and doing exactly what you're talking about. Where we just went out, took any meeting anybody would take with us, had in-person conversations. We would drive all over the Bay Area. Didn't matter how far away someone was if they were willing to talk to us.

Then, we would go and build essentially low fidelity, like paper mock ups, or other types of mock ups and take it back to them. Then have a conversation with them and say like, "Hey! Is this something that you would pay for?" That was the only way we were able to actually be – you know, reached any level of success, it was really through that iterative, fast-learning cycles that got us to a product that we could take the market that people were willing to pay for.

[00:38:58] AS: This is why the Bay Area is so special to build companies. People forget, it's not the fact that we have a lot of traffic or housing prices are very high. That makes us such a great place to start companies. What makes it such a great place is, that you can jump in a car and actually go talk to real potential customers. In fact, I'll go so far as to say, that if you have not jumped in a car, and haven't gone and talked to a bunch of customers within the Bay Area, you're probably not building an interesting technology company. Not guaranteed. I can think of exceptions. But that's a rule of thumb that you could apply as a first order filter in terms of doing it.

Let me also give you the test for when you should stop talking to a customer. Since I'm so much of an advocate of going and talking to customers, and spending time with them, and all of that and people are like, "Well, I should start building because he took a meeting with me and he said he wants to buy it." I said, "So when should you stop doing it?" After you receive the purchase order. Don't stop until you have a purchase order, because they're not a customer until you have a purchase order. Companies can be built and they are built by having purchase orders before the product is built. That's when you know they have a real need. That's when you have a guaranteed need for the customer. Everything else is talk until that point.

[00:38:58] SF: Yeah, absolutely. Are there technology and trends in the market that you're particularly excited about right now? What would you love to see someone out there take on the challenge of solving?

[00:40:29] AS: Wow! That's a really broad question. I could take it on many different levels. One answer you've already given from the technical level, which is obviously AI. We are seeing dramatic changes and dramatic improvements in AI, all of the ramifications of which we don't fully understand and are going to have huge application. There's no question about it. Yesterday, or I think on the weekend, we saw the release of the chat API. You probably heard that, the chat GPT, API. That's just fascinating to play with it, and to see some of the results that are coming out of it. How do you use it? How do you build a business around it? What are the implications of that is clearly a very interesting area to me.

The second area that is near and dear to my heart is, broadly speaking what I call data, or privacy, or security, et cetera. All of these are related to each other. These are unsolved problems in computer science. Yes, we know we can do encryption. Yes, we know that keys work. Yes, those theoretical concepts have been answered. But at a practical level, it is an unsolved problem. When every company has data breaches, when every company has privacy policies, when the government comes after you that are unenforceable, et cetera, you know we have a problem. There must be a technical solution to this at some level. Clearly, that's a very interesting area for me. I know you work in that space. So obviously, you're making some great movements over there. Clearly, another very interesting area.

The third area that I like to talk about, by the way, broadly speaking, the area that that you are in is, I describe it as, data is the new oil. Data is the new asbestos. In other words, it's toxic, it causes cancer, and it's going to kill your company if you aren't careful in terms of how you manage your data. The third area where I see less work, but I think is potentially a very interesting area is – and I have a company working in it. Was actually on a slide that professors, Hennessy and Patterson had put as part of the Turing Award lecture, which is, what are the implications of the end of Moore's Law on software? They had this slide where they showed the stack for the matrix multiply on a standard x86 Intel chip. Showing sort of the performance improvement that was available that was just sitting for free. That's just sitting out there. We waste all of that performance. It was a 63,000x factor of performance on a matrix multiply on a standard Intel x86 that we waste for good commercial reasons, for good business reasons.

But my God, as an engineer, that's just billions and billions of dollars sitting out there just crying out for a technical approach, a better technical solution, than saying, "We will slap on yet another layer of abstraction, make our program even slower, and try to make something work." There's got to be a better way. That's very exciting to me.

[00:43:29] SF: Yeah, absolutely. To your point about data, privacy, security. I spoke on a panel a few months ago, and one of the first questions I got was, how are we doing as an industry? This was for data engineering with respect to privacy. I'm like, "Well, take out your phone, go to Google News and do a search for data breach. You'll have a pretty good idea how we're doing as an industry right now, from the perspective of privacy. We're not doing very well." I often say that if – the industry was an engineering team that I was leading, I would be declaring a code red right now, in terms of where we are from a data privacy perspective. I'm well aligned with your visions there.

You've been an investor for a long time as we've mentioned. What was the most surprising thing that you've had to learn about investing?

[00:44:28] AS: Not only I'm an investor. I'm also an engineer. In venture capital, we talk about the power law, in terms of how concentrated winners are, how disproportionate winning is, and how counterintuitive that is statistically to a human being to our human experience. Because human experiences tend to be linear and not exponential. That is something which I still think about every day. and I still feel that I haven't quite fully internalized as much as I talk about it, as much as I've experienced it. I invested in a company – my first IPO, we invested a few million dollars valuation. It was worth two and a half billion dollars. I mean, that's the classic power law distribution outcome that we talk about in venture capital. But it's still something that I think because we are so wired to be linear on our real day-to-day life experiences are so linear, that I'm still not used to. Perhaps, we never can be used to them. Maybe it's just my limitation, or it's a human limitation, I don't know. But that is the mystery of venture capital for me. It's also what keeps it super exciting, and super interesting every day for me.

[00:45:40] SF: Yeah. Imagine a big thing that would be interesting as an investor, at least, this is what I've kind of always imagined for myself. It seems like, based on our conversation, this is something that would appeal to you. But as you really get exposed to just so much stuff that's going on in industry, you constantly be learning and kind of dip your toes into these different areas. You don't need to be an expert in them, but you're constantly educating yourself enough so that you can actually make a determination about, "Does it make sense to invest?" It's a career path where the learning is really never going to stop in the breadth of information that you're sort of consuming is really, really massive.

[00:46:22] AS: That is certainly the intellectual curiosity gets satisfied, and continuously you get to fulfill that all the time. Absolutely, that is certainly a very, very nice aspect of venture capital, of being a VC in Silicon Valley on the cutting edge of technology. Every day, there's so much creativity. I still go watch seminars at Stanford. I still go listen to, sometimes PhD. Sometimes I just go listen to it, or read it or whatever. I just marvel and I go like, "There's no way I can read even 1% of 1% of 1% of what people are doing." It's just amazing, human ingenuity. Then yet, you'll be walking along, and suddenly, someone will come up with some brand-new idea, and it just hit you like a ton of bricks. That's what keeps this job so exciting.

[00:47:12] SF: How do you see startup investing changing in the next five to 10 years? I think one trend that seems to have happened in the last few years is that the global market where people are starting businesses is massive valley. It's not just Silicon Valley, there's great companies that are coming out of all over the world: Israel, even other parts of the United States that weren't traditional places where startups were founded, Germany, the Nordics, and the list goes on and on. But are there other things that you're thinking or anticipating are going to be changing?

[00:47:45] AS: I think it's important to recognize that we are at an inflection point in venture investing. And yes, we are going to see some important changes. One of them, you've already recognized, which is globalization of venture capital. You have to recognize that that is happening at a time when the world is deglobalizing. So the macro trend on world trade is actually in the negative direction. So that's going to create some very interesting set of opportunities and companies which will get created. Technical innovation continues to accelerate, despite some pessimism from some people that we've seen in the past. Computer science is an incredibly innovative area. It continues to innovate. We continue to see new breakthroughs. We will definitely see breakthroughs over there.

Here are the ways a startup entrepreneur may think about it. Number one, the startup game is no longer a secret. For most of the history of venture capital, the startup game was a relatively closely held secret, and only few people became entrepreneurs, only few people knew how to raise venture capital, how to leverage it, and then we saw some disproportionate outcomes over there. That is no longer true. Thanks to podcasts like yours, but also a lot of coverage. There are books you can read. There are YouTubes you can watch. There are podcasts you can listen to. It is no longer a secret. So the game is going to be played at a higher level. So you as an entrepreneur must figure that out, what is that game. You must know the game that you are playing before you play the game, and if you are going to win at it, and figure that out.

Number two, there are multiple ways to win. For a while, it felt like in Silicon Valley, there was a formula that there was a standard way in which companies were built. You raised a Series A, you ran it, you sold it for 300, or 500 million or something, and there was a formula that got created. That formula has completely broken down, thanks to the reduction in cost to start a company, and thanks to the increase in cost to actually take a company public. These two trends have ripped apart the standard formula, and so now, there's lots of opportunity. You can be creative and you can think about new and innovative ways of creating companies. It's going to be exciting. It will be tough. There's going to be some – there's going to be people who will be hurt. There is no question about it. But there's also a time of great opportunity ahead of us.

[00:50:05] SF: Do you think that given that the – raising something like venture capital is less of a secret now, that that will help with increasing that diversity of founders? Because it's less of – you need to go for this school, you need access to these certain types of people. Do you see that as a potential trend in startups as well?

[00:50:31] AS: To some extent, yes. Because there is a recognition that we have this problem in venture capital, and because it has become more open. And therefore, it will be more diversified. All of those things will help us in terms of approaching it, but we are also fighting human nature over here. Human nature is exclusionary. Human nature has certain attributes. We like people. In other words, people who are like us. That's just human nature. The sense of ego, the sense of power, those are very fundamentally human traits. Those are not going to go away. Therefore, I believe Stanford will always be a great place to go to school for the foreseeable future. Could it change? Yes.

But remember, Oxford is not 100 years old, it's hundreds of years old, it's a thousand years old, and it's still a great university. I think those types of places will exist, and they will give you an unfair advantage. There's no question about being able to – the world will get organized in that fashion. I think these are just trends, which will fight each other. I'm not a political scientist, I'm not a lawyer, I'm not a social activist, so I don't study those parts of the world. From my vantage point, which is arguably very uneducated, I just continue to see the existing trends continuing.

[00:51:57] SF: Yeah, that's fair. Well, Ashmeet, I want to thank you so much for coming on the show again. I really enjoyed our conversation. Thanks for being my first ever video guest. It's great to get your insights on the market, your perspective on investing. Hopefully, some of your advice is going to help someone who's listening out there found their first company and raise that initial fund to go out and tackle something that is going to be really technically difficult and lead to products that change the world.

[00:52:27] AS: Thank you, Sean. I really enjoyed it. If it's helpful to them, that's even better. And of course, I'm always available. If somebody is starting a company, give me a call, give me a holler and I would be happy to sit down with any entrepreneur who's looking for that first million-dollar check.

[00:52:41] SF: Awesome. All right. Thank you.

[00:52:43] AS: Take care. Thank you.

[END]