Oral History of Forest Baskett, Part 2

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Shustek: My name is Len Shustek, and it's January 31st, 2017, and we're here at the Computer History Museum for the second part of the Oral History of Forest Baskett. We did the first part back in October of last year.

Baskett: Oh, wow, time flies!

Shustek: it's been a long time. Welcome back, Forest!

Baskett: Yes, thank you.

Shustek: I think what we'll do now is start on your career as a venture capitalist for the last 17 years. You've been in NEA, New Enterprise Associates. Let's start by asking why, after so many years of doing research and working on products, did you decide to become a venture capitalist?

Baskett: It turned out to be an easy, natural thing for me to do. During my years at SGI, I was involved in all of the new technologies that the company was either doing or thinking about doing. I was also involved in all of the new technologies that other people wanted us to do or think about doing. So I would go to at least one meeting a week involving SGI people and some little start-up company that had a new technology or a new idea that they wanted to get us to cooperate with them, or buy them, or use their product, or whatever. Just because SGI was a big company and had the kind of distribution that could make a difference to a startup, it was natural for people to do that. Lots of my startups today do that same sort of thing. But at any rate, I was involved in these meetings on almost a regular basis. What became a pattern was that, after these meetings, my colleagues would frequently say, "Well, Forest, you really ask a lot of good questions. You sound like a venture capitalist." <laughs> So that was one thing that happened, for quite a number of years. The other was that I was part of a wine-tasting group that met once a month, again for quite a number of years, and about half of the people in that group were venture capitalists. I mean, practicing venture capitalists.

So these were my first exposure to venture capitalists, other than the two that were on the SGI board who had done the initial funding for the company. They were people that I liked, and I enjoyed being with and talking to. Because of my experiences with the kind of startup companies that kept coming by to see us, it was pretty easy to talk to them about what they were doing, and ask them about companies that I had seen that sometimes they had seen, too. So when it got to the point where I felt like I really had to leave SGI, it just made sense for me to think about going into venture capital because I had friends there, and I liked them, and felt like I liked what they did. And people had been telling me for a long time that I seem to know how to act like a venture capitalist already.

So I left SGI. A clean break, and then started talking to the people that I knew in the venture capital community, and ended up having a number of interviews and getting a number of offers. Of course two of the places that I went were to the people that had initially funded SGI, because I knew them well. One of which was Mayfield, and the other which was NEA. I knew the person who had been on the SGI board, Dick Kramlich, who was one of the founders of NEA. I also, by then, knew one of the other partners there, who many years before had left SGI and gone to a startup, and then gone to NEA. So I actually knew two of the partners there really well.

I met all the other partners in thinking about which of the firms I might be interested in joining, I really felt comfortable and close to the people that I had met at NEA. It was, I think, a natural decision to join the group of people that I really liked, and really enjoyed talking to and working with, and being with. So that's how I got into NEA in particular, and that's a little bit of how I ended up in venture capital, per se. Of course, it was about new ideas and new technologies and things that I had been keenly interested in all of my adult life. Certainly I had had a number of great experiences involving new technologies at Stanford and then follow-on at SGI. So it became a natural next step for me. And it was a pretty easy step.

Shustek: In addition to the personal connection to the people you knew at NEA, was there something different about NEA? Are venture firms different from each other?

Baskett: They are. The differences are, I think, mostly cultural, as opposed to structural. Again, I'm talking mainly about traditional venture capital, not about private equity, or buyout [firms], or those other kinds of financial institutions. Venture capital firms invest in startups. They take equity. They take board positions, and they try to turn the companies into stand-alone successes. That's different from what a buyout firm does. I think if you're a traditional venture capital firm, you have pretty much the same model as the others, but the personalities are quite different. The cultural traditions are often quite different. I think many of us believe that culture really matters. We certainly believe that about our startup companies. The culture at NEA was one that I felt like I recognized and liked and wanted to be a part of, because it was similar to things that I had felt like I'd had success with in the past.

Shustek: What are the characteristics of that culture? What makes it different?

Baskett: Well, NEA has a partnership culture. That sounds a little odd, because venture capital firms are partnerships in a technical sense. But it has a partnership culture in more of a spiritual sense, that everyone has the opportunity to have a voice, and everyone is expected to participate and contribute, and everyone is listened to. There wasn't a culture where there were one or two or three people who really made the decisions. Decisions were really a mutual understanding that we came to. Decisions got discussed and voted on, and the votes were mostly unanimous. There were a few "no" votes on occasion when something difficult came up. Well, I should say “mostly unanimous” in that things typically were either clear-cut, "No, we're not going to do this," or, "Yes, this is exciting; let's keep going." So it was very collegial in that respect, and it was really nice to be a part of and to contribute, to know that your ideas

were being heard, were being listened to, and to feel like you occasionally made a difference in terms of whether the firm did something or not.

Shustek: Talk about the structure and the logistics of dealing with prospective investments. There are dozens of partners, but hundreds, or maybe even thousands over years, of investments you consider. Does everyone look at them all?

Baskett: Well, no. We have a model that's probably not unusual. We describe it in kind of a formal way, although it's pretty loose, where we all have our contacts and our sources. We go to meetings, and we meet people, and we know people. A friend will call up and say, "You should talk to so-and-so; he's got a really cool idea." The typical thing is that those kinds of contacts will result in… Say in my case, someone will recommend a company to me, a particular person, usually at a company, and I'll make contact with that person. We'll get together and have a one-on-one meeting, and I'll say, "Tell me about what you're doing. Why is this exciting? Why are you doing this? This could represent a significant part of your life!" So you get the story, and you listen to it. Sometimes you say, "Well, you know, you ought to think about this, or you ought to think about that. My experience has been you're going to have difficulties here." That can be very useful to the person in question, but that's kind of a negative outcome. The positive outcome is you listen and you say, "Wow, that's really an interesting idea! I need to get Greg to hear this, because he knows a little bit more about this particular subject than I do. And also Ron might also be able to help out."

So we have another meeting. This time it's three people from NEA, rather than just one. We go through the same process again. And maybe they say, "Well, what about X? And what about Y? And you know, you really ought to think about Z." So the person in question, or the group in question, goes back to their drawing boards, or goes somewhere else. Again, that's the negative outcome. But the positive outcome is the three of us will say, "Wow! This is really, really something that we need to look into." Then we start doing due diligence. We talk to other people at the company. We talk to people that might be prospective customers about what they might think about this idea. We try to find out who the competitors might be. We try to find out about these people's backgrounds and their experience levels, and what other people think about them. If all of that makes sense, then we put together what we think is a reasonable offer for financing the company.

If that makes sense to the company, then we typically have one last meeting for some pretty large subset of the partnership. That's where we decide whether or not we're actually going to make the investment. Those meetings are, in a way, a bit of a formality. But not quite, because you really have to be prepared. You really have to have done your homework. You're in front of all of your partners saying, "I think we should do this." You really want to make a good impression, because you're asking the partnership to commit the resources of the fund in question to this company for a long time. So even though those meetings typically result in a "yes" vote-- occasionally a "no," but not very often-- they're more than a formality, because you're on stage, and if you haven't done your homework, you're not going to like the

outcome. Occasionally those meetings result in people saying, "Well, you can do this if you want, but you ought to think about X and Y and Z." Sometimes you go back to your office and you think about it and decide, "Well, maybe this is not really where I want to spend a lot of time for the next few years."

Shustek: Are the company founders at that meeting?

Baskett: Yes, they present. The company founders make the final presentation, with coaching, and background material and such.

Shustek: And then is there a closed session later without the founders where the partners discuss the case.

Baskett: Yes, right, that's what happens. You get the presentation, the company representatives leave, and then there's a partnership discussion and all of these issues come up. Typically there's a vote at the end of that meeting to whether to go forward with the funding proposal.

Shustek: Is it a formal vote with votes recorded, written down, and all that?

Baskett: Yes, it is. We have limited partners that [have] access to that information: who are we investing in, and how much are we investing, and who voted for it, and who voted against it. That's all part of the record, yes.

Shustek: Do you keep long-term track records of the deals that the partners bring to the firm?

Baskett: We do, yes. There's a saying in the industry, "It's a very slow way to get rich," because the typical deal takes eight years. It's a long time between making the initial investment and having a return that you can give to your LPs. So it's a long time before you get to the point where people can look at your record and say, "You're a good investor," or, "You're not a good investor." But that is one of the things that we try to be able to do. And that's why getting to be a general partner is also a very slow process, because being a general partner is like having tenure at a university. It's something that you don't go into lightly, and you don't get dismissed from lightly either. Generally the people who get there do have a track record and look like really successful investors.

Shustek: I won't ask you about your own track record -- unless you want to offer that information -- but do you have information, generally, about the firm in terms of how many the investments are successes?

Baskett: Yes. NEA is 39-years-old; it's one of the oldest venture capital firms. It's one of the original traditional venture capital firms in Silicon Valley, although we were founded by Coastal. We have made over a thousand investments. We've had 400 of those have exits. 200 of those were initial public offerings; the companies became public. The other 200 were merger and acquisition kinds of outcomes. So we have a good track record in that respect. We have also been what's called a “top quartile” fund or a “second quartile” fund for almost all of our funds. We're just finishing up our 15th fund, a fund that took up the last three years. We're regarded as one of the top 5 venture firms. So it's a great firm! It has a good reputation, has good results. Our limited partners like us. We're just starting fundraising for NEA 16, and we're having a very positive response from the candidate investors, candidate limited partners.

Shustek: Are you almost always the lead investor in the portfolio companies, or do you co-invest with others?

Baskett: We like to co-invest. I think our ideal investment would be a co-investment with another well-established, experienced venture firm. We like to co-invest on a 50/50 basis. The idea there is that there's no lead dog, that both investors are equally committed and equally expected to do the work that's needed to help the company succeed. That's been our philosophy for a long, long time. We like it and we think it works. And it does seem to work well.

We do many times become the only investor. That happens when the company either doesn't need much money initially, or is already highly valued for whatever reason, and the only way to get a decent ownership percentage is to be the only investor. Because the ownership percentage is something that counts a lot for us. Our most critical resource is the time of the partners -- the time that we spend working with companies and trying to make them successful. In order for that time to be valuable, we need to think that there's going to be a decent return for our investors. And to have a decent return, first of all, the company has to become big and successful; and secondly, you have to have some noticeable ownership stake. So those are the two parameters that push us either to partner with someone else, or to go it alone.

Shustek: When you partner with another venture capital firm, how do you negotiate things like company evaluation, who gets the board seats, so forth?

Baskett: Mm hm. If it's a co-investor that we like, that's usually something that's pretty straightforward. We've all done this a lot, and we kind of know what works. It's normally a pretty short discussion. You say, “Well, can X percentage work for each of us? Are we comfortable with this valuation? Will our partners scream and holler? Or will they say, "Oh, that's a good deal!" And the board positions -- we're pretty flexible on that. One of the things that is structural about NEA is that the returns to the partners are not preferentially based on who was the lead investor in a company. So if a company returns X dollars to the partnership, those X-dollars get split up in a formulaic way, no matter who was the lead investor.

Shustek: And no matter who has the board seats?

Baskett: No matter who has the board seat. What that means is that we're all eager to help our partners with their companies, because we all share the returns.

Shustek: Is that a traditional structure, or do other firms do it differently?

Baskett: No, other firms do it differently. There are some firms that do it like we do. But there are other firms where there's a formula that's based on whether you brought the deal in, or whether you had the board seat, or those kinds of estimates on levels of effort and participation.

Shustek: That seems complicated as time goes on.

Baskett: It is, it is. And it leads to all kinds of discussions <laughs>, as you might imagine, about who should get credit for what. So having a structure where everyone gets credit means that everyone is very helpful. And very collegial, as well.

Shustek: How do you handle the fact that you invest in multiple disciplines? There's biotech, there's clean energy, there's IT, there's pharma.

Baskett: Well, I think a large fraction of venture capitalists are generalists, in that they tend to invest in a variety of areas. But even as generalists, we all have backgrounds that inform us more fully on certain areas. So we have a Med Team, and we have a Tech Team. The Med Team tends to invest in the medical technologies: the devices and the bio/pharma companies and such. The Tech Team tends to invest in things that related to information technology: gadgets and software systems and whatever. Then inside those two big areas, in the Med Team we have a couple of people who are especially experienced with medical devices. We have a few people that are especially experienced with diagnostics.We have a few people who are especially experienced with bio/pharma. A few people that are especially experienced with healthcare delivery systems. Then if you go into the technology side of things, we have people who are better at consumer products, and people who are better at software infrastructure for enterprises, and people who are good at semiconductors, and people who are good at storage systems. So you try to get the people that you think know the most about a particular subject to be involved in companies that are going into those subjects. But it's still somewhat fluid. I have ended up working on mobile applications for consumers, as well as semiconductors for various kinds of purposes. It's pretty wide-range.

Shustek: What difference does the prospective timeframe make? A drug development might take ten or fifteen years. Or an IT company could be a billion dollar company in two or three years.

Baskett: We like the IT companies that become a billion dollars in two or three years, but in fact those don't happen very often! <laughs> Occasionally they do, and they get a lot of headlines, but it's not that common. Our going-in position is that each company is going to take a while, and we need to build it carefully. We need to plan out how we spend money, at what rate, and how fast do we hire, and how fast do we try to go to market. It's a complicated process. Experience helps. It helps a lot, because there are patterns. Building a company is often a pretty repeatable process, in terms of how you do it, and what the rate is, and who you need to hire when. I think that's where venture capitalists actually add a lot of value to very young entrepreneurs who just haven't done it that often. I think that's part of why we're in business.

Shustek: What are some of the common mistakes that entrepreneurs tend to make that venture capitalists can help correct in the early days?

Baskett: The first thing we want to see is a vision for a big company. What is the idea that makes this company into a billion dollar company? What is that idea? That's the vision. But we also want to see a notion of what the MVP is. When we say MVP, we almost always mean the Minimum Viable Product. What is it you first take to market and try to sell to someone. It's typically quite different from what the vision is, but it's something that's on the road to that vision. Having the notion that there's this long sequence of steps, and there's an ultimate goal, which is a big idea, but there's a starting point as well. And that has to be a starting point that makes sense, that people are interested in and feel like they need, and are willing to pay for. In developing that model, we expect entrepreneurs to have some inklings of what those big ideas and those starting points are. But filling out what all those steps are is something that is, I think, less frequently planned out by entrepreneurs unless they've been through it several times. And so I think we help a lot.

Shustek: What mistakes do they make in organization and staffing, and the decision of who gets to be CEO, and that sort of thing?

Baskett: Uh huh. There's a truism that there are CEOs who can take a company from zero to a hundred million dollars in revenue. And there are CEOs who can take a company from a hundred million dollars in revenue to a billion dollars in revenue. But those people are frequently not the same people. We try to actually have that conversation with founders when we're talking about investing, and find out whether they think there's some truth to that, or there might be some truth to that. If they're comfortable with that kind of truth being revealed over time. Because that's a signal that you've got someone who's flexible and wants the company to be successful, as opposed to someone who has a notion that he or she is the thing that's going to be successful. It's important to have that conversation up front. We try to always do that.

But even when we do, people don't always know what it is that's important to them. They think that it's the company, but when it comes time to make difficult decisions about leadership -- either new leadership or

shared leadership -- it's hard for people. They feel like they have their baby, and they don't want to give up their baby.

Shustek: The problem is there are high-profile cases they can point to, like Jeff Bezos or Larry Ellison…

Baskett: That's right!

Shustek: …and they want to be that!

Baskett: Yes, exactly! <laughs> That's absolutely true. Unfortunately, there are some other cases. Larry Page always comes to mind as someone who said, "Oh, okay, we need real management here," and they brought in Eric Schmidt. Eric did a great job for a while, and then things changed again, and Larry said, "Well, maybe I can do this after all," and the board agreed and then went back. There are several other cases like that, but we all know about that one. Those are discussions that we often have with young entrepreneurs. Go through that storytelling, because this is really serious. Do you want to invest in a person who is going to have a death grip on the company? Sometimes it is a death grip.

Shustek: How important is the entrepreneur's prior track record -- if they had a failure or a success?

Baskett: Oh, very important, very important. So what do we look for? We think about three primary aspects of a potential investment. One is the technology or the idea: what is it that this company wants to do. Two is the market: is it a big market for this idea or this product or this technology. And three is: who are the people that are going to try to make this work. You can roughly characterize different venture firms as weighing those three aspects differently. For NEA, I think we most commonly put more emphasis on the people than either the market or the technology. There are other firms who really are keen on, "How big is that market? What is that market like?" Then there are other people who say, "Well, the technology is what matters, and we can change out the people, and we'll find the market." But it's those three things. It is a tripod; you have to have all three legs be strong. But for us, we look really, really carefully at the people.

Shustek: Does it always have to be a huge market, or can you fund a company that has a narrow product in a more modest market?

Baskett: No. <laughs> Right. That's where the vision of the entrepreneur makes a difference, and it's also where I think the experience of the venture firm makes a difference. Because many times as technology moves forward, we get to a point where we can do something that was never possible before. One of the by now trite examples is the iPhone. Completely new capabilities that we'd never seen before. Our belief as venture capitalists is that those new capabilities often mean that there are new markets that

we didn't know existed. You have to go out and demonstrate that those markets exist and then build them. And that happens. That happens frequently.

Shustek: Did you mean the iPhone, or the smartphone in general, starting with the Palm and the Handspring?

Baskett: Well, the iPhone was the first! <laughs> Yeah, okay. Sorry. <laughs> The smartphone, in general. The ability to have a lot of computation and a lot of user interaction in a portable device. Amazing capability. Changed the world. And the wireless capability that went with, it that made mobility part of that equation.

Shustek: It seems to me that the easy cases for a venture firm are those companies that either become a success, or the ones that become a failure and you shut it down. The fun cases are the living dead, the ones that go on for many years and you're not sure what's going to happen.

Baskett: Yes. Yes, yes, yes.

Shustek: How do you deal with those?

Baskett: We have a monthly meeting where we go over our portfolio and we say, "Is it Category 1? Is it Category 2? Or is it Category 3?" Category 1 are the ones that we call needle-movers. These are the companies that we think can be so successful that our limited partners will notice, because they'll make noticeable money for our limited partners. Category 3 are the ones that we think they're not going to make a difference, or they've run out of steam, or the market turned out not to be that great, and it's time to move on. So find someone to buy them, or shut them down. Finding someone to buy them is more the typical activity. You just have to shut them down when that doesn't happen. But the notion is, "Let's not put a lot of our energy into these companies, because they're not going to make a difference."

The Category 2 companies are the hard ones. That's where most of our time goes, because we're not sure. Are they going to break out and become successful, or are they going to turn into Category 3 companies? Those are the ones where we wring our hands a lot. We just do what we can, and we get our partners to help us and really try to figure it out. We frequently have companies in that category that get promoted up, and those are big successes for us personally. Those are places where we've added a lot of value to the company, we think.

Shustek: How aggressive are you about doing follow-on investments? And particularly when you're co-investing with other people, do you always do so on an equal basis?

Baskett: We believe in follow-on investments. That's a place where some venture firms have a different model. There's one well-known venture firm that always tries to be the first investor and get a big percentage ownership of the company, and then expects other people to do the follow-on investments; they stop. Our approach, as I said, is we like to co-invest half-and-half with some other firm, and then to follow-through. Get smaller firms to lead subsequent rounds, but always participate and continue to invest and continue to be part of the company. We're big believers in follow-on investment. That's part of our reputation. It's part of our culture. I think it is part of why we've been a long-term success, because entrepreneurs can discover that about us, and that's an attractive proposition for them as well.

Shustek: Do you have rules or expectations for how the founders and employees in the company should share equity?

Baskett: We do. Not rules, but guidelines. Almost everything in our business is a guideline as opposed to a rule. And there's a lot of survey data that gets generated, and propagated. Just this morning I was sharing some of that survey data with some other investors in a company where we're going to have to review the compensation of the executives. There are norms. They're kind of, "This is what the industry standard is for a company that's this size in this area." If this company is exceptional, perhaps the person gets more than what the norm is. If the company is not doing so well, maybe he gets less. There are lots of guidelines and there's lots of data that gets accumulated and shared.

Shustek: What about the democratization of the equity sharing and the social activities of the company? Do you get involved in any of that at all?

Baskett: Again, like salaries, equity levels for different functions in the company are something for which there are guidelines. We share those. And again, what we try to do is to have a company develop a set of bands for both salary and equity for each of the different categories of employees. When a new employee is hired, if what the company wants to pay that person, in both salary and equity, is inside those bands, then the board typically says “Fine.” If the company says, “Well this is an unusual person, we’re not going to get this person if we’re in the band. We need to go outside of those bands”, then the board says, “Tell us about this person, and why this person is unusual.” Then if it’s a good argument, the board says ok. It’s a “by exception” kind of thing.

Shustek: How big are the boards typically, in these companies? How many seats do you and the other VCs have?

Baskett: In early stage companies, we like to see a board of three or five. Five is pretty common, which would be two investors, two company representatives, and one independent. Entrepreneurs worry a lot about board structure, and who is in control. “What’s going to happen to me?”, and dot, dot, dot. We often have to say, “Look, the board is really in this to make the company successful. It’s not about who has the

votes, it's about what makes sense for the company." We almost never have voting contests, where it matters how many board members there are. It's usually more about what is the right thing to do. What is the discussion like, and can we get everyone on board about what to do next.

Shustek: How often does the board meet? And how often are you in contact with the founders, outside of board meetings?

Baskett: For early stage companies, we actually try to have a board meeting once a month. As the company becomes more established, we'll frequently go to every other month. Public companies, as you know, have board meetings once a quarter. That's the spread, from once a month to once a quarter. One of the things that we've done, just to be more efficient about advising a company, is to have alternating strategy meetings and board meetings. A strategy meeting is like a board meeting, except that no votes are taken. There aren't any equity grants, there are no motions, there are no minutes, there are no lawyers.

Shustek: <laughs>

Baskett: But you talk about how the company is doing, and what the issues are, and what we need to work on. Just like you do at a board meeting.

Shustek: Is it just you and the founders, or are other people in the company involved?

Baskett: That varies. What we like to see is a three-layer board meeting, where the first part of the board meeting involves a number of people from the company. The VP of engineering might want to talk about what the engineering plan is, the VP of sales might want to talk about how sales are going, the VP of marketing might want to talk about how marketing is going. That's a session where participation is encouraged. It's really usually good for the company to feel like they know what's going on, and who the board members are, and what their questions are. Then there is often a closed session, where just the board members [and the executives] meet and talk. That's typically where compensation issues get discussed, and any kind of tough personnel issues come up. Also where, if there's an acquisition candidate circling around, that might get discussed, because those are so sensitive. Then following that, there's often a closed executive session, which is oddly named because it's where we typically throw the executives out, and just the investors talk among themselves. About how they think the company is doing, and how the CEO is doing, and those kinds of things. The first part, the open part, is the long part. The closed part is half or quarter that. The executive session is a few minutes, but always worth doing.

Shustek: Are there ever any relationships between your portfolio companies? Could one be a supplier to the other? How do you deal with pricing? Could they be competitors, either initially, or as it develops later on?

Baskett: Well, we try not to invest in companies that would be clear competitors to our existing investments. That kind of a thing is a guideline. But as you say, what a company starts out doing is not always where it ends up. So companies do become competitors that are in our portfolio. When that happens, we try to be sure that when we have discussions about those companies, that the person that's on the board of company A is not part of the discussions about company B, and vice versa. We do that so that the two companies, A and B, have some reassurance that we're not sharing information about their competitors. They don't always believe us completely, but..

Shustek: <laughs>

Baskett: …we really do try, and we have a good reputation. We do try to have those kinds of walls on information. But we also have companies where one becomes a supplier to the other. The issues about pricing are things that we then try to stay out of. That's between company A and company B, and the investors say, "Well, you guys work it out. <laughs> It's up to you. It's got to be consistent with what the other customers are doing, and what's fair." I think it's a pretty workable kind of situation.

Shustek: Do other issues come up with respect to confidentiality of company matters?

Baskett: Entrepreneurs are often quite paranoid about their confidential information, and venture firms really work hard to respect that confidentiality, mostly. There are exceptions, but mostly we do. NEA, for example, almost never signs nondisclosure agreements with companies. One thing is, there are so many companies. It's so common to have two or three companies propose the same idea to you. If you sign a nondisclosure agreement with one company and another company comes in and presents the same idea, even though you say nothing you're potentially legally exposed. So we say, "Look, we have a good reputation, we don't share information. You either trust us, or talk to someone else."

Shustek: How often do you lose a company that you think is a good idea and you wanted to invest in, but the entrepreneurs don't?

Baskett: Well, for what we think are the really good companies, the funding is often competitive. We're often pitted against another respectable venture firm, in terms of who wins the deal. That's how we phrase it: "Who wins the deal." I've frequently had an entrepreneur tell me, "Stop having people call me. I know you're a good guy, I've heard it over and over and over. You don't have to tell me anymore." <laughs> We work hard to get our existing entrepreneurs to vouch for us and give us references, and it's very effective. We tell our entrepreneurs, "Look, make cold calls. Do your own checking. We're proud of our record."

Shustek: After 39 years, you'd think that most people would know about you.

<laughter>

Baskett: Well, most entrepreneurs are pretty young.

<laughter>

Shustek: What's your relationship, if any, to your limited partners? Do you meet with them? How much do you disclose to them?

Baskett: We do, we do. That's one thing where I think NEA has an especially good reputation, with our limited partners. We have an annual meeting. It's several days, and we have presentations on each of the funds. We have a lot of five-minute or ten-minute segments about companies that are in those funds, and what they're doing, and how they're doing. That, plus coalescing how the fund itself is doing: how many investments, how many are positive, how many are negative, what the follow-ons have been, how many have gone public or been acquired. Our limited partners tell us that our annual meetings are better than most of the ones that they go to. We're a big fund, and we have big limited partners, and they invest in a number of other venture firms. So they have perspective, and we get really good reports on that. I think it's part of the NEA culture to be pretty open, and pretty straightforward about everything. There are firms that we hear about that try to keep their limited partners in the dark <laughs> -- just don't tell them much. I don't know how they manage to do that, but there does seem to be a divergence in what firms tell their investors.

Shustek: It seems like the relationship between the venture capital firms is a little bit strange, because on the one hand you're competitors with each other..

Baskett: We are competitors.

Shustek: …on a whole bunch of levels. On the other hand, you cooperate and co-invest in companies.

Baskett: Yeah, yes.

Shustek: How do you manage that?

Baskett: As collegially as possible. If I lose a deal to a friend of mine at Sequoia, I say, "Good luck and congratulations", because he may lose one next month, or we may invest together on one the month after that. So it's just, "you win some, you lose some". But it's an ongoing, long process. Whatever you do today, you need to be careful that you think about how it affects what you'll be able to do tomorrow.

Shustek: Before we get into individual companies, of which there have been 80 that you've had... <laughs>

Baskett: …been involved in.

Shustek: …NEA invest in, let's talk about your interests. What areas do you particularly focus on? Is it hardware versus software? Consumer versus enterprise? Is it all IT, or is it other technology?

Baskett: I'm interested in new ideas, typically, that have some technology component. That's what my background has been, starting off as a mathematician and then becoming a computer scientist, electrical engineer. My technical background says that I should work on things that have a technical component, so that I'll know what I'm doing, as well as potentially being able to help. But some things are just structural; they don't have a technical component in an obvious way. Some of them are just opportunities that look exciting, and have a twist to them that make them sufficiently unique. They have a chance to become important by capitalizing on some uniqueness.

An example that I'm excited about right now is a company in Europe called "Go Euro." It's a company that provides travel information for travelers. The interesting thing about it is that it provides not only the usual airline stuff that we all know about, but it also provides coordinated information about trains and buses. That's unusual in the travel business. It's especially interesting that they can do trains in Europe, where train travel is a big deal. Train travel is not such a big deal in the US, for various reasons. But trains are a very popular and convenient and comfortable and economical way to travel in Europe. Lots of people use them. But most of the train companies are derived from government monopolies, and the information technology systems that those train companies have had, have been old legacy technologies. All of the IT systems that we know about, that are so common in the airline industry, don't exist in the train business. This startup said, "Well, people need that same kind of widely-available information that we see in the airline business, for the train business." Because you go from France to Germany, or from Italy to England, whatever. The company said, "Well why don't we just talk to these train companies, and see if we can figure out how to help them?" So they developed the ability to get all of the train schedules from all of these old government monopolies, put them together, and offer a travel service. You get the airline data, that's easy. You've got the train data, and then expand into buses. That turned out to be pretty virgin territory as well. So now there's a travel app in Europe that's just amazingly useful. They have many millions of users every month. It's quite remarkable.

Shustek: So this is an example where it wasn't breakthrough technology, but it was an application..

Baskett: It wasn't breakthrough technology, it was an application. It was a situation where technology had not penetrated, for structural reasons, legacy reasons.

Shustek: Have you figured out how to monetize this product?

Baskett: Well, there are lots of ways to monetize it. We take a very small cut of the ticket, when we sell tickets through our system.

Shustek: How do you arrange that with government monopolies?

Baskett: That's always a negotiation. Yes, it is. But their goal is to keep their trains full and to provide the right kind of service to their customers, so they want to do the right thing, it's just that they haven't been trained to the level of discipline that might exist in some other consumer-related areas. But they want to do the right thing, so you can work it out.

Shustek: I assume Go Euro is a European company?

Baskett: It is incorporated in Delaware. But the company is headquartered in Berlin, which is a great place for software, by the way. It has offices all across Europe, and is starting up in China and other places as well.

Shustek: How many of your companies are based out of the US?

Baskett: Not very many; I'm not sure what the numbers are. We're happy to look at, and invest in, companies that are outside of the US. But it's easy for us to see companies that are in the US, and of course I think the US has more of an innovation culture than exists other places. England's been very good. Israel's been quite remarkable, Germany is doing quite well. So there are places.

Shustek: For the domestic companies, are most of them in Silicon Valley, or are they widely spread?

Baskett: They're widely spread. I'd say two thirds of our investments are Silicon Valley, maybe three quarters. But there are quite a number of companies elsewhere. I've had companies in Boston, in Austin, in Seattle, in Portland, in San Diego… a variety.

Shustek: How do you manage your time? How many boards are you on, and how do you manage that?

Baskett: My board load is always a sore point, because it's so high. I've varied, over the last several years, between 15 and 20 boards, which is a lot. I have an absolutely amazing assistant that keeps my calendar, and makes the trains run on time, so to speak. I couldn't do it without her, or someone like her.

I just love doing it, and I devote a lot of time to those companies. None of them complain that I neglect them, so I feel like… At least, most of them don't complain that I know of. So it works for me.

Shustek: Your administrative assistant can help with the scheduling and the logistics. But how do you deal with the mental context switch that you have to do, when you move from one phone call or one meeting to the next, and it's totally different?

Baskett: I don't know. It doesn't seem to be a huge problem. I do have to remember if I need to call people at night, or call people early in the morning to follow up on something, because there are lots of loose ends that need attention all the time. I don't get to all of them, but I get to enough of them.

Shustek: When you're a venture capitalist, you could describe your role as an enabler of people's success, or companies' success. When you're a researcher, you're a participant; you're crafting it. Can you compare the satisfaction you get from those two different kinds of roles?

Baskett: Well, I have a joke that I've developed over the years, that people who know about university researchers, in particular, find amusing. Which is: that being a venture capitalist is a lot like being a professor. The difference is that as a venture capitalist, when you give people advice they frequently follow it, whereas students are more ornery <laughs>. We do give people a lot of advice, and I think occasionally that advice is transformative. It's really about ideas that the entrepreneur hasn't thought of, and they can be very creative. Entrepreneurs can pick up some things and run with them, and you can see things blossom and develop, which is terrific. That's the unusual case. The more usual case is that you find someone who's got a great idea, and you really like it, and you want to see it succeed. You help the person, and it goes somewhere, so you feel like you've been an important cog in the machinery that gets something to happen.

Shustek: How do you deal with people who are personal friends who want to be entrepreneurs and be funded by NEA? Or people who were funded who become friends? Is it difficult to make clear decisions?

Baskett: Well, there's a pretty easy "good cop, bad cop" kind of situation there. My friends have to answer to my partners, and so it's easy to say, "Well I'd love to work with you again, but this idea is not going to fit in with what my partners want to do." So there's always that.

Shustek: You've been involved in some 80 companies over your 17-year career in NEA, and obviously we can't talk about all of them. But you've been kind enough to identify a few that might make interesting conversation, so let's talk about a few of them. One of them was Atheros, which I know is the Wi-Fi chip company.

Baskett: Yes. Atheros, or Athēros, depending on how you choose to pronounce it, was one of my early investments when I first started at NEA. Not the first one, but pretty close. I knew something about the company because the founder was a Stanford professor that I had known.

Shustek: Teresa Meng?

Baskett: Teresa Meng. She's a really talented, brilliant woman. She had been telling me about what she was working on. I had visited their little tiny house where there were 11 people slaving away, and saw what they were working on. When I got to NEA they had done a Series A, first funding round, with two other firms. Teresa knew that I was NEA and said, "Well. time for the Series B. Let's go talk to Forest about it." Atheros actually, I think, made Wi-Fi the thing that it is today. The way they did that was by being really the first company to show that you could build radios in CMOS. Radios are very analog-y, high frequency, demanding circuit kinds of gadgets. To make a radio that works is something that's challenging for an accomplished electrical engineer. Up until that point, people had believed that CMOS just didn't have the oomph, the analog precision, and the amplifying power that would be required to make radios. What Teresa had imagined at Stanford was that, no, that that was not true, that CMOS had progressed. If you understood enough about how CMOS devices worked, you could in fact design radios in CMOS.

They had gotten to the point where they really had demonstrated that they could build radios. But then the question is, what kind of radios should they build? It was a new capability, that you could make radios that would be very inexpensive, because of the economies of CMOS, and could be easily mass produced, again, because of the economies of CMOS. But for what purpose? I mean, how many radios were there in those days? The answer was: not many. Mobile phones had just barely been invented in the year 2000. I had one, but it was about this size, it was huge. Radios were part of that problem. They discovered this kind of unused standard, called [IEEE] 802.11, that some radio engineers had designed, and specified, and gotten standardized by appropriate standards committees. There were a few uses of it, but it was being implemented in gallium arsenide, or silicon-germanium, or some other exotic technology that was expensive, slow, and difficult to use. Not something that was conducive to mass production. So they said, "Well what about this?" And, it was unlicensed spectrum. It wasn't something that the telephone company owned, or that the government owned. It was available for anyone to use, under certain rules about how much power could you transmit, etc.. So they said, "Well, let's do that. It's unlicensed spectrum. If it's convenient, then people could figure out how to use it from their desktop computer to their access point in their building. And then from the access point, to whatever the networking infrastructure that the building had might be.

That's what they decided to do. Again, there were a few examples of that with exotic technologies. But to me, this looked like this could be a breakthrough. You could do something that was really, really useful, and really inexpensive -- at a price point that people had never imagined before -- and could become widely popular. It was an example of technology making a new market possible that we didn't know

about, but we could imagine it. Because if you think about the kind of networking that people had been doing with wires -- which was pretty expensive and pretty complicated, and a pretty big business -- it made sense to think that this was something that could really take off. So I became the new investor, NEA became the new investor, and I went on the board. Of course, it was a company that did make Wi-Fi the thing that it is today. Competitors developed quite quickly, but we got it started.

It was interesting, in terms of my venture capital career, that my partners didn't quite understand what it was. But I was persuasive about why this was a good idea. They also said, "Well, the other two venture firms that are involved in the Series A, they're respectable venture firms, and the board members are experienced. If Forest goes on that board, he'll probably learn a lot." So they had this notion that it would be a good training wheels for me, as well as maybe it would be an interesting investment. That was a great takeoff point for me, in terms of finding ways in which venture capital made sense, and I could participate.

Shustek: The breakthrough technology there was the use of CMOS for analog circuitry.

Baskett: Yes.

Shustek: It wasn't digital signal processing, and digital software-defined radios and all of that.

Baskett: No, it wasn't. It was analog circuitry -- that you could build really interesting analog circuitry in CMOS, if you were careful and knew how CMOS really worked at a very low level. Then the economics of CMOS would pay you big benefits.

Shustek: That was an interesting decision, to go after a market that was immature -- the 802.11 Wi-Fi short distance market -- as opposed to what was clearly going to be a big market, which was the cellphone network. Why not provide the radios for cellphones instead?

Baskett: Well, the interesting thing about the Wi-Fi potential market was that it involved enterprises. It solved a problem that the enterprises had, and it didn't involve the carriers. We all knew by then that anything that involved the carriers had a whole set of complications around it, that if you didn't have to deal with you were better off to not deal with them. It was really that notion. The cellphone market was starting to take off, and it was clearly a big market. But you would have to deal with the carriers.

Shustek: As people like Qualcomm did.

Baskett: Yeah, absolutely, right. But it was a long, slow battle for Qualcomm actually, to get going.

Shustek: Are you still on the board of Atheros? Are they still an independent public company?

Baskett: I was on the board for seven years. No, longer than that, I can't remember. Atheros went public and became a successful public company. I stayed on the board, I think, for about four years after it went public. Then I left. Still had lots of friends there. They eventually, after being a very successful public company, decided that it actually made more sense for them to be part of Qualcomm. So they were acquired by Qualcomm, and that's where they are now.

Shustek: Is Teresa still involved?

Baskett: No. Teresa went on to do other things, probably about the same time as I left the board. She went back to Stanford and is involved in other research activities.

Shustek: Next company on your list is ReShape.

Baskett: Yeah, ReShape. I put this one on the list because it's an example of an experience that I've had a number of times, where you can see an idea that makes sense, but the market isn't ready for it. The idea of ReShape was to do physical design for integrated circuits as a service. If we look at that idea today, people would say, "Well, of course. That's a specialized kind of capability, and the specialists are few and far between. If you have to hire them yourself, that's going to be tough. If there's a service that can do that, if you can tell them in detail what the circuit should be, and they can go off and do the physical design in a respectable way. Be a great service." But this was back in…I don't know how long ago it was, but early 2000s. Companies were not used to that idea. Something as a service was not the big idea that it is now.

Shustek: Well, they were used to getting their ICs made as a service, by TSMC and so forth.

Baskett: That was after you did the physical design. You did the physical design, you made your mask, you sent your mask off to a foundry. But the fact that someone else would make the mask for you? Unheard of. Everyone had their own team. That's how it was done. We just were not able to… We only had a few customers from big companies. Even there, those companies also had their own design teams, so there was this kind of internal politics about the internal design team wants to do all the physical design.

Shustek: It seems like the opportunity would have been with smaller companies that don't have their own design teams.

Baskett: That was hard too, because there were quite a number of people that felt like they could do physical design, and wanted to be a part of startups. And the small companies typically were very cash constrained. They just felt like they'd be better able to control their own destiny by doing their own design.

Shustek: From the way you're talking, it sounds like it was not a success.

Baskett: It was not a success. No, we couldn't make it go. We didn't have enough customers.

Shustek: Has anyone done that since?

Baskett: No. It's still an idea that deserves to be part of our landscape, but isn't.

Shustek: Thank you for talking about a company that was not a success.

<laughter>

Baskett: Well, I've got several, if that's what you want.

<laughter>

Shustek: That's important.

Baskett: You learn a lot from the companies that don't make it. You really do.

Shustek: So what was the lesson that you learned?

Baskett: This one is a little hard, because I've repeated this mistake several times: How do you know that a new idea that deserves to be part of the landscape, you believe, [but] the landscape isn't ready for it? The ground has not been plowed, so to speak, and it won't grow. I don't know what the answer to that is, but I have learned that I need to think very, very carefully about that question when I look at a new idea.

Shustek: So you still think that, in that case, the idea was good but the timing was wrong.

Baskett: Yes, the timing was wrong.

Shustek: Yet it hasn't been done to date, so the timing is still wrong.

Baskett: It hasn't been done to date. Part of the reason is that the whole area of integrated circuit design has become so expensive that only big companies do it, and the big companies still have their own physical design teams. This notion of X, Y, Z as a service hasn't affected those people. It's an interesting situation.

Shustek: You would have thought that, ever since the Carver Mead paradigm, that it would have been easier to design custom ICs.

Baskett: It is easier. And that was part of why you couldn't sell it to startups, because they thought they could do it.

Shustek: The next on the list I know a little bit about: Foveon, the color camera manufacturer.

Baskett: Yeah, Foveon. Again, a situation where there's a really cool technology. This particular technology was about having a camera sensor [where] each pixel would respond to red, green and blue simultaneously. The sensors that were in use at the time had filters on, so that one pixel would be a red pixel, and another pixel would be a green, and another one would be a blue one.

Shustek: Optical filters.

Baskett: Optical filters, so that only the red light would get through to the red pixels. Foveon had developed a technology that was a multilayer cell [where] each layer could respond independently to the red, green and blue. And the technology worked really well. Beautiful results with cameras.

Shustek: The advantage is higher resolution?

Baskett: The resolution was a problem because, as a startup, we really couldn't either afford to or keep up with the optical resolution of the filter sensor technology. We could build a sensor that had the same effective resolution in terms of the quality of the image, but not the spec resolution, the optical resolution in terms of lines per inch, if you will. We were trying to deal with that. It was a marketing problem for one thing, and it was a problem with our customer base. We were up against Canon and Sony and some other really large companies with really big research budgets and really big market presences.

Shustek: Why weren't they your customers?

Baskett: We tried that too. Again, they believed that the roadmap that they had internally for developing that technology was the right one, and that this was a departure from that, and they didn't need to go there. Why bother? We just couldn't get there. So it was a technology that turned out to be a really good technology, but not good enough, and up against some big incumbents where you would have to be dramatically better in order to get past that kind of market hurdle.

Shustek: I know Carver Mead was involved with this. Was it his idea?

Baskett: He was. Carver Mead was very involved. He was on the board.

Shustek: Was it his ideal originally?

Baskett: Yes, it was his idea originally, right.

Shustek: So, Foveon doesn't exist anymore?

Baskett: No. It was sold to a camera company in Japan and it's still being produced. actually.

Shustek: Ah hah. That answers my question about what happens to the IP of a company that doesn't go on to do what it had originally intended.

Baskett: Yeah, you can still buy Foveon-type cameras from Sigma. They're still out there. But, it's not a big success.

Shustek: "Aeluros"?

Baskett: Aeluros was a company that was in the analog networking business for high-speed interfaces -- for Ethernet, for example. It was actually the first company to build a 10 gigabit Ethernet interface in CMOS, which is remarkable. Some just brilliant [folks] out of Stanford.

Shustek: So it's the analog-to-digital transducer for those networks?

Baskett: Exactly right. It was actually a very successful company. It didn't take much cash, became profitable, and eventually was acquired by one of the big players that went to Broadcom eventually after a couple of steps. It was a successful company -- great people, fun experience -- but it was small. It was a really good idea for a pretty narrow market.

Shustek: So how does that get rated by the VC firm?

Baskett: It's "okay". We made money, but not very much. On the other hand, it was reasonably economical in that it didn't take a lot of cash and it didn't take a lot of time. The team was excellent. They executed well. It was pretty straightforward.

Shustek: It seems to me they were doing something hard.

Baskett: They were doing something hard.

Shustek: Analog electronics is really hard.

Baskett: Yeah.

Shustek: Does it ever annoy you that a company that has achieved success in something that's really hard doesn't make a lot of money, but a company with the next dating app makes a billion dollars?

Baskett: Yes. Of course. Right. The problem with the dating app is that the ones that are successful are very successful, but they're probably 10 percent at most -- maybe 2 percent -- of the total number of dating apps, and all the others fail miserably. Whereas a company like Aeluros, nobody else could do that. There wasn't any real competition.

Shustek: "Newisys"?

Baskett: Newisys. In my SGI days, we built pretty amazing computer systems. Newisys was a design for a bit of a continuation of that theme of building some really interesting computer systems. Really smart people came out of IBM, where they had done some similar things that we did at SGI and the design was really good. The execution was good, but it was another case where it was only a little better.

Shustek: What part of the spectrum was it in?

Baskett: It was in multiprocessors. High-density, high-performance workstation and server kind of multiprocessors. It was a good idea, and it was easy for me to understand, having been involved in that before. It seemed to make sense, but the commercial success was limited because it wasn't four times better. It was only two times better. That's become a bit of a truism in the business in terms of thinking about something that goes into an existing market. Our rule of thumb now is that if the entrepreneur says that it's ten times better with some data to back up that claim, then it's worth looking at, because then it'll turn out to be only four times better, but that's enough to have some market success. If the entrepreneur says that it's four times better, then it'll only turn out to be two times better, and that's not enough for an existing market because the incumbent players have so much power and presence.

Shustek: And presumably are going to improve their own product anyway.

Baskett: And they're going to improve their own product as well. So, ten times is what the entrepreneur has to believe in order to come up with a four times better product and a successful company.

Shustek: That's a good lesson. On the face of that, would you recommend that entrepreneurs go instead after new markets as opposed to improving existing markets?

Baskett: No, not necessarily, because I've seen cases where they come in with a 10x claim and do really well. So, that's an ok route. That's a complete ok route.

Shustek: "Fulcrum Microsystems".

Baskett: Fulcrum was kind of in between the 10x-4x thing. That was a very high-performance networking technology.

Shustek: A switch?

Baskett: Switch chips at the heart of big switches that drive the Internet today. It was initially a bet on a technology out of Caltech that I had been enamored with since my Stanford days, called asynchronous logic. Most of the digital logic that gets designed and built today is synchronous: that is there is a clock which is your master, and you have to get everything done before the next tick of the clock. That's your window for getting something done. It turns out that that's a discipline on designers that's useful for most designers; where you've got so many nanoseconds to get something done. I think that discipline has enabled people to build some pretty amazing things.

Asynchronous logic is different, in that it says, well, there's no clock. It gets done when it gets done, and it tells you when it's done. You can build systems so that when everything is done, you move onto the next phase of whatever it is you're doing. The potential advantage of that is that you end up with something that's lower power and potentially faster than the clock approach, which by necessity has some things that barely get done in time and other things that are easy and then you're sitting there twiddling your thumbs.

This was a technology out of Caltech, and the team had mad enough progress to look like it could become commercial. It was a little bit of a speculation on my part to go after it, but I thought the team was really good -- they were -- and that there would be something that we could build. We worked on the basic technology for a while, and we got to the point where I said, "Ok, what is this technology really good for?" Because we were up against this established clock design paradigm that was everywhere at the time. What the team said was, "It's really good for data paths. It's not that great for control logic, but it's really good for data paths." I said, "Where are there some data paths that need to be fast?" Network switches were an obvious example, so I said, "What about using it to build network switches, the data paths, in network switches?" The team said, "Hmm, that's a good idea. Let's do that." So, we did. The first two generations of products were terrific, and they became very successful in a big market. The third generation… Because this was something that got better every year as CMOS technology progressed, the switches were getting bigger, and the data rates would go up, get faster. So you had to keep doing the next version. But generation three turned out to be an overreach. It was too big, and it was too complicated, and it didn't get done on schedule. It didn't get done on schedule pretty badly. When it finally appeared, it was late to market and it consumed way too much power. By then the company had lost its market grip to one of the big established players. We ended up selling the company to Intel.

Shustek: Couldn't they have applied the technology to some other area?

Baskett: Potentially. But we put all this effort into what we saw as a really good target market, and if we had been on time and on budget with respect to power, it would have been fine.

Shustek: I think this asynchronous idea is a great idea, but that was going back to the days when you had me reading, as my professor, papers by Jack Dennis on data flow architectures. Right?

Baskett: Yes. Exactly.

Shustek: So, this was in the late1960s?

Baskett: Yes, right.

Shustek: It still seems to be a great idea that's in search of a good application.

Baskett: It is. That's right.

Shustek: Isn't Ivan Sutherland now working on this up in Portland?

Baskett: He is, I think, yeah.

Shustek: Do you think it has promise?

Baskett: I don't know. Maybe, but I don't know what the application is now.

Shustek: Its advantage is that you can do a design based on typical propagation [delays] of the logic rather than worst case propagation, which makes a huge a difference in performance.

Baskett: Yes, exactly. Huge. Huge difference, right. Someday it will successful. There are some new application areas out there that we need to keep in mind. The one that comes to mind to me almost every day now is convolutional neural networks.

Shustek: I think you're right. Time for a new startup.

Baskett: Time for a new startup. I'm working on that one.

Shustek: Ok. "Data Domain".

Baskett: Data Domain was a really terrific experience. When I was running the Western Research Lab for Digital Equipment Corporation, I was still in touch with the university community in great detail, and was hiring people out of PhD programs around the country. I interviewed this guy who was finishing his PhD at Yale. He was a great guy; seemed to me to be a terrific candidate to work in my lab at that time. He interviewed with all the people at the lab, and everyone loved him. We wanted to hire him. I was having dinner with him after this process, talking about whether he wanted to do this or not, and he asked, "Forest, you've been an academic, and now you're in industry. If I wanted to do both of those things too, which one should I do first?" I said, "Well, Kai, I think the truth is it's easier to go from academia to industry than vice versa." He said, "Yeah, that makes sense." So he took a job at Princeton and turned me down! But we kept in touch, because he was a great guy and I was interested in what he was doing, and continued to be interested, and followed it.

Shustek: What was his name?

Baskett: Kai Li. He was the founder of Data Domain. Many years later when I was at NEA, having gone through… it's 15 years later. We had been in touch, so it wasn't an empty 15 years; I knew what he was doing. He had me be on the Princeton External Advisory Committee, so I was really very much in touch with him. He called me up one day and said, "I'm taking a sabbatical in the Bay Area and I want to think about doing a startup." I said, "Come see us. We'll set you up." We do this. We offer people what we call an "entrepreneur-in-residence" opportunity, where they can come hang out with us for three months and see whether they come up with an idea or not. I believed in this guy, and convinced my partners that we should let him do this. It's a low overhead thing, so it wasn't a huge decision to do that.

He came out, and he started working away. He had some initial ideas that were terrible, and we convinced him that, no, that's a bad idea; don't do that. Eventually he came up with this idea for backup for storage systems. The novel thing about the idea was that he had used some fairly esoteric and sophisticated algorithms for finding duplicate strings in data streams. He could efficiently find any duplicate in a long string of data. You could think about all of your data -- take all of your files for example -- and just run this variable length window over all of your files and it would say, "these 7 bits are the same as these 7 bits over here, and these 97 bits are the same as these 97 bits over here." If you look at a file system that has a lot of text in it, which most of them do, there are lots of duplicates. Tons and tons and tons of duplicates. He'd started getting this algorithm together, and had hired a couple of other people to work with him to help him. We started doing some experiments with it, and it looked like we could get a factor of 20 reduction in the size of a data set that we wanted to backup.

Shustek: So this is a general compression algorithm?

Baskett: It's a general compression algorithm.

Shustek: It could be used for lots of other things besides backup.

Baskett: Right. But we were interested in storage systems. Backup was a big problem, and a 20x reduction was way better than the 10x that was my rule of thumb. We decided that there was a business plan here, and we worked it out. He hired a guy that was a brilliant product marketing guy.

Shustek: Did you patent the algorithms?

Baskett: We patented the algorithms, definitely. And funded the company, and started working on it. We put in a seed investment to get the company going; the seed investment was like $250,000. We got it up to a point where it was time for a Series A, which is $10 million or so. We recruited Greylock [Partners]

to be a co-investor with us, because it was going to take some serious marketing effort to get this off the ground as a storage company. We said, "We've done all this work already, but we believe in being co-partners. Why don't you take half of this deal and we'll take the other half, and let's make this go." They were all for that. It was a good deal. So we did that, and we built a company, and it was a really good success. It just worked quite well. We ended up hiring a great CEO; the founder was a very technical guy, avPhD in computer science. We took the company public, and then a few years later EMC bought the public company for an even bigger price. It was a great success.

Shustek: It seems to me the technology could be used for lots of other things besides backup.

Baskett: Yes.

Shustek: Are they doing that? Are they looking at that?

Baskett: People understand that now. People looked at Data Domain and said, "Oh, my god. These algorithms work, and they're efficient." That was the thing; they'd come up with algorithms that were not exponential running time kinds of algorithms, they were linear. So, yeah, they're now widely known. Widely used.

Shustek: It seems to me you could use those kinds of algorithms for understanding of databases as well is looking for duplicates, like looking for plagiarism.

Baskett: Ah, yes, plagiarism. I hadn't thought about that. But, I'm not sure how big that market is.

Shustek: That's true. Well, that sounds like a good success.

Baskett: Yeah. It was an example of where a relationship really made a difference. I think that's why I had it on my list.

Shustek: A personal relationship.

Baskett: A personal relationship with someone deeply technical, but that you nurtured over time.

Shustek: I presume you helped him get the rest of the company established.

Baskett: Mm-hmm.

Shustek: Bringing together business people, and marketing people, and so forth.

Baskett: He's one of the people that I can count on to call new entrepreneurs to be a reference for me. He's done that many times.

Shustek: What is he doing now?

Baskett: He's still a Princeton professor. After he got this company started, he took one-year leaves. Then it was part time. But he maintained his professorship at Princeton.

Shustek: This is interesting. When he first took his leave of absence to be an entrepreneur-in-residence at NEA he didn't have a strong idea. He was working on several different ideas, some of which didn't pan out.

Baskett: Mm-hmm.

Shustek: How often does that happen? Don't people often have a passion for a particular thing?

Baskett: I think it happens both ways. I think people decide that they've got a lot of ideas, and if they just really confront what the market opportunities are, that they can figure out that one of them is going to go somewhere. I think that's where he was. He has had lots of ideas over the years. He's a very creative guy.

Shustek: "T-RAM Semiconductor".

Baskett: Ah, T-RAM. We probably don't have to spend a lot of time on that, because it's a little bit like Foveon in that it was a technology for faster, denser memory cells. Memory is a big business, but it's also a business that is dominated by some really big players. It just wasn't good enough. Didn't make it.

Shustek: Wasn't 10x?

Baskett: Wasn't 10x, yep.

Shustek: What lessons do you learn from that?

Baskett: That was a case where we wanted to license the technology to big semiconductor companies. I didn't appreciate how difficult that task is, at the time. They have their roadmap, and they have their internal people, and getting their attention is really hard.

Shustek: I know companies that have had success with that, like Rambus, for example.

Baskett: Like Rambus. Rambus did.

Shustek: They're the poster boy for that.

Baskett: Yeah, exactly. A good counter example, I think.

Shustek: "Luxtera".

Baskett: Luxtera. This is an example of longevity, in that I saw this company first in the year 2000, 17 years ago. It was out of Caltech. A group of guys who had figured out how to do optical processing in silicon. The basic idea is pretty simple. If you etch a channel in silicon, over silicon dioxide, and fill the channel with silicon dioxide, which is easy to do, you've got something that's equivalent to an optical fiber because the index, the refraction between what's inside the channel and what's outside the channel is different. So if you project light down that channel, it stays in it. Just bounces, along it does. The interesting part is that if you then put something like a capacitor on top of that channel and charge it up, it changes the index of refraction of the channel, and the light will speed up or slow down depending on what the strength of the field is that you apply. You can build what are called Mach-Zehnder interferometers, by having loops that you circulate the light in with different electric fields applying to different parts of the loop. When the channels come together, you can cause them to cancel or to reinforce. So you can do on-off switching of light electrically inside a piece of planar silicon. It's remarkable. Amazing. Optical communications was a big deal even then, being invested in at Bell Lab many, many, many years before. Optical switching was something that was becoming more and more and more important.

Shustek: Were these optical switches fast enough to be interesting?

Baskett: They were very fast. They were faster than anything else around. At the time, there was another technology called VCSEL technology. VCSEL stands for Vertical Cavity Surface-Emitting Lasers, VCSELs. That was a way to put a laser in a semiconductor device that you could switch off and on, also electrically. But it was a directly modulated laser, as opposed to having a laser that's shining a continuous beam into this channel and then you're building an interferometer to switch it on and off. Two different approaches. The VCSEL technology was slower, and we knew from a semiconductor

processing perspective that it was limited in how far it could go, that it would become more and more power hungry and the quality of the switching would become less and less as you push to higher and higher speeds. The Luxtera optical switching technology didn't have those problems, but had very high quality switching and very high speeds. So we embarked on making that technology a market success back in the year 2000. VCSEL technology kept getting better. We kept getting better. They kept getting better. We kept getting better. They kept getting better. We had modest sales because of the fact that they kept getting better.

Shustek: What actually were you selling?

Baskett: Transducer.

Shustek: Okay, an electrical-optical transducer.

Baskett: Yeah. The thing where you're generating data and you want to send it somewhere inside a computer, and you want to turn it into pulses on an optical fiber so you can send it a long distance.

Shustek: As opposed to using this technology for computation?

Baskett: Yes, right; this was strictly communications. There are some potential computation applications, but it's not clear that they're dense enough to make a difference. But the communications side was easy. The company still had promise, and we kept investing and we tried to control the burn rate. When we got to ten gigabits per second, the VCSEL technology actually did run out of steam. Now the company is making 25 and 50 gigabit per second optical transducers. The rise of the hyperscale data center, where you've got many thousands of machines in a very large warehouse, turns out to need moderate-distance very high speed communications for those machines to talk to each other. So the market now is huge, and this is the only technology that works.

Shustek: Over the course of those 15 years, or whatever it's been, there must have been some interesting partner meetings about whether to continue to invest in Luxtera.

Baskett: I know. I know.

Shustek: How did that go?

Baskett: It was a struggle, because it looked like the technology of the future, and it always would be. It's one of those situations, because it kept moving out. But it was sufficiently compelling technically that

we just stuck with it. Eventually our predictions about what the physical limitations of these different approaches panned out.

Shustek: Was that a case where you had a co-investor?

Baskett: Yeah, we had several co-investors.

Shustek: Did the founding team stay around, or did they leave?

Baskett: There were quite a few changes over the years. The initial technical genius moved on. The initial CEO moved on. But we had built a team that kind of understood this technology. There were some engineers there who were really dedicated to it. They really understood it, and are still there.

Shustek: It shows the value of long-term thinking, that venture capitalists can apply, that public markets won't.

Baskett: Yeah, public markets will not.

Shustek: They could not have survived as a public company.

Baskett: They could not have survived. That's right.

Shustek: "Tableau Software".

Baskett: Tableau. Tableau is one of my biggest successes, actually, from both a company perspective and a financial perspective. At Silicon Graphics, of course, I was involved with a lot of graphics applications of the machines that we made. One of our big customers was Pixar, because they used our machines to make movies. That was great, and I visited up there many times. One of the people that I got to know during that time was Pat Hanrahan. Pat is a graphics wizard in a variety of ways. He's the only person I know who has three academy awards on his fireplace for technical achievement in graphics. Movies. Not too long after I was at NEA, Pat had left Pixar where he'd done all these incredible things. First he went to Princeton, where he was a professor. One day when I was still at Silicon Graphics, John Hennessy, who's the president of Stanford, calls me up and said, "Forest, we're trying to hire Pat Hanrahan. Can you help us?" I said, "Sure," because I knew Pat from is Pixar days. So I called up Pat and said, "Pat, why don't you come to Stanford? Why would you not come? You'd have a lot more fun than you can have at Princeton." Again, I was on the Princeton External Advisory Committee, so I'd seen him there as well. Funny network. Pat said, "Well, how can I afford to live in Palo Alto? It's so expensive

there." Princeton doesn't have the same cost-of-living structure. I said, "You can consult one day at week at Silicon Graphics. I'll pay for that." He said, "Really?" I said, "Oh, yeah. Not a problem." So he came to Stanford and became a consultant at SGI. That went great. That was terrific. Then a few years later I was at NEA, and Pat calls me up. He says, "I've been working on this visualization software for the Department of Defense, and it works really well. A couple of us think it can be commercialized." I said, "Well, come over." So he came over that day, and talked about what they were doing. I dragged one of my partners in to talk to him about it, and we said, "We'd love to fund you to make this a startup." It took a little bit longer to actually get the terms set, because they were pretty demanding. He had a really smart business partner. But we got it set up, and we became the only Series A investor in Tableau.

Shustek: What was the product?

Baskett: It was data visualization for big data sets, to do all kinds of interactive visual analysis. Drag and drop. Take these big complicated multidimensional data sets and --

Shustek: An offshoot of what you and I saw at SLAC [in the 1970s], that John Tukey had done in "PRIM-9".

Baskett: Yes, exactly right. This was a dramatic generalization of that. Yes, exactly. So I kind of knew what the problem was, and I could see that he was doing amazing things in terms of how fast it was and what big data sets he could handle and how beautiful the results were.

Shustek: Was this for scientific and engineering use, or business use?

Baskett: It was for business use. Business intelligence is what the area was called at the time. There were some big companies that were already there, but we founded the company. They started with a desktop software product, and expanded to a server product, and now a cloud product. They grew by leaps and bounds. I think they have 25,000 different corporate customers today. Big. They went public in about, let's see, four years ago, and they're a multibillion dollar company now.

Shustek: The intellectual antecedent that I mentioned, PRIM-9, was designed for scientific data and visualization.

Baskett: Yeah.

Shustek: Is there an application here, or is there just not a big enough market?

Baskett: Well, there could be. Tableau is used by everyone now. Scientists use it. CFOs use it frequently, because it's easy to use. It's widely used by the high-speed trading world for example. It's just everywhere. It's just a huge success.

Shustek: What is the future of a company like that? It's a successful independent--

Baskett: The problem that the company faces now is how to transition to a world where most of this kind of computation is cloud-based. They've been a little slow to become cloud-based, and they're facing competition from both Microsoft and Amazon. So their stock price in the last couple of years has suffered because of this perceived competition from these big players. But they're getting it done. They're still way better than anything else out there. The question now is, can they make the same transition to the cloud that Adobe made years ago? It's that kind of software problem.

Shustek: When a company like Tableau goes public, do you or NEA have any continued involvement?

Baskett: I'm still on the board at Tableau.

Shustek: So the simple answer is yes.

Baskett: Yes. I'm likely to end that soon, because that's not our job, to be on public boards. But it's been a useful perspective on the whole big data world to be on that board, and see how that world is developing, and what the opportunities are, and what the other players are. We invest in big data kinds of things, so it's been really useful.

Shustek: What's it like being on a public company board as opposed to a private company?

Baskett: Again, it's not our business to do that in general. But it is useful sometimes to be on a board where they see an industry that is important to our firm in a broad sense. I have the perspective that I have on what the competition is, and who the players are, and where these might be some opportunities. As long as we're not investing in some company that might compete with them, then that's ok. If we were to decide to invest in some potential competitor, then I would have exited the Tableau board quite some time ago.

Shustek: "SiBEAM".

Baskett: SiBEAM, ok. This is another example of being too early. These are some people out of Berkeley that I had discovered. They came out of Bob Brodersen's lab at Berkeley. Bob ran the Wireless

Research Center, and they had figured out how to build 60 gigahertz radios in CMOS. 60 gigahertz is, again, unlicensed spectrum. The thing about 60 gigahertz is that the wavelength is very small. What that means is the antennas are very small too. So you can actually put an array of antennas on a piece of CMOS. You can do what the phased-array radar people do, on a little piece of silicon that's about this big. You can aim these beams of 60 gigahertz information wherever you want. It's just remarkable technology, really remarkable. Didn't know what to do with it.

Shustek: What's the distance?

Baskett: But, the distance… Ok, the reason that the 60 gigahertz band is unlicensed is because it is what's called the oxygen absorption band. Oxygen absorbs that energy, so you can't go very far as long as you're in air.

Shustek: It would be great in space!

Baskett: Yeah, right; it would be great in space, but there's no business in space. You can go 30 feet. You can go ten meters without much of a power budget, and you can go really fast. This was many years ago, and we could do ten gigabits per second easily. Our initial target was flat screen TVs that you hang on the wall. You want to have your receiver here, and your TV over there, and you want to send the information to the TV without wires. Beautiful product. Worked great. But a couple of issues. One, the TV market is a very cost conscious, high volume consumer market, and we were a little more expensive than wires. The technology had other uses that we weren't exploring, because that seemed like a natural fit for what we could do. Again, we ran out of steam. The technology is coming back now for other uses, because everyone understands that you can do this now.

Shustek: It seems like high-speed, shorter range, directional communication could be used for lots of things.

Baskett: Yep. Yep.

Shustek: Does the company still exist?

Baskett: Yes it does, actually. We sold it to Silicon Light, and then Silicon Light was acquired by-- I can't remember. Another somewhat bigger company. Yeah, they're still slaving away.

Shustek: Technology continues on.

Baskett: Yeah, it continues on. It wasn't a big success as an investment though. Technologically it was beautiful, but didn't make any money.

Shustek: Is that sometimes a problem, because you've fallen in love with technology and don't see a clear path to making it into a successful company?

Baskett: Right. Yep.

Shustek: "Arch Rock".

Baskett: Arch Rock is another example of being way too early. These were, again, some people out of Berkeley that I had known for some time. They basically were doing as a business in those days, which was ten years ago, what today we call the Internet of Things. They built little sensors with little radios that formed a mesh, communicated, got to a backhaul point, and you could upload the information. There was a lot of networking involvement. Meshes were not well developed, about how to do them and what the protocol should be and how to make them reliable. There's a whole set of how do you build this complicated software on these little tiny devices. Worked great, was terrific stuff. But the market wasn't there. The "Internet of Things" term hadn't been invented. Again, we ran out of steam before the market happen.

Shustek: The road, unfortunately, is littered with many corpses along the way.

Baskett: Yes. Yes.

Shustek: I remember Ken Oshman working on, what was it, LON Systems, Local Operating Networks?

Baskett: Yeah. His company was--

Shustek: It was not LON, but something like that.

Baskett: Yeah. It'll come to me. [It was Echelon, making LonWorks – ed.]

Shustek: I remember they had the idea of having these little wafers that were about the size of a nickel that would go in the base of a light bulb, and then automatically control the light bulb remotely.

Baskett: Mm-hmm, yeah. There are a whole bunch of companies that are now making that product.

Shustek: Right. It's often not an advantage to be too early.

Baskett: That's right. Exactly.

Shustek: "Audience".

Baskett: Audience was an example of getting killed by your customer. Not quite, but somewhat. Audience was a technology. Actually Carver Mead was one of the backers, sponsors, of this. His graduate student out of Caltech invented this technology, and he was part of the board for a long time. The notion was that by looking at a different way of characterizing speech, you could in a sense generate a signal processing equivalent of a phoneme, and recognize that easily. You could take two microphones, and use that kind of characterization of speech to do spatial localization of speakers. You could, if you had two microphones, distinguish you speaking from a room full of people. You could get this dramatic improvement in intelligibility in noisy spots. That applied not only to other people speaking, but to wind and to fan noise and a whole variety of things.

They came to see me the first time, and they were going to use this to go after speech recognition as a market, because they had these phoneme things that they could figure out. I said, maybe, but it's going to be hard. The nuance was an existing company that was already out there doing that. I think, "I don't like business model," so I turned them down. Somebody else funded them. Tallwood funded them. Then they got to a Series B, and they came back to see me and they said, "Well, we've got this different business model now. We see that cell phones need to be able to do this to know that I'm the speaker and everything else is noise." I said, "That makes sense as a business model." So I became the lead on a Series B investment. We developed the chips. Our big break was to become incorporated into the iPhone 4; a lot of business there. Samsung came along shortly thereafter and became another big customer and big ramp. Big success, and went public. It was great. This was still not a huge company, but pretty big.

Then the iPhone 5 came out and Apple had decided to do it themselves. So we were not in the iPhone 5. It was hard on the company, but we had by then acquired a large number of other handset manufacturers as customers. We survived that, although it was a huge hit on the stock price, as you can imagine. There were shareholder lawsuits, because people said, "Well, you must have known," which we didn't. But that happens, and they were all dismissed.

Shustek: Did Apple not use any of the IP?

Baskett: Not that we could tell. No, they didn't. They were using a more standard two-microphone sound interference kind of thing.

Shustek: What was Audience actually producing? Any hardware, or designs?

Baskett: They were producing chips that went into phones. When I funded them, their notion was to be a software company, because all they needed were microphones which other people supplied, and some software to operate on the digital version of what came out of those microphones. Their model was to build this software into the applications processor on the phone. Technically makes perfect sense. I said, yeah, technically that makes sense, but you're not going to succeed that way.

Shustek: You're not going to make money that way.

Baskett: You're not going to make money that way. First of all, the price will be very low because it's just software from the cell phone makers' perspective. Secondly, getting into that software stack is really hard. It's complicated. It's delicate. It's fragile. People don't like to change it. They're not going to want to go there. I said, "Why don't you put all that software into a separate chip, and then just have a single microphone output that has filtered out all that stuff?" Encapsulate all that software into hardware realization.

Shustek: It's audio pre-processing.

Baskett: Audio pre-processing, right. Then you don't have to touch the software stack on the applications processor on the phone. As far as they can tell, it's just a microphone. They went for it, where they would not go for the other, as I had predicted.

Shustek: Interesting.

Baskett: Apple went for it. Samsung went for it. All the other players went for it.

Shustek: Even Apple, a company who likes to make their own hardware?

Baskett: Yes. Yes. .

Shustek: What about applications for hearing aids?

Baskett: Sure, there are potential applications for hearing aids. But as you know, that's a really nasty business.

Shustek: "Solar Junction". That doesn't sound like an IT company!

Baskett: No! I got interested in the energy world many years ago. Oil was escalating in price, and solar technology was improving by leaps and bounds. Some guys out of Stanford had figured out how to make solar cells that were 43% efficient.

Shustek: That's huge.

Baskett: That's huge. That's huge. It doesn't quite past the test though. In order to get that efficiency, the cells were really expensive to make, to get that efficiency. So in order for it to be economical, you had to put a concentrator on it. Concentrators are cheap. The cells are expensive. You could use a small number of cells and capture a lot of sun.

Shustek: They can tolerate high temperature?

Baskett: They can tolerate the heat; it's not a problem. Those cells still have the world record in terms of efficiency still today, even though the company is gone. It's another example where 2X is not enough. And you were fighting the oil companies. The energy world turns out to be a very tough place to do business.

Shustek: So sensitive to the price of oil and gas.

Baskett: Right. Yeah.

Shustek: Is that a technology which you think will have value at some point in the future?

Baskett: Maybe. Perhaps. I invested in -- I don't know if it's on here, it probably isn't -- Alta Devices. A similar kind of idea except that it was a more mass producible: cheaper, high efficiency solar cell. But it still meant that you had to build a factory, which was labor intensive. So it wasn't as cheap as it could be. It certainly didn't meet my 10X requirement. But it produced, like, 30% efficient solar cells, just plain cells. When they wanted me to invest I said, "I don't like your business model. But I really like your technology, so I'll invest a little bit." We invested a little bit, and I tried to persuade the company that the market that they should go after were solar cells for drones. They didn't until they were just about out of money and couldn't raise any more. By then had realized that that was a market that would work. We sold the company to a Chinese conglomerate. It's the only existence [??] and it is being used for that, but not a big success.

Shustek: There are applications like that that are independent of the price of oil and gas.

Baskett: Exactly, Right. Weight matters, and these were very thin lightweight cells, in addition to being high efficiency.

Shustek: It's sometimes frustrating, because it seems so obvious that there is good technology. Yet you have to struggle so hard to find a product and a market match that makes it a commercial success.

Baskett: Absolutely.

Shustek: It's the way the world works. "Serious Energy". It sounds like another energy company.

Baskett: Yeah. Again a hard business to be in.

Shustek: Photocells again?

Baskett: In this case it was energy savings for buildings. They had very high efficiency insulated windows, and they had building automation systems for managing HVAC systems and lighting systems. But that's a very fragmented industry; lots of competition. It's hard to build with--

Shustek: Lots of big old line companies.

Baskett: Big old line companies in it. It's a hard place to go.

Shustek: "Fusion-io".

Baskett: Fusion-io was fun, and it's an example of doing things right, in a way. I met them at a show, where they had built their prototype and they were showing it along with 60 other companies. It's one of these "Gong Show" kinds of things; you have five minutes to describe what you're doing. They were building solid state disks, which are now quite popular. But this was one of the early solid state disks, which uses a technology called "flash", which is the memory technology that's in your cell phone or your camera. It's cheap, and pretty fast, and solid state. It's faster than a rotating disk storage system by a lot. They had built a card that you could plug into your servers, that would have a lot of this flash on it, and act like a disk, except it was solid state. It was a lot faster. More expensive, but a lot faster. For demanding data applications in computers it was a great deal. They had figured out how to deal with a lot of the problems that flash has, with respect to wear leveling, and append-only, and limited-write cycles, and had

done it really, really elegantly. So, we invested in them. This was also the time when big data centers at Apple and Google and Facebook and other places were taking off.

Shustek: Were they building their own flash chips?

Baskett: No, they were just buying flash chips. All of the intellectual property was in the algorithms that were in an FPGA that were the controller on that board full of somebody else's flash chips. They worked really well. Really reliable, very high-speed, none of the lifetime issues that flash would otherwise have. They had really good software interfaces. They attacked a hard problem, which is that every different brand of computer had a different disk interface in it. Your "disk" had to act like a real disk, which meant you had to pretend to have all the peculiar idiosyncrasies of all the disk drives that were out there. That's hard, but they just, "Well, work at it." Clunk, clunk, clunk, clunk, clunk, one after another. Go after the high volume ones first; standard, kind of "cut the tall weeds first." It became tremendously successful at Apple and Facebook and several other big data centers. Those people would tell us that they couldn't have expanded their data centers the way they did without this technology.

The company took off. The sales ramped really fast. The company went public. It was a big success. But we charged a premium for this pretty nice technology, and other people started making the solid state disk cards without the same reliability characteristics, but at a much lower price. It became a very competitive business. We made money from exiting after an IPO, but then the company began to struggle because it became such a competitive marketplace and became commoditized, as some of these markets do. The company then eventually got purchased by SanDisk, and then SanDisk got bought by Western Digital. So the technology is still out there. It was a successful startup, but it was one where the market was so big and so important that it became commoditized.

Shustek: This is a movie we've seen many times before, where an early developer with clever technology at a high price point becomes very successful, and then is a victim of commoditization. When entrepreneurs come in and pitch an idea to you, you said you wanted the long-term vision as well as the near-term market. What if the long-term vision can't succeed?

Baskett: Yeah, you're right. I don't know. Maybe it was just that it was so clear that they had something that worked. There were no additional problems to solve, other than getting all these drivers to function correctly, and we knew how to do that. So it was a short-term opportunity and we jumped in and took advantage of it.

Shustek: There are companies you invest in that you know won't be around 15 years from now, but still make viable investments?

Baskett: Right. Yep.

Shustek: "SuVolta".

Baskett: How are we doing? Are we running out of time?

Shustek: There's still six or eight companies on the list. You can skip some if you'd like.

Baskett: I was thinking about being a little more selective. SuVolta is interesting because it works with another company that's on the list called PsiKick. They're kind of a pair. SuVolta was a semiconductor technology that enabled low power operation, and we can make devices that were about half the power of conventional devices. Like SRAM, for example, which is a power consumptive kind of device.

Shustek: Do you pay for it in speed?

Baskett: Not really. You paid for it by needing a special process at the foundry, and that turned out to be the sticking point for SuVolta. Getting 2X, but getting the foundry to change their process didn't compute. That one eventually flamed out. Also, the company was run by people who had experience in the licensing business, and they wanted to do a licensing company. The investors didn't want to do a licensing company, but you can't force a company to change its stripes, so to speak. Our vision was: figure out where you can build something with ultra-low power that makes sense, and sell that. Don't sell the technology. But that's not what the company tried to do. It tried to get the foundries to buy the licenses to the technology. It didn't work.

I talk about PsiKick because that's a recent company that has also developed an ultra-low power technology. It's dramatically better than what SuVolta had. It's more like the 10X rather than the 2X. And they have taken our advice to build something with the technology rather than licensing it. The obvious thing to build are sensors for the Internet of Things. It turns out that the characteristics of the devices that they can build are so low power that they can power a gadget that has a processor, a sensor, a radio, and an energy harvesting interface, with just the energy that they harvest. No battery!

Shustek: From mechanical motion?

Baskett: From almost any available energy source: photons, mechanical energy, thermal differences, RF. Whatever your favorite available energy source for your application might be, they can use it.

Shustek: Sounds like magic.

Baskett: It sounds like magic. It's very cool. But it's taking a technology that blows you away at a fundamental level, and saying: where can we apply this technology so that we can sell something of significant value?

Shustek: It sounds like, in general, you're not a fan of the technology licensing model.

Baskett: No.

Shustek: You'd rather see a company with a great idea make a product, even if they have to concentrate on a subset of what the ultimate market might be?

Baskett: Exactly right.

Shustek: Why is that?

Baskett: Well, the licensing business is just really hard. There are some notable exceptions. We talked about Rambus. Dolby often gets mentioned as another example, although Dolby is not a very big company. Then Qualcomm was a huge success in the licensing business, although they quickly devolved into making stuff that they sold. Otherwise, there's just lots of either small companies or failures in the licensing business.

Shustek: Sometimes that seems like a shame, because you might imagine you could have greater impact by licensing your technology to many companies in many different fields, as opposed to building something in one particular field.

Baskett: Right. Yeah. If you have this notion of, "How can I change the world -- I've got this idea; let's get it out there as broadly as possible," like many of us in universities had, then that's a natural thing to try to do. But in practice, it's not a great way to build big companies. As a consequence it's hard to get funding for that.

Shustek: "Bitglass".

Baskett: Bitglass. That's too complicated.

Shustek: Okay. We can skip it.

Baskett: Yeah. We talked about GoEuro?

Shustek: We did.

Baskett: I think maybe the only one left that's worth talking about is Pilot.AI.

Shustek: Ok.

Baskett: Because machine intelligence -- artificial intelligence -- is a big deal. I like to characterize machine intelligence as the new Moore's Law. The reason is that Moore's Law has just changed our lives many, many times over the last 50 years, I guess, because it kept making new things possible. These resulted in new devices, new systems, new markets, new ways of living. It's just been amazing. But Moore's Law is, in fact, slowing down. The underlying physics is becoming a real limitation. The fact that we can do four times or eight times what we used to be able to do in the same space -- whether it's power, or physical space, or whatever -- is becoming less of an event. But machine intelligence is something that has become really powerful in a way that is remarkable. It's enabling that same old silicon technology to do some things that we never imagined were possible before. It's giving us a new opportunity to have a new kind of functionality improvement, which is in this "ten times" category.

Pilot.AI is interesting as a startup. They've just barely gotten started. We're an early investor. One of the things that they have concentrated on has been a problem for machine intelligence. Many of the applications are based on convolutional neural networks. People have figured out how to train them so that they can do these remarkable things, like speech recognition, and imaging recognition, and robot training, and a whole variety of things. But the data that typically gets generated by a training system is voluminous; it takes a big powerful computer with lots of memory and lots of bandwidth to process. The question is: how can you accomplish the same thing in dramatically less space, and less energy, and less power, than the straightforward approach of using convolutional neural networks.

These are some people out of Stanford that have figured out how to compress a pretty sophisticated trained neural network down to a small space that a modest memory and a modest process can execute. With their technology, you can imagine a security camera that can recognize the people that are allowed, and flag the people that it doesn't recognize, on a security camera that's smaller than a baseball.

Shustek: They reduce both the memory and computational footprint of what the algorithm requires to execute?

Baskett: Yes, right.

Shustek: In some sense, it's like pre-compiling?

Baskett: It's like pre-compiling. Yes, right.

Shustek: It seems like a technology that would have lots of applications.

Baskett: It may have lots of applications. For me, it was interesting. When I first talked to the team they really didn't want to tell me how it worked. They would show me what they could do, which is pretty remarkable, but they didn't want to tell me how it worked. But a few things slipped out. I had been involved in large scale computation for years in various ways, so after not very long, I said, "Oh, I get it. I know how this works. This is a really cool idea."

Shustek: But these are academics. Don't they write papers to describe what they're doing?

Baskett: Well, they were smart enough to not write papers about this.

Shustek: Or file patents?

Baskett: They have filed some patents. But they're very careful about what patents they file, because the fundamental idea is one that if I told too many people about it there would be competition.

Shustek: That must be an interesting dilemma for entrepreneurs in general.

Baskett: It is. It is.

Shustek: Do they patent and therefore publicly disclose their ideas, or keep them secret? What's your advice?

Baskett: I think you should be very careful about patents. They're not necessarily a good idea. In the business world they're mostly used for defensive purposes. That's not true in biopharma, where you patent a drug and then you license it. The profits are so large that the royalties are wonderful. That's not the case in the technology world. Patents, as I said, are mostly defensive. One company tries to sue you, you sue them back. I think as far as protecting ideas -- not such a great thing to do.

Shustek: Keep them secret instead.

Baskett: Keep them secret instead, yes. That's an overgeneralization, but there's a lot of truth to it.

Shustek: Do you then have to worry about people who will reverse-engineer your technology?

Baskett: Of course, yeah.

Shustek: There have been a number of high profile technologists who have expressed some concerns about the dangers of AI becoming more successful. Do you share those concerns?

Baskett: Not really. I think any new technology has those kinds of dangers associated with them. For the most part, technology advances have enabled us to do new things. It's up to us as people who are doing new things as to whether those new things are good things or bad things. Sure, we have to think about privacy issues -- Big Brother watching you, and all those kinds of things. But I think that's more of a human problem than a technology problem.

Shustek: What about the social impacts? About the people whose jobs will disappear because of new technology?

Baskett: I think you can make a pretty persuasive argument that the jobs change, but they don't disappear; that we've created more jobs than we've destroyed. They're different. But there's just so much more we can do now. There's so many different kinds of skills that the world needs and can use that are based on new technologies. I think we all have to be adaptable. That's just part of being a successful species. But I don't feel like we're cutting off our nose to spite our face so to speak. I think it's progress. I think it's progress.

Shustek: This has been wonderful. Let me end by asking what your advice would be to a young person wanting to be in technology. What are the hot fields? Where should he go? What should he or she do?

Baskett: Well, from a career perspective, that's really hard to say. When I started out I said: Gee, what am I going to do? Computers are new things (they were brand new when I was young), and they seemed really interesting, and might be really fun to work on. So that's what I did. I don't know what a young person today would look at and say, as a new thing, "that would be fun to do." I think there are lots of possibilities. I was encouraged to be intellectually curious and to understand how the world works, from as young as I can remember. In fact, I think that's been the key.

END OF THE INTERVIEW

