Oral History of Peter Hart, part 2

Interviewed by:
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Brock: Well, Peter, we were talking about Shakey when we last spoke and we had sketched out your kind of evolving roles in the project; we had talked about Charlie Rosen's conception of the Shakey project as this kind of integrative platform for all these different strands of research in what we might call artificial intelligence. And so I would like maybe to return us to Shakey and to ask two questions of you to kind of get us back into the flow. One would be well, your reflections on the importance of Shakey first for you and then in general for kind of the advance in the story of computing and robotics -- so personally and then historically if you will.

Hart: Okay, good. Well, thank you again. I don't recall if last time-- to answer your first question, the importance from me personally, I don't recall if I related the story about how there was one incidence with Charlie that really was a turning point for me. Did we talk about--

Brock: --what are you afraid of?

Hart: Yes.

Brock: Yes, we did.

Hart: Was that there?

Brock: It was, yeah.

Hart: So- so that was one. Really only in hindsight can I appreciate how it really was a turning point for me and changed my attitude ultimately towards risk, towards personal risk, what do you risk by trying something new, uncertain outcomes, and I might have last time continued with the story about how I consulted for the British government.

Brock: We did not--

Hart: Well, that's an interesting-- perhaps an interesting little—

Brock: That would be wonderful.

Hart: So while I was still in my SRI portion of my career but I think it might have been after the Shakey project ended, I consulted with a small group of people for the British. It was variously called the Ministry of Industry or the Department of Industry or something like that. It was a cabinet-level department, and the question for this small group of us, maybe no more than half a dozen at most, were asked to address was "Why didn't Britain have more entrepreneurship?". They pointed out that Britain had a glorious history of innovation going back to the earliest days of computers, the invention of what they called the B box and today is called the index register, but very little commercialization. And we spent a week consulting with or being visited by the top strata of the British establishment: The financial sector, the City as they called it with a capital C, the academic sector, the labor unions, trade unions, the government people, the civil-service bureaucracy, quite a cross-section of that kind of leadership community. And they said, "Well, what can we do?" and they used words like "Britain is sclerotic, arthritic," etcetera. "We can't ever seem to

get commercialization" and so there was a lot of stroking of beards and so forth. And I had a hypothesis which I can by no means prove but I think is a somewhat interesting one and I thought the reason that they really had such a small deal flow-- we spoke to their very small venture-capital community and we were told, "Our deal flow, or the opportunities to invest, is a tiny fraction of what you see in Silicon Valley." And my conclusion was that it was because of the penalty of failure. And I later discovered a very parallel situation in Japan. I visited Japan maybe 75 times, it was very similar in that respect. And the issue was this, that if you try something in the British business culture, something like a startup, and you fail: My impression, not ever having lived, there but my impression listening to all of these really very informed, well-educated people was that it was kind of a black mark on you for life. And I asked one of the people there-- I said, "Well, if I tried to do a startup and it was I'll call it a noble failure, a great attempt but outside forces, etcetera, and we failed, would people say, 'Good enough chap, that Hart, but you did hear about his'"-- and I put it that bluntly and this other gentleman said, "That's absolutely right"; it's not that I could never get another job but it would always be remembered. And the contrast I'll offer is that one time I was at a fundraising event. And it was quite a distinguished group of people including some venture capitalists. And one venture capitalist, a very prominent person in the Valley who I'll leave unnamed who I was acquainted with but no more than that, came to me and he said to me, "Peter," he said, "I heard about your last company," which happened after eight years to have been a victim of the biggest recession before 2008, -- maybe we'll get to that part of the story later, but in any case for this story he said, "I heard about your last company." He said, "I know it was a big deal for you but nobody else here gives a"-- He said, "Your next business idea, you bring it to me first." And so the contrast between that and the many-years-earlier story in Britain I thought was really striking, that if you had a startup here that didn't do what you hoped for you got some experience; we learn more from our failures than our successes, etcetera, etcetera. In the American culture, whatever negatives you might have, a fabulous attribute is you get to try as many times as you want. You can step up to the plate and take as many cuts as you want. You don't do well in school, you drop out; try again. You drop out again try again. You want to do something, go to junior college. You drop out; try again. Oh, you made it through. Now you can go to a four-year school. Many other countries in the world are nothing like that. In Japan or in Britain if you do poorly on some nationwide test in your adolescence my understanding is that you get tracked this way and never that way. Again I'm not an expert; that's my impression. Our culture is very different. You fail at anything and assuming you haven't done something truly reprehensible you get to try again and that's a marvelous and I think under-appreciated aspect of our society around these parts at least. And it's a long story about the poke in the chest from Charlie and what are you scared of, what's the worst that can happen, if you fail at this you can still try again, and that kind of closes the loop on that somewhat lengthy digression.

Brock: When was that UK--

Hart: It was in the late 1970s; it might have been around '78 or '9 or sometime around then. So the Shakey project ended in '72 and this was some years later; I was-- I suppose I was the director of the Artificial Intelligence Center at SRI in those days. So getting back to your question, what did the Shakey project mean for me: So that turning point was a really big deal that flipped a switch in my mind. I was much better able to accept risk and from that point on I accepted increasingly large personal risks and it all turned out just fine even though every particular venture didn't turn out as I hoped. No regrets, no

complaints about my path. So that was a big deal. The other thing that was a big deal for me personally was that, as I think I might have mentioned last time, there was so little that had been done in artificial intelligence up to that point that there was so much low-hanging fruit and we could just pick it everyplace you looked. Another aspect of that was that again because there was so little that had been done you could, every couple of years say, move to a different what today would be considered a very narrow subspecialty of artificial intelligence and actually make meaningful contributions because there wasn't a thousand papers <laughs> to catch up on or something like that; there was almost no prior art. And so over a period of a handful of years-- say a half a dozen years I was able to work in automatic problem solving and planning and a lot of work in computer vision and scene analysis, a lot of work in heuristic search and shortest paths and so forth. Today each of those are their own field and it would take quite-- there'd be quite a high switching cost-- kind of context switching cost for a researcher to move from one to the next to the next, but back then any of us-- I wasn't anything special-- any of us could move not every six months but maybe every couple of years to an entirely new area, whatever we needed to do for Shakey, whatever we needed-- whatever we found interesting for ourselves. So that was-- and some of my-- I suppose I've been cited quite a few times and my most cited work all stems I think from that period.

Brock: Did it feel at the time that you were shifting areas or did it seem like you were just exploring new questions in kind of this general area?

Hart: It was pretty clearly a shift of disciplines so for example the work that we did on the A Star [A*] Shortest Path Algorithm, I mean that would have taken a stroke of insight. Somebody once asked me was it inspiration or perspiration. It was mostly inspiration; a bit of perspiration followed but it was mostly inspiration after that it was formal mathematics. We proved theorems. I don't remember if we covered that—

Brock: We did.

Hart: I think we did, yeah. That was quite different from robot planning, problem solving, execution monitoring and error recovery. You didn't prove theorems about that, you invented algorithms, and then in computer vision again a different story. So we all thought of it as AI. We didn't make any distinction between AI and robotics, at least I didn't, but it was pretty obvious that we were using different technical tools to address different open problems. Then getting on to your second question, what did Shakey mean for the field, what was the-- so a few years ago-- I think I might have pointed this out in the previous session-- people around the world noticed that it was the 50th anniversary of Shakey by some measure of whenever it started. And so for two or three years various AI and robotics conferences really around the world were having special plenary celebration sessions, panels, talks on Shakey, and I went to several of them and spoke at several of them. And one of the big questions that came up was what was Shakey's biggest contribution and everybody had a point of view and I can offer mine.

Brock: That would be great.

Hart: My point of view was that it was no single algorithmic or narrowly defined technical contribution. Some people said A Star [A*] was really a big deal because as you know now it's used everywhere; people use it every day without of course knowing what's under the hood. I thought that the big deal

about Shakey-- and again none of us foresaw this at the time; this is all 20/20 hindsight-- I think the big deal about Shakey was the totality of the existence proof,. That it was possible to build an autonomous vehicle we called it in those days, robot if you like, that could perceive its environment, could solve problems for which it had no previously programmed solution, that it could execute that solution in the real world, that it could monitor how was it doing because things go wrong in the real world, it could correct, it could overcome errors of execution, and it could even learn in a certain sense by using plans that it had computed for the solution of future problems. It could take a plan, generalize it in a certain sense and say, "You know, I could use this in some future problem by applying it, changing variables and so on and so forth" so it was the totality, in my opinion, that this was possible. Before Shakey it really was science fiction as I'm sure I pointed out previously and now, although Shakey was so limited particularly in its mechanicals, which were primitive even then because none of us were very good at that, and of course nowadays the mechanicals really look primitive, and in spite of the very confined computing environment that we talked about last time, it was possible to do this. And that I think gave a lot of other people the idea that you know what; we can do that better. We can develop better mechanicals, controls and that-- really the field of robotics for decades moved over into the-- really the control-theory aspects, how do you make a two-legged walker and so on. We can do much better on mechanical design and the control of that. We can get more sophisticated layered architectures for the software. We can do a lot better on computer vision of course nowadays; from the perspective of 2018 it's phenomenal what's possible. And so I think Shakey really gave a lot of very smart people all over the world the confidence that yeah, not only is this possible but we can do better. And that's how research fields get initiated and that's how progress gets made.

Brock: Yeah. I have used in the past the phrase "a faith in the future," that you see there is a development, that there's an analysis where the research community sees it and it says, "Yep. This opens up a line where there's no obvious brick wall. We can push along this line and we have enough confidence, faith from these results that we're willing to invest our career, which is a precious resource, in pushing this and that it can have meaning." It's toward a meaningful end—

Hart: Well, I love that view and I'll offer a couple of kind of complementary or supplementary versions of that. One is there is a lot of headroom. Sometimes I tell people that the reason you invest in basic research is the same reason you plant trees; it's to benefit future generations. And that's not quite the same as your phrasing but it's-- I like yours very much; it's complementary. The other thing I'd say, the no brick walls, I supervised a lot of research over many, many years, hopefully always trying to hire people smarter than me who would do better <laughs> than I would do, but I did learn a thing or two about how you kind of manage, how you head up research centers. And sometimes I would ask a researcher two things-- and this is going to circle back to your view-- two questions. The first question is, "What's your dream? If you don't have a dream I'll give you some time to formulate one. But if you don't ever have a dream I sort of wonder whether you should be pursuing some other chosen profession, because research is about trying to make dreams come true and so we should talk about this." My second question is, "So what are you going to do next Monday?" and the thought was that if you have a dream but you have no idea how to get started I'm pretty sure it'll never be more than a dream." And I sometimes told the story how when I was a kid standing on the shore of a lake I used to dream that I could figure out how to make things weightless and invisible. A great dream, but when I got a little older I realized I didn't have a clue

as to what to do next Monday on that so I relinquished that dream. So back to the Shakey context: I would say that Shakey showed that there could be a dream and that's your comment that there's a future-- there's a faith in the future and you know what to do next Monday because here's Shakey already; your next Monday's job is to make it better. So there's no brick wall, you know how to get started because there's a platform of sorts so that you know at least this much is possible--it's not like weightless and invisible-- you know this much is possible; do better. So I really like your phrasing; I'll just offer these as a little different perspective on the same concept.

Brock: Yeah, and even if you're going to wildly divert, if it turns out using wheels, forget that later on, you have that. You know what to do on Monday to see if wheels are going to pan out. Do you think that the power of example, of having this integrated example of a robot that could accomplish these performances, do you think that was in Charlie Rosen's mind as why this integrative platform would be-- why he came up with that idea of an integrating platform or do you think that it was-- well, what do you think? Was that in his mind that if successful you would have this powerful example or--

Hart: Well, I wish Charlie were here to ask him that; I don't think we really can know that. I knew Charlie extremely well, worked with him daily for 15 years <laughs> and as I pointed out he was really my most important mentor, but I don't know the answer to that question. I can tell you what I think I said previously, which was the idea that-- the idea behind what became Shakey, was that it was a means for integrating all these previously separate sub disciplines as artificial intelligence was then constituted and I think I probably previously went through that litany of vision and so on and so forth. And what might round out the picture a little bit about Charlie is that I think I mentioned that he had a remarkably integrative intellect. He had this extremely broad base of knowledge and he could associate ideas from very disparate fields. At first blush they would have nothing to do with each other, and then he'd put them together and you'd say, "You know, maybe something's possible there, maybe from three different apparently very disparate fields." And so remembering Charlie's very associative, integrative kind of mind I think it very much appealed to him that if you pull together these very separate sub disciplines of AI: Vision, planning, learning, etcetera, in one integrated system. Things are going to happen that you can't predict and that's what makes it a very valuable research enterprise and as I previously said it took years to sell that but my goodness, was it ever worth it.

Brock: Why did the Shakey project end? Why not just continue and build a better version, a better version, a better version?

Hart: Strictly government funding. We were eager to continue it. We wanted after about five years to build a completely new generation. The concept was-- and this is going to be very interesting once you think of a decades-long story-- we wanted to build an office assistant that we called-- the "general factotum" was the working title. A factotum is something who-- somebody who does stuff for you. We didn't care what the physical embodiment was exactly, whether it was a sleek or wheeled vehicle or legged or whatever. But we wanted it to be not a reconnaissance vehicle, which was the founding premise for Shakey, but an office assistant that would do stuff for you, help you with your daily tasks, do your scheduling or your whatever it was or shuffle papers when we were a little bit more paper intensive in those days and so on. And ARPA would have none of it. We had other funding but ARPA was by far the cornerstone by-- it might have been 80 percent of everything or whatever; I don't remember-- and we were done. I can't

remember whose reign it was at ARPA then. I can't remember if it was George Heilmeyer or Colonel-- I can't remember his name now-- but in any case they were done; they would have none of it. We switched the gears a bit to talk about having an assistant that would help with maintenance tasks, for example how do you maintain a high-tech fighter aircraft and how would you help a maintenance technician and there were ideas around that, but they would have none of it and so that was the end. To come back to-- I said I would connect that with years later-- decades later there was a program at SRI and about twenty-odd research centers around the country-- you'll have to help me remember the name-- it was to make an office assistant

Brock: Yeah, or--

Hart: What was it called—

Brock: I don't know but I know exactly what you're talking about.

Hart: Yeah. So somebody watching this will remember the acronym and the name. I think that SRI was sort of a prime contractor or at least—

Brock: Or was it integrating contractor or something like that, yeah--

Hart: Or the integrating contractor, something like that, and it had a piece of the research and there was I think between 20 and 30 research centers. I think Tom Garvey at SRI might have been a principal person there. In any case, I was out of SRI for decades at that point so I'm not the correct source but my understanding is that that's what led to Siri—

Brock: I believe that is absolutely the case.

Hart: --and so Siri was the office assistant or the personal assistant except it could live in your phone; it didn't actually need a physical embodiment walking around an office and shuffling papers because what's paper nowadays, but it is interesting that we had that idea somewhere around 1971 when we saw the end of the Shakey funding program to be in 1972 and SRI of course lives then and lives today on contracts and grants.

Brock: It's also interesting to me that this is just kind of across the way as Xerox PARC gets set up. It too is focused on the office of the future.

Hart: Of course, and some of-- yeah, and some people who had worked in various capacities both in the AI center but more prominently with Doug Engelbart's group right down the hall. He was down the hall from me; Doug and I and our wives used to go backpacking together and so on. So that's what really seeded an important piece of the PARC stuff and of course the rest as they say is history, but yeah, we-- Nils and Bert Raphael and I and Dick Duda and one or two other people perhaps really wanted this office assistant project to be the successor to Shakey but funding wasn't there and we had to move in other directions.

Brock: Do you think you came to that focus on the office of the future independently or was it kind of just clear that this was a fascinating area because of discussions generally about office automation and--

Hart: No. I mean I suppose all of us are influenced by our intellectual milieu in ways that might be hard to tease apart so maybe some stuff from PARC-- Xerox PARC then seeped in-- who can remember the exact conversations at this remove-- but the office was not our focus. The focus was the individual so maybe that's a distinction without a difference, but we were focused on how do you help the individual; I want a personal factotum. We had secretaries in those days, but it was one or two secretaries for thirty or forty or <laughs> many, many people so you had a very small time share or time slice or bandwidth and so it was like I want more of that help. So if you think of Shakey as a physical embodiment rolling around the smooth linoleum or tile floors why can't that be somebody for me so it was really very much-- the concept, the proposals we wrote, were very much along the lines of a personal assistant; I want somebody to help me. And I guess if everybody has one, that's office automation in some sense, but it wasn't what do we do for the office; it's like how do you help me.

Brock: Right. I see the difference. Maybe one other question if I may on Shakey and it's all of these performances that it was able to exhibit. Did you then have I guess it's almost about thinking of Shakey as a science project, about learning something about human intelligence or the nature of intelligence no matter where it is embodied or did you see it as more of an engineering technology project to create a system that can perform these designed functions?

Hart: That's a great question. I think probably all of the above in all of the principal researchers at one time or another, in different combinations. So for example I think I may have mentioned already that we were very interested in everything from the neurophysiology of the occipital cortex because when we were doing computer vision, like how did mammals do it, how did frogs do it, if we only knew the wiring diagram wouldn't that help us build better algorithms, so there was some of that. I think I might have already mentioned that Nils Nilsson and I made a pretty serious study of what then was called brain science, which is a combination of neuroanatomy and neurophysiology, even cognitive psychology and so forth, if we could figure out how does it work in nature wouldn't that give us some hints about how to build computer programs. But the flip side also, artificial intelligence and really pretty much all of engineering, computer science including our synthetic disciplines, we build stuff. They're not scientific disciplines in the sense of particle physicists who were trying to take apart the universe and see the smallest things they're composed of. So we ultimately were interested in building things but, to go to your question and the flip side of that, once you have models that do stuff they at least provide some intellectual ingredients or grist for the intellectual mill to create models of how stuff might work in nature. And so the timeworn example is birds and birds don't flap-- I mean planes don't flap their wings, that was tried 2000 years ago, it didn't work, but in trying to build planes we did learn a lot about aerodynamics, which is a scientific side of things, fluid dynamics, compressible fluids and so on and so forth. And I'm by no means an expert but I would imagine that anybody who wants to understand how do birds, natural organisms, fly, probably would be well advised to learn something about aerodynamics to try to figure that out; it provides a model or at least some of the ingredients of a model for the natural phenomenon you're trying to understand. And so in the same vein if we had synthetic models that worked to some extent that might suggest some model. If you go back through history people always used the most complex artifact

to explain the brain. Three hundred, four hundred years ago mechanical clockworks were models for the mind, the most complicated artifact people had built perhaps. And then when computers-- when IBM first built computers-- they were giant minds; they were human brains; well, far from it but that's been the long tradition: That you used the synthetic models as at least suggestions for how you model a real-world phenomenon and create scientific theories.

Brock: Right. So in that way it's interesting, I mean listening to you describe it that way, this kind of model building as a way for reasoning about how nature works. Is it kind of ordinary science if you will? And perhaps what's interesting is where-- but somehow when we're talking about artificial intelligence, the discussion, my impression is that it's somehow more fraught and maybe it's because we're applying it to ourselves rather than a bird where we don't think that--

Hart: Not too threatening.

Brock: Right. It's not threatening to think a model may inform our understanding of how a bird flies.

Hart: Yeah. I think you might very well be right, David. I don't know that I can add any perspective to that but I'll offer you one example that to me is deeply ironic if we can fast-forward to today's or yesterday's headline, AI news. So I- I've already mentioned that we were very interested in learning about the anatomy and physiology of natural neural systems because if we only knew the wiring system, the thought went, wouldn't that give us hints. And I think I referred to the Hubel and Wiesel Nobel Prize-winning work that goes back decades and decades now about recordings in the occipital cortex to show that certain organisms have edge detectors and movement detectors and so forth so-- and early vision systems used them. And then sometime later I think it was Brenner in the UK who got a Nobel Prize for C. elegans, which is this little, tiny transparent worm, and I may again be remembering history very poorly but as I recall there's this thing called a Planaria, C. elegans, a little, tiny flatworm or something that's sort of translucent or transparent and I think he mapped a complete-- what they called the neurome-- the complete neural anatomy, complete nervous system, which might be under a thousand neurons and that was a big deal; a worm can't do much but at least you knew whatever it did. So now fast-forward to today and deep learning of course is the current super-impressive, triumphant piece of artificial intelligence and one of the two or three presenting great application areas is computer vision for deep learning . So the deep-learning systems do phenomenally well, far beyond anything I could have imagined we'd be able to do ten years ago or twenty years ago, far beyond; it's just astonishing. In fact, I just heard a talk just a few days ago by one of the founders of Siri pointing out that this progress greatly exceeded his expectations too. So now let's think about this for a minute. So now we have these deep-learning systems with terrific performance whether it's-- I don't know-- whether it's for autonomous vehicles or whatever the exact application, fabulous real-world performance. They're massive; I don't know how many tens of millions of computing elements they have. You may call them neurons. They're little computing elements. They're usually taken an inner product and then have either some sort of saturation function, a hard or a sigmoid or something like that; they are connected; they have weights and so on and so forth. If you ask, "What's the design of that system?" we can tell you exactly the physical connections-- well, it's in virtual space because it's software but we know exactly what the connectivity of that neural net is, that deep neural net; maybe it's eight or ten or fifteen levels deep. We know exactly what's connected to what. We know exactly what the weights are on every connection. We know exactly how it was trained; maybe it was

partially supervised and partially unsupervised. We know from an engineering perspective everything we could possibly want to know about that and if I asked you-- said, “How does it work? Why does it work so well?” Not much of a theory. So it’s as though if I go back to the Hubel and Wiesel we’re-- and we’re talking about 30, 40 more years ago-- or the C. elegans it’s as though well, we finally got our wish. We have the complete connectivity of the neural system. We know even more than the connectivity; we know the weights and the computation and we still don’t have really a firm, clear understanding of why it works so well. Again I’m not a specialist in that area anymore, my research career is kind of receding in the rear-view mirror, but my impression is that we really don’t have much of a theory as to why these are fabulously effective from a performance standpoint, why do they work so well; what exactly are they computing and why is it so good.

Brock: Yeah, that is interesting. You have total knowledge of the whole current and past states of that system.

Hart: You know everything and you know what’s being-- and partly-- if you go back-- this is maybe a bit of a philosophical digression you may not want to entertain-- so there’s a long, well-established practice, philosophy, concept, that says the way we understand complexity is by divide and conquer. If you want to build any complicated system the first thing you have to decide are what are the cleavage planes, what are the subsystems of that, whether it’s software or hardware-- let’s say software because that’s intangible, harder to understand in some sense that physical stuff-- what are the components, how do you divide the components in ways that there’s strong interactions within, but only minimal interactions between, well-defined minimal interactions between the components. How do you “hierarchicalize” and how do you put them in layers. Who needs in the Computer History Museum to talk about layered software designs or objects or anything of that nature? They’re all means of controlling complexity by breaking a very complicated solution, an engineered solution, the science of the artificial, into smaller and smaller pieces that can be understood, managed, created, debugged and so on and so forth. And by the way, integration test is the hardest part of debugging because it’s when you bring it all together; at least that’s what it used to be. <laughs> All that practice, decades if not centuries of engineering practice of divide and conquer, make layers, separate things out, put things into smaller and smaller packages is violated by deep-learning networks. There is not much of that and so we have one giant thing, hard to understand. Now I’m exaggerating a little bit because if you have receptive fields in computer vision applications you can look at receptive fields, you can say they’re this collection of computing elements, neurons or whatever you want to call them, so to some extent I’m overstating. I’m exaggerating a bit, but it’s not like a system that was designed top down where we did all this separation and all this layering so we could manage and understand complexity. So again I’m just kind of an armchair philosopher at this point but that’s kind of my intuition about why it’s hard to understand this stuff.

Brock: I think if my research serves me well it was around 1976 that you and Richard Duda and Nils Nilsson began to explore using Bayesian statistical methods with inference systems for rule-based expert systems and this to me seemed like a very significant development both for you and for expert systems. So could you maybe describe how you started to get into that area?

Hart: Sure, happy to. So there’s a lot to the story.

Brock: Please.

Hart: Okay. So I probably started a bit earlier than that; it might have been-- the publications might have started in-- I haven't looked it up but '75 or whenever it was, but how we got into that-- so we just were talking about the end of Shakey. And Samuel Johnson or somebody or other supposedly said something to the effect that, "Nothing concentrates the mind like the immediate prospect of hanging." And for a researcher, nothing concentrates the mind on writing proposals than the imminent end of a very large and long continuing research project and the end of funding. So, as I pointed out a few minutes ago, it was clear that ARPA was not continuing with anything like a Shakey follow on. So, it was like, "What do we do now?" And so, around that time, Ted Shortliffe and Bruce Buchanan at Stanford had created this MYCIN expert system, which was a rule-based expert system, the first ever rule-based expert system, in that case, for diagnosing bacterial infections. And Ted was an MD or an MD student. And he probably wound up eventually with maybe both a PhD in computer science and an MD, I guess. Wonderful guy, very, very bright and very lovely. And so, we often went over to Stanford and listened to an occasional small research seminar, not a great big presentation, just a handful of people. I was very taken with that. And so, I started looking around. And I was always an enterprising sort of person. Well, where should we apply this? And not medicine, not only were we not MDs, but it was clear the medical establishment would take a very long time to accept any kind of computer anything, not that. Well, at that time, you may remember, there was the oil crisis, the oil shock, 197--

Brock: 3, 4 was the first.

Hart: Something like that, yeah. And so, it was natural resources. And so, one thing about my sort of intellectual curiosity is that I've always been interested in what you might call the introductory course in anything and everything. I wasn't necessarily interested in the fifth such course, but I was always interested in what are the fundamentals, what are the basics, what's the stuff about, what are the open questions, what are the tools, just the introductory level. And so, that made me a pretty well-suited character to explore new areas. And so, I thought, well, natural resources. And so, the first thing I thought about was hydrocarbons. So, a gas crunch, a petroleum crunch, let's look at that. So, what I'd found, it didn't take all that much digging, was that the technology of hydrocarbon exploration, was really very advanced and very sophisticated. Schlumberger, a name unfamiliar to most people because they make no consumer products, was, and still is, the giant. And there was very sophisticated instrumentation and, by the standards of the day, quite sophisticated software analysis. And I thought, I don't think we can add much there.

Brock: Now, this is sort of the geophysical seismological signal processing stuff.

Hart: All of that, geochemical, geo-- yeah, seismic. Schlumberger puts instruments down boreholes and so forth. In any case, the whole technology of oil exploration looked to me to be very sophisticated, very well staffed, with very, very expert people. I didn't see that this new expert system concept would really find a very comfortable or productive home there. But right next door to hydrocarbon exploration was what we learned to call hard rock minerals. And hard rock minerals are things like copper, lead, zinc, uranium in those days, gold is fine, almost anything you can think of, tungsten, anything and everything

that makes the modern world-- other than aluminum, which is everywhere and just needs cheap power to refine, bauxite, and maybe iron ore, which is widespread. So, other than those, practically every mineral that is-- every metal that's used to create the modern world, is what's called a hard rock mineral. And it is true that the hard rock mineral industry is just a small fraction of the size of the hydrocarbon industry. It was plenty big enough. Moreover, my impression was that the level of technological sophistication, particularly on the computing side, was very, very limited, very basic, very elementary, certainly compared to the hydrocarbon side. It was quite the contrary. And I thought we could really make a contribution there. So, I dug in pretty deep and secured funding initially from the U.S. Geological Survey, which conveniently had its western regional office sharing a back fence with SRI, although, I did have to trek to the Reston, Virginia where the headquarters is near Dulles Airport quite a lot. And we got some National Science Foundation, a little bit of other funding. And Dick Duda and I, and to some extent Nils Nilsson and a couple of other people, started this project called Prospector. And there's really some funny things about the technology. It looks pretty simple by today's standards, but some funny little quirks with the technology thing. So, we thought-- I thought we'd do something like MYCIN but just apply it in this new world. And we'd make some improvements that I'll get to in a minute that you brought up. But the first thing was that, of course, I misunderstood exactly how MYCIN worked because, again, I liked the first course and everything. And I was moving-- I'm always moving too fast. And I go through a lot of stuff, but things fall in the cracks, too. There's a penalty. So, I didn't quite understand exactly how MYCIN worked. And in particular, I misunderstood the control structure. And I didn't appreciate that MYCIN's control structure was nothing other than a LISP push down stack. And so, instead, what I did was create an explicit network to connect these rules, which MYCIN never had. And so, once you have an explicit network, you can create whatever control regime you want to, how to explore that, what do you do next, what question do you ask next, how did it propagate, and so on and so forth. So, the first thing I did was, in misunderstanding MYCIN, I created what I think is probably a better conceptual structure because, again, an explicit network really allows you to do a lot more.

Brock: May I ask a question about that to make sure I'm following what an explicit network actually is?

Hart: Mm-hmm.

Brock: In a rule-based system, I understand the rules are most often expressed as if/then statements.

Hart: Right.

Brock: For the most part. Now, is the network then a network of these if/then statements--

Hart: Exactly, that's all it is.

Brock: Would be to say the edges of that graph are dependencies, or--?

Hart: It's-- no, you were exactly right, David. There's a node, which is a rule. And it says if this and this and this and this, and that's the input, then this is the conclusion. And you get the conclusion in that parent node. And so, you just connect those together. So, it's exactly the same. If you were doing this

from a knowledge engineering standpoint, you might be trying to elicit if/then rules, except I just wrote them down explicitly in a network because I thought that's what Ted had done. I believe he didn't, or at least not in the very first incarnation. There must have been ten or more PhD theses from that eventually, but in any case, so we had this explicit network. The other thing I did different was that I didn't really like the certainty factors that Ted used. They were serviceable, but they were just an ad hoc weighting scheme, because we all appreciate that these decision problems involve uncertain or inexact reasoning, so you cannot just use Boolean logic exclusively. You had to have some sort of weights or soft or fuzzy reasoning or something. And of course, Lotfi Zadeh at that point had gotten fuzzy logic on the map. And I knew Lotfi very well and understood it and decided no, I didn't really want to do that. But I was sort of an unreconstructed Bayesian. And in fact, my PhD dissertation, at that point not too many years previously, was on non-parametric decision theory. So, it was like a big super set of Bayesian stuff. And Dick and I decided-- Dick Duda and I decided that Bayesian inference was the way to go. And so, we created a very formal structure of Bayesian inference and used those kinds of computations explicitly mapped onto this network. And that's the way Prospector worked.

Brock: How would you describe, or could you describe Bayesian inference in the most accessible way for somebody reading this or watching this? How should we understand what Bayesian inference is?

Hart: The-- that's an interesting challenge. I've never tried to explain this really simply. But a key concept in Bayesian reasoning is that if you're trying to sort out which of a couple of hypotheses is true, in the simplest case it's yes/no, a/b, binary decisions, it's important to take into consideration the prior likelihood of each of those two situations, prior meaning before you've gotten any evidence about this particular case. And a good friend of mine teaches-- is a Stanford professor in the medical school who teaches diagnosis to medical students. And he tells me on our bike rides, do I know about Bayesian reasoning, the importance of priors? And I say, "Well, as a matter of fact, I do." And he says, "Well, that's what I teach the medical students." So, I'm also a birder and a bird photographer. And the joke among birders, is if you hear hoofbeats just over the hill, and you're wondering is it horses or unicorns, I can't really see anything, go with the horses. In other words, if you're unsure which species a bird is, go with the more likely one.

Brock: Yeah, okay.

Hart: So, the idea is that, as an example, is it a common cold, or is it an obscure tropical disease, those priors are very important. And so, it's not that they cannot be overcome by sufficiently strong evidence, but you cannot treat the world as having all hypotheses or all possible classifications as equally likely if they're not. So, Bayesian reasoning is a formal theoretical structure, quantitative. It's very simple formulas, very simple elementary formula manipulation for representing the fact that different outcomes or different classifications like disease have different initial or prior probabilities. And you have to include that when you compute or personally estimate what's called the posterior probabilities, which means after you've observed the evidence for this case. Now, we're down to specifics. We're not talking about all possible people and all possible disease. We're talking about you and your case and your lab results, diagnostic findings, and so on and so forth. So, you have to be able to combine those two. Otherwise, you're really missing the boat, and you'll misdiagnose a lot of things. So, Bayesian reasoning is a way of

doing that. Bayesian inference is a way of doing that. And a good friend of mine at the time, a professional friend of mine, was Judea Pearl, who was a professor at UCLA, computer science. And we see each other at conferences all the time and sing folksongs together and whatnot after hours. And Judea really picked up on that, much to his credit, and wrote a textbook about Bayesian reasoning. And Dick Duda and I were just interested in make Prospector work. And that really became a very important theme in artificial intelligence. Judea, as you of course know, became a Computer History Museum Fellow on the strength of that. I presume that was the main contribution. And he really--

Brock: Focused on--

Hart: Told me in so many words that he had picked it up because he saw that the stuff that Dick Duda and I had done was really-- can be taken much further than Dick and I were interested in doing. Judea was a professor at UCLA. Dick and I were building systems.

Brock: And he-- it is my understanding that he also, as he got into the Bayesian inference world, that he was also exploring it in the context of rule-based expert systems.

Hart: I suppose. I don't really remember the history well enough to speak to that. But I do think it's the case that for quite a few years, maybe still today, Bayesian inference has been a pretty important thread in what's nowadays called GOFAI, good old-fashioned AI, to distinguish it from neural nets, which don't have anything at all really-- or not much to do with that. But to get back to Prospector because that was a system that we eventually built, I'll mention two or three things that might be of interest. When Dick Duda and I gave our very first public presentation on that, it was in a-- if memory serves, it was in a kind of specialized systems conference in Hawaii in 1974 or something, whatever it was. You will have to look it up. And one of the attendees was Doug Lenat, who-- Doug is a wonderful person who, for the last thirty years or more, had been working on a system called Cyc, but at that point was maybe still even a graduate student or not much beyond. Very, very lovely guy and very, very smart, incisive, insightful kind of person. And he wrote-- did a write up in some little maybe AI little newsletter or something. He did a little write up review of this conference. And I still remember, even though it was forty or more years ago, he said-- he mentioned Prospector. And he said, "This is the one to watch." And I happened to see Doug here at the Computer History Museum a couple of months ago or so at an event here. And we were chatting, glad to see each other, hadn't caught up for quite a while. And the event was about expert systems. So, this topic came up. And I said, "Doug, you know I still remember you wrote this. We loved you. Dick Duda and I loved you. I remember reading this. And I remember running into Dick's office. And I said, 'Dick, look what Doug wrote about us.'" So, we got a kick out of that. And of course, Doug remembered it. So, Prospector really was pretty important. I already mentioned that one of the unique contributions of Prospector at the time was the explicit inference network as we called it, that there was this explicit connection of all the rules and that that allowed you to have different control regimes for what-- how do you use these things, how do you compute, what do you compute next. And that led to a very important initially totally unpredicted advance, which was that we had a wonderful, initially, he might have been a student, wonderful researcher working with us named Kurt Konolige. And Kurt later went on to great fame as a roboticist, another brilliant, brilliant guy. But at that time, he was, quote, just a programmer. He was much more than that. And he said to Dick and me, he said, "You know, we can

compile the inference nets so if you put all the data in at once at the tip nodes, as opposed to asking questions," you know with our control structure, well, you say what's the next question to ask. "Forget that, we put all the possible data in and just do a forward propagation, compute everything, and we'll get an answer." Totally unexpected initially, but because we had this explicit network structure I mentioned, unexpected benefit. That's just the beginning of the story. So, there's a-- well, we can compute stuff really fast now. Where do we get a ton of data as opposed to asking an explorationist one question at a time painfully? And the answer is geological maps. That was the next breakthrough because geologists' most fundamental tool is mapping. All geologists learn that. The specialty of geology having to do with mineral exploration has this odd name of economic geology. Economic geology is founded on maps. So, you have a map laid out. And it has the geological data, which might-- the major kinds of geological structures or faults or sedimentary layers or what have you. It has all the geophysical data if you've done any kind of aerial surveying or something like that or gravimetric or something like that. There's all the geochemical data. The joke was you take graduate students-- they call them mud scoopers. They walk up the stream scooping mud and saving it every hundred yards. And you'd do an analysis of that. So, you overlay all the geochemical, geophysical, and geological data on a map. And now, we could imagine putting a little grid on that map at some resolution, of course, laughably small by today's standards because of computation but good enough. And for each point in the grid, you have all of this data, the geology, the geochemistry, and the geophysics. And now, you can make a Prospector model. And that's the way we organized the knowledge base; we had what are called models of ore deposits, which the best economic geologists spend their careers formulating. So, we have the model for a particular kind of deposit. Let's say we're looking for a particular kind of molybdenum deposits. They classify these things, Mississippi Valley type lead zinc deposit, massive sulfide deposits, and so on. That's what the economic geologists do. So, let's say we're wondering do we have a molybdenum deposit-- we have a molybdenum model. And we're looking at an area under exploration. And we have this map that has all this data that I mentioned. And we have a compiled knowledge base, which means for every point on the map, we can forward chain. We could do a forward propagation through the knowledge base and come up with a favorability index using the Bayesian reasoning we were describing. For each point on the map, we can compute favorability score of how likely is it that there's a-- let's say in this case, molybdenum ore at that point in the map.

Brock: Wow. Okay, let me see if I'm following that. So, is it that-- so, you have a set of data for each point. And then you are able to take that set of data and sort of ask the knowledge base which of these rules--

Hart: No, we--

Brock: Speaks to this data?

Hart: We use all the rules. We use all the rules. Think of it as being a bunch of tip nodes, that's where data goes in, and a network that propagates forward or upwards, those are the rules, which are typically layered because when we build the knowledge base, we tried to think of it the way the geologist thinks of it. They think of the geological structure, the tectonic setting, the mineralization, and so forth. So, there are sections of the knowledge base. It's not like we-- the deep learning thing. We've built it. We know the subcomponents. All this data from the bottom regarding this one little map-cell gets fed forward through

our compiled network. All the Bayesian inference computations get performed. And out of the top comes a favorability rating, how likely is it that all the data that you've got for this little map cell, the map-cell, is where you could find molybdenum. And so, you do that over the entire prospect, as the geologists call it, the area that's thought, maybe there's something valuable down below. So, we have this explicit network. I'm kind of recapping. We have this explicit network, which I mentioned earlier had real value. We can very rapidly compute everything through it. We're not stopping asking question one, question two, question three. We're taking all the data from that map-cell computing everything at once, then go on to the next map cell. And we can output a favorability map. Now, mineral exploration is extraordinarily difficult. Most economic geologists never find a minable deposit in their entire career. The expression of the industry is you have to look at a thousand prospects to find one minable deposit. Each stage of exploration is ten or a hundred times more expensive than the previous one, surface walking, mapping the surface, eventually diamond drilling in some remote area--none of these things is next to a freeway. Put a diamond drill rig in the field a hundred miles from the nearest train or road or something, hugely expensive. So, you've got to go through what AI people call a progressive deepening strategy, which is kind of funny in the context of geology, and get as much information as you can at that level of expense and then decide to proceed or to cut your losses. So, here is the big result of all this. This has been a lot of explanation about how does this stuff work and so forth. It's very difficult to get actual field data from the mining industry because it's famously secretive because if your competitor thinks that this is a valuable place, they'll try to buy up the mining rights before you get there. So, it's very secretive. However, because we were able to work with really the best-known exploration geologists or economic geologists in North America, and also in some other parts of the world like Australia and Canada, I would say the best economic geologists in the English-speaking world. We had Americans, Canadians, and Australians on our expert contributor team. We were astonishingly able to get data from a molybdenum deposit in I think it was Idaho. And one of my key collaborators and lifelong friend, Alan Campbell, then a graduate student, Dr. Alan Campbell, is a Canadian economic geologist whose father was also a famous economic geologist. And through Alan Campbell, we were able to get access to typically highly secret commercial data about a particular prospect in I think it was Idaho called Mount Tolman, T-O-L-M-A-N, the Mount Tolman deposit. Eventually, that deposit was exploited by a mining company. It was mined. There's a big open pit. And absolutely everything is known about the geology of that deposit because it's now been mined for a long time. But through Al Campbell, we were able to get the initial exploration data from the company before any of the actual diamond drilling was done, which in turn-- that's to bring up cores for analysis--which in turn was before an open pit mine exists. So, this is like take yourself back to the time when you were just a surface geologist getting some level of relatively inexpensive geological, geochemical perhaps, data and so forth. So, we had that dataset. We went through exactly the process I've described. And we produced a favorability map, which was just color coded. Red was hot. Dark was cold. Just what you would have done. And compared that with the known deposit as revealed by many, many years of actual open pit mining. And when you placed one over the other, the congruence was so exact you would have sworn that we dry-labbed it if you didn't know better. Down to the point that there was actually two lobes, or two parts of the ore body, a big one and a little one and a strip in between they called the barren zone, and our favorability map exactly reproduced that. You look at the red, the bright red-orange, whatever hot spots, there was a little one there and big one there. It was so-- as I say, it was almost too good to believe. It was that accurate. So, we published that as a short item in *Science* magazine. We published longer descriptions in the more narrowly AI-ish technical literature. We

published a short account of that with the maps in *Science* magazine. And I'm pretty certain that's what ignited the boom in expert systems. The magazine, the *Science* magazine article, was immediately taken up by a wide spread of media. You know how that process works because you're a professional. It usually got headlined as "Prospector Discovers a Mine" or "Artificial Intelligence Discovers a Mine." Of course, that's not strictly accurate because by the time we were did our work there was already a mine. But the spirit was correct. We used the preliminary data from long before there was an open pit. We correctly predicted exactly where the richest ore bodies would be. It was minable. That's kind of the economic definition of an ore body. You can mine it at a profit. And that news item got from the more specialized magazines to the-- to I forget *Newsweek* or *Fortune* or *Businessweek* or everywhere. We started acquiring a set of clippings like that from the more popular press. And that really is what got things launched. At that time, I was thinking maybe I should be looking beyond my career at Stanford Research Institute, or then SRI International, the name had changed by then. I was the-- had been the director of the artificial intelligence center for some years. And so, I was starting to look for what am I going to do next, perhaps someplace else. And the field of expert systems just exploded.

Brock: Two quick questions before we continue to follow that line. How did you get access to these experts in economic geology in the English-speaking world?

Hart: It was a struggle. My first expert was Professor Charles Park, who was the dean of the school of earth science at Stanford and the legendary dean of economic geology in the English-speaking world. And I did my first knowledge engineering with him, and he was helpful. But I recognized that I had somehow not structured it right because there was like almost flat, almost like checklist style. And I realized I somehow wasn't getting the right structure. So, I didn't know enough to ask him the right questions because he knew everything there was to know about that field. Then through another professor there, I met Al Campbell. It was a professor there who was really more on the hydrocarbon side, but he said, "Oh, I've got a graduate student." And Al Campbell came ambling into the room. He's like six foot five in a plaid shirt. He was born in Yellowknife in the Northwest Territories in a mining camp. He's just wonderful. He's a lifelong friend. We're still very close friends. And Al immediately jumped into this because he was interested in the-- he had a somewhat unusual thesis that connected pure economic geology with decision making mechanisms, how do you make decisions, and what's the theory behind that. And so, he was perfect. And he also-- so, we built another model with him. And that had a little better structure, a little more generalizable, a little more structure to it. And then through him, I met Marco Einaudi. And Marco also became a lifelong friend, wonderful person who's a professor of economic geology at Stanford. I think he became head of the department at one point, now I guess emeritus. And with Marco's help, Marco had been a practicing field economic geologist for mining companies before he became an academic. And with Marco-- we had step one with Charles Park, step two a better model, structurally a better model, with Al Campbell. And with Marco, we really struck pay dirt to pursue the analogy. And he figured out how to structure these knowledge base models, these ore deposit models, in a way that exactly mirrored the way the geologists think about it. There are sections. what's the tectonic setting? What's the regional setting? What's the local geology? What are the alteration and mineralization products that you could observe? And just the way a very sophisticated, intellectually deep, thoughtful geologist would structure these. And I think Marco is in the middle of writing like a definitive work on this. And so, Dick Duda and I saw it and said, "This is fabulous." Not only does it allow us to get into

tremendous depth and specialization about the questions, and the nuggets, and the modules of the knowledge base, but it's also generalizable. Marco was an expert in a particular kind of copper deposits called porphyry copper, economically very important set of copper ore deposits. Same approach can be used for anything for which the geologists think they have a model. And that just-- it almost became cookie cutter after that. So, we already had Charles Park, the most famous economic geologist probably in North America at least and Marco Einaudi, who at that point was a professor. We had Al Campbell whose father, Neil Campbell, was credited with two major discoveries. I told you most economic geologists never have one in their entire career. I mean this was the all-star team. So, then we started getting some of the best economic geologists in Canada. We got the most famous economic geologist in Australia who had done work in diamond deposits in South Africa in what are called spinifex, which is kind of a texture of rock stuff. Dick Duda and I put on a fabulous show. After we got our first real models working, we set up something at SRI. And I can't tell you how difficult it was to get twenty displays hooked together to show the same thing from a time-shared computer in 1975 or whenever it was. This was-- just setting these demos up were super challenging just getting stuff wired. And we invited the leading English-speaking exploration geologists. And must have had twenty-five or thirty people there. They typically had titles like VP of exploration for Anaconda or something like that. And we showed them our stuff. And I mean it was jaw-dropping. It was like, "Can computers do that?" It was fabulous. So, then we set up a little bit extra funding program they could contribute to and that sort of thing. So, it was really a very big deal. And you have to remember this was all in the second half of the '70s. There wasn't much else around.

Brock: Right.

Hart: So, we were very, very early.

Brock: So, I was just making the point that with all this interest in your wonderful demo, it would seem to me that there was the possibility for doing a large project to make Prospector version two or--

Hart: Right.

Brock: You know, extend it at SRI International. But if I'm-- and all this public attention. But is it also-- but is this the same time period when the whole Machine Intelligence Corporation story begins?

Hart: No.

Brock: Okay.

Hart: That's different-- well a bit overlapping. So, I can sort of sort that out.

Brock: Thank you.

Hart: So, Machine Intelligence Corporation was a Charlie Rosen brainstorm. And I don't remember the exact founding date. It would have been '76, or were you able to look that up, or do I look it up?

Brock: I have ’78 written down--

Hart: Maybe ’78.

Brock: But I could be wrong.

Hart: Well, I don’t recall, but it was well before 1980. I left SRI at 1980. And Machine Intelligence Corp was started with the breathtakingly naïve business plan that it would be an AI company whose mission it was to start more AI companies. So, you think about focus, focus, focus, and it seemed that it was just unbelievable that we thought to do that. But the idea is we do a vision company, an expert system company. Gary Hendrix, another wonderful, very smart guy was doing natural language interfaces to databases, natural language query to the databases. So, we would start this Machine Intelligence Corp. And Machine Intelligence Corp would start all these separate companies. And so, Charlie became the first employee. And Earl Sacerdoti, who was the next generation of brilliant young researcher, became maybe the second or third employee. I was a co-founder. I think Dick Duda was probably a co-founder, if I recall. I definitely was. And we elected never to become employees.

Brock: I see.

Hart: So, we never went there. And then Machine Intelligence Corp spun off to its own fate and another long story. But by the late 1970s, Prospector was mainly funded by U.S. Geological Survey. And their motivation was that they have a mission, at least they did at the time, to assess what’s called the mineral endowment of public lands. And the concept is that you need to know, if you’re the government, you need to know something about what the likely mineral resources are when you make decisions about, for example, setting vast tracks of land aside as no-go areas by whatever of many several mechanisms obviously, national monuments, or so on and so forth, or maybe what to charge for leases, or what have you. And it’s a very narrow line they walk in a sense because they’re supposed to get this broad scale endowment figure, I mean like all of Alaska. But they’re not supposed to actually find the mine because that’s the mining company’s job. So, they want the best information they can get, but not too localized or too precise because then the government’s not supposed to know “Drill here.” So, they had this kind of broad scale thing. But that was never really my concern. I was just dimly aware of that as kind of a backdrop.

Brock: Right.

Hart: So, long after I left Prospector, and actually after I left SRI, I think there was some continuing work on this mineral endowment kind of thing or the kind of larger scale. So, I lost visibility. But to your question about couldn’t we get research funding, at that point, all of our attention was on the private sector. This was the expert system hothouse boom. AI was going to finally do something. Expert systems was the vehicle for that. What do you want to do? And so, I thought about starting a company to pursue something or other. But I really thought that-- I wasn’t interested in the tool business. At that point, Ed Feigenbaum and Bruce Buchanan and others at Stanford had dozens of graduate students who had

worked on extensions of MYCIN in one sort or another. And they had formed I guess it was initially Teknowledge.

Brock: Right.

Hart: And that was the first of quite a large number of so-called expert system tool companies. I don't think many of them did very well eventually by making what originally called an E-MYCIN, the idea being an empty MYCIN, just the software framework but not the rules for medicine. So, go apply it to something else.

Brock: Right.

Hart: So, I chose not to do that. And instead, through sort of a series of interactions, I got connected with Schlumberger. And that's the beginning of a new chapter if you want to go there now.

Brock: That would be wonderful.

Hart: So, Schlumberger, I mentioned briefly before, is not widely known to the general public because it makes no consumer products or services. But I guess it still is, though I haven't followed it, at the time, it was fabulously profitable. It was one of the five or six biggest market cap companies on the New York Stock Exchange even though the revenue was not nearly the same level as the other gigantic cap companies like the oil companies. The reason Schlumberger's market capitalization was so enormous is because it was fabulously profitable. And it was run at the time by a business legend name Jean Riboud. And to call Jean Riboud the Louis XIV of Schlumberger is not to give him enough credit or to say enough. He was brilliant. And he had orchestrated this growth of a company over several decades from a family business run by two brothers, the Schlumberger brothers that dated back to the late 1920s I think, to this fabulously successful worldwide thing. And in recent years, they had moved away from the oil field services, which was the birth and profit driver of the company, to various kinds of electronics and computing companies. They had a separate half of their business or side of their business. And around that time, Jean Riboud proclaimed that AI was the new oil. And he went on record with that. By the way, if you want to learn about Riboud, there was a two or three part personality profile in the *New Yorker* about him back at that time in the late '70s [May 29, 1983 issue] or something like that. You can look that up. I think [Ken] Auletta was the author, something. Anyway, so he said AI was the new oil. And I had approached Schlumberger somehow or other. They-- I guess they put on a huge extravaganza, AI meeting, and recruited AI big names, so to speak, to come. They had a big tent in their gorgeous laboratory in Richfield, Connecticut. It's sort of like a PARC but fancier with a French chef.

Brock: Were they-- now, it's a French company, but they had a headquarters in-- but they're on the New York Stock Exchange?

Hart: It's a unique company.

Brock: Can you break it down for me?

Hart: Again, I haven't followed it for a long time.

Brock: At this time.

Hart: But back in the day, it was a French-American company. They had dual world headquarters in Paris and in Park Avenue in Manhattan at that time. They were a phenomenally decentralized company. Their headquarters staff was probably a few hundred combined. Compare that with any other company of that scale, maybe there's only a couple hundred total. They regarded themselves as the world's only international company, as opposed to a multinational company. They said multinationals are domestic companies with foreign operations. They were international. And so, for example, the person at the time who ran I think the Mideast was Scots or something like that. And the person who ran the Western hemisphere was German. I mean something or other like that. The language of the company was English, the business language. All the senior executives probably lived in ten different countries by the time they were forty or something like that. It was a phenomenal company, just phenomenal company. And so, they put on this extravaganza as I recall with literally circus tent scale stuff on the grounds of their just gorgeous research facility in Richfield, Connecticut. As I said, it was sort of like Xerox PARC but nicer. Maybe not quite as big but fancier. And there was two days or something, paid us something or other to go there, and two days of discussions about AI and where's it going. So, I tried to, after that, to convince Schlumberger to fund me in some new venture. I didn't even know what the hell it could it be. But they were better negotiators. And they persuaded me to start the world's first artificial intelligence center in a commercial corporation. Which I did. So, the guy I was dealing with was a phenomenal guy named Tom Roberts who later became both famous and infamous in the Valley. But Tom was like a thirty-eight-year-old CFO of this phenomenal company who was actually a West Pointer who had come up with blood and guts secret missions in the Mideast and had survived all sorts of firefights. And Tom was a very remarkable guy. But I don't know what became of him. In any case, he definitely out negotiated me. And he said, "Well--" He said, "Peter, you have the reputation of being probably the only sane person in artificial intelligence." I don't know where that came from. But I remember the comment. So, he said, "Write me a memo about three locations in the country where we might set up an artificial intelligence center." I said, "I'd be happy to, Tom, but the only place I'm going to do it is right around here. So, if you want me to do it, it'll be around here. But I'd be happy to write you the memo." So, I set up an artificial intelligence center in the original Fairchild building on Miranda Avenue in Palo Alto, which is kind of a frontage road to Foothill Expressway and adjacent to Gunn High.

Brock: 4001 Miranda, Avenue?

Hart: Yeah, I think that's it. Yeah.

Brock: That's Gordon Moore's old R and D lab?

Hart: Yeah. Yeah.

Brock: Was it-- the building still there?

Hart: The building was there.

Brock: So, it was in that building?

Hart: It was in that building.

Brock: Whoa.

Hart: It was an R and D lab. But of course, it was a semiconductor R and D lab. And here I was a fish out of water. But we recruited some of the best people in-- I thought some of the best people in the country. Three or four colleagues from SRI came over with me, including Dick Duda, Marty Tenenbaum, and Harry Barrow, another wonderful researcher who eventually returned to England, his home country. And recruited wonderful people like Dick Lyon, for example, I mean just fabulous staff. And there we were creating the new oil.

Brock: A couple questions about that, at this time, Schlumberger had bought Fairchild's semiconductor operation from--

Hart: The company. They had bought the company. And Tom Roberts was--

Brock: Bought the whole Fairchild--

Hart: The whole thing.

Brock: Camera and Instrument, including the electronic--

Hart: No, Fairchild Semiconductor, which was a different company.

Brock: Yeah, okay.

Hart: It had bought Fairchild Semiconductor. Tom Roberts had engineered the purchase. He came out to run it. Schlumberger has done numerous acquisitions. And the way they do it is they bring only three people, a CEO, a CFO, and can you guess the third person? The head of HR. And with those three, they can control the company.

Brock: And keep everybody else?

Hart: Keep everybody else.

Brock: So, was that-- so, Schlumberger had already, through that acquisition, had that building where you built your--

Hart: No, the timing was very funny. I was chatting with Tom about this and going through this negotiation. And then I pick up the San Jose Mercury one day. And it says Schlumberger is acquiring Fairchild. And the next thing I find out, Tom Roberts is coming out to run it. It's like-- it was like just--

Brock: Independent strands.

Hart: Yeah, well Tom no doubt knew what was happening. But I didn't. So, in any case, we did the deal. And Tom became a little bit infamous because he made comments to the senior-- the executive staff at-- top management staff at Fairchild that gained currency and weren't highly flattering. So, he became a little bit--

Brock: Critical of the previous management.

Hart: Well, there was a famous quote. He said something like-- now, I should be careful because this is going to be on your camera. Maybe I better not give-- maybe--

Brock: Okay, but it offended people?

Hart: It offended people a bit. But Schlumberger poured unlimited money. And they created a new fab up in Washington state in some location there. And but after about two and half years or so, I had understood a lot about what Schlumberger was about and how it worked. And I decided this is not my future. So, I mean a fabulous company in many respects. Their HR practices, I still remember their HR practices. One performance evaluation form for pretty much every professional in the company around the world on one page, if I recall. And I remember one of the questions, it was great. It says-- well, two questions, one question was, "Can this person be promoted two levels at once?" In other words, it's the fast track potential person. And another question was, "To what extent is this person respected?" And then it said on the form, "Not liked, respected," interesting insights. I probably am revealing a trade secret that I swore not to, but they can do what they want. In any case, I concluded that my bliss did not lie there, and I should seek excellence elsewhere. And at that time, Dick Duda and I decided we were starting a new company. And a good long-term friend of mine, Steve Weyl, spelled W-E-Y-L, Steve Weyl, who I have known since he was sixteen and a half years old when he had already finished a year of college and was working for me as kind of an intern, Steve was a prodigy, brilliant. And at that point, he had gotten a Stanford master's in computer science and a Harvard MBA. And we decided we're starting this new company. And then in the process, we learned another guy named Sheldon Breiner was going to-- was thinking about starting a company in the same area. He was kind of a local entrepreneur. And so, we combined forces, and we started Syntelligence. And I said goodbye to Schlumberger. I thanked them, thought they-- it was a great experience. I loved it. But I was going to be off to doing this new stuff.

Brock: Before we dig into Syntelligence, I wanted to ask you about Schlumberger's ambitions for your operation. Was it the case that-- was it a blend of two things? One, AI is the new oil so drill for new AI, and we'll sell it. But was it also artificial intelligence in service of Schlumberger's other existing businesses in electronics, in computing, in prospecting in oil stuff?

Hart: No, Schlumberger I think had a brilliant top management. And they did-- they approached this exactly the way I would have if I had been smart enough and thought about it and were in their position. It was a little bit of a take off of the famous Supreme Court comment about pornography. Do you remember that famous one?

Brock: I'll know it when I see it.

Hart: I'll know it when I see it. And the way it was put to me in so many words, Riboud had a wonderful kind of a right-hand technical man named Jean Baboud.And Baboud was at the vice president level. And he kind of circled the world talking to people like me within the Schlumberger world and figuring out how to make connections. And the message was really clear. "Peter, we'll give you two or three years, and then we'll see if anybody cares, anybody in Schlumberger cares". So, it's up to me, and they supported us very well. And people like Jean Baboud would make any kind of connections that we needed because nobody in Fairchild, other than three people, knew anything about Schlumberger. Nobody around my managerial environment could help with the possible exception of those three individuals. Lots of support, do something that makes anybody in Schlumberger care about what you're doing. And what I taught people, because I've always mentored people my whole career, in fact I still mentor a lot of people nowadays, is that that's the best deal you can get as a research manager, as a research leader. If you don't like that, I recommend you seek excellence elsewhere because it's like "Don't tell me what you want. I'm going to tell you what you need". And it's up to me to figure that out. I've got to figure out what you need that I can provide. And then I've got to create the programs and the deliverables to do that. So, I think that's a terrific deal. You have tremendous autonomy, a huge amount of rope. Just don't hang yourself with it. And so, that's exactly what I did. And I thought that was terrific. I was also just high enough, so I had a lot of visibility into the top level, beginning with Riboud. I remember having dinner with him, small group dinner. And I happened to be seated by him. And I asked him, "Mr. Riboud, how do you decide how to buy a company?" because he had done so many acquisitions, big and small. I said, "Companies are so complicated. There are so many moving parts." I said, "If it were like a sofa, it would be easy." And he says to me, "Oh." He said, "You do the same thing you do for a sofa." "What's that?" He said, "Ask your wife." So, I had a lot of visibility there. And once every five years, Schlumberger did a worldwide offsite for their top executives somewhere in the world. And it happened that one of them was coming up when I was there. And I got invited. So, my wife and I went there. The women were told to stay in Paris until the end of the work session. And then we went off to Deauville where the meeting was. Tom Roberts used to say that Riboud always picked great places but always the wrong time of year. Who wants to go to Deauville in the winter, Normandy coast or something? Anyway, so that was basically the deal. Do something that somebody will care about. And I can't define it upfront. We'll know it when we see it. So, I thought that was just a brilliant strategy, brilliant marching orders, perfect charter or mission for a new research center. And I was happy to sign up. But after a few years, I decided that I can see this will be a long slog. What's the upside? What are the constraints? I'll have to move-- everybody in Schlumberger moves all the time. I'm not going to move. And so, Dick and I decided to start a new company, which we did.

Brock: So, it was less about support or culture, and more just for you, your interests and seeing other opportunity.

Hart: Yeah. We were very well supported. It was terrific. But I, at that point, I would have been about-- what would I have been by then? I would have been early forties, forty-one, forty-two. And the question was did I really want to have an extended stay of my career there. What Charlie used to teach us was-- this is good advice that I've repeated-- is that people and plants both benefit by being repotted every five years or so. And it doesn't have to be exactly five. It could be three. It could be seven. But think in those terms. Think about your career in five year chunks. And when I think about my long career, that's pretty much the way it worked out. You don't have to necessarily change your employer, but you have to change your mission, your work situation, your job responsibilities. So, for example, at SRI, I was in really three different kinds of responsibilities in fifteen years. It worked out just about like that. So, anyway, that-- I had internalized that by then. And I thought do I want this to be my five year stint. And I thought well, yeah, but no more than that. It's coming up on three. And again, there was so much ferment about commercial applications of AI. So, I decided yeah, this-- I want to go do that.

Brock: So, the discussions that lead to the formation of Syntelligence begin when?

Hart: No later than '82.

Brock: Which is really interesting because Syntelligence, from the beginning, is going to be aimed toward finance, correct?

Hart: Sort of, yes, with two caveats or two additional comments. First, we were determined that it would not be an expert system tool company or programming shell company, as I discussed before, which set it apart from every single other expert system company that we were aware of. And there were at least two different newsletters about expert systems then. In fact, one of the newsletter writers was here at your event a couple months ago.

Brock: We collected his newsletters for the collection.

Hart: It was Paul Harmon, was that it?

Brock: Yes.

Hart: I have a-- I think that's-- yeah. So, we were not going to become a tool company. And the second is finance is a big world. And so, how do we figure out which piece we should go after? So, Steven Weyl, as I mentioned, Steve and I really-- Sheldon Breiner was the money raiser. But Steve and I, I think, were really the operational heart and soul of that company at the outset. And Dick Duda was the technical genius. I mean I still remembered a thing or two technically. I hadn't totally lost my chops. But Dick was still strictly technical. So, Steve Weyl and I started going around to all the various different financial institutions, some in San Francisco, but mostly in New York. We visited stock brokers. We visited fund managers. We visited commodity traders, foreign exchange traders. We visited somebody in San Francisco whose business was to evaluate money managers for pension funds. If you were an institutional investor, like a pension fund, a hundred people are pitching you. How do you evaluate them? This guy was really at the metalevel. His job was to evaluate other people whose job it was to invest

money. So, we really looked very widely. And some people were fabulously successful. We visited somebody in a Park Avenue duplex who literally had a maid, a woman in a French maid's costume, answer the door when you went up their private elevator to the penthouse, literally in a--

Brock: Costume, yeah.

Hart: So, here's one thing that Steve and I learned. Everybody in that financial sector has an incredibly compelling story to tell you about why the stuff that they're doing and the way they're doing it is brilliant, effective, and is bound to bring unbounded returns, to quote Gilbert and Sullivan. And Steve and I quickly figured out that the reason is that anybody who does not have such a compelling story quickly gets weeded out. But it doesn't mean they have reproducible expertise. This is expert systems, right? So, we went through a lot of stuff, a ton of stuff. I remember going to the senior vice president of underwriting at Allstate, just outside of Chicago, and him saying to me, "You know, Peter," he said, "Once you've seen forty million drivers, you've pretty much seen them all." So, again, my predilection for liking the first course in practically every subject really stood me in very good stead because I could pretty quickly go through the first course, the first learning experience, the first few learning experiences in all of these subspecialties in the huge financial sector. And what I came up with, and Steve supported, Steve seconded it, was two closely related areas. They were not directly related financially, but technically, we thought virtually identical, and that proved to be the case. They were both underwriting tasks. Underwriting is a financial term that means risk assessment. And it comes, I think historically, from the idea that to underwrite means to sign your name below.

Brock: Oh, right.

Hart: Here's the contract.

Brock: Literally underwrite.

Hart: Literally, I am signing my name to the bottom. I think that's the origin, perhaps. But in any case, it's risk assessment. And we concluded that, both in the insurance sector and in the bank lending sector, you both are underwriting financial risk. You're assessing financial risk. And in both of those-- we could have done either, but we wound up doing both because they seemed so similar. In both, we decided that the very high end of the market was not where the sweet spot was because, for example, if IBM gets insurance of any kind, it's not remotely a risk assessment. It's a financial engineering transaction. IBM will pay its losses. The insurance company will not. They will self-insure because they're so huge. But the management of that and how they finance that, that's complicated. At least that was my understanding. It's not risk assessment. At the bottom of the market, it's you know once you've seen forty million drivers, you've seen them all. It's rate and weight. They may have half a dozen factors or a dozen factors. It's a purely statistical thing. They've been doing it for decades. It's satisfactory. There's no upside. There's no headroom. But in the vast what are called the middle market, that's a different story. It's not the high end. It's not the consumer level, the retail level. So, for example, it might be how this museum gets insured. It might be a strip mall. It might be a bowling alley. It might be a privately-owned company, a machinist company that-- it maybe has fifty people in it. Or it might be a middle market retailer or distributor that has

a hundred or three hundred people and a fleet of trucks or something like that. They may have revenues in the tens of millions or maybe the hundreds of millions, but not the billions.

Brock: Got it.

Hart: And the same thing is true on the lending side or the borrowing side. And so, now it's back to my Jean Riboud comment, which is why I mentioned it. How do you evaluate a company? They're complicated. There's lots of moving parts, even a relatively small company. It doesn't have to be IBM or Google.

Brock: Right.

Hart: So, there, they rely on human underwriters. They are risk assessors. It's very experience-based. There's some amount of financial analysis. And there are, of course, computational tools for doing financial analysis. But it's largely human judgment. As I say, it's largely experience-based. So, the more experienced people generally are thought to be better at it. It's not very highly paid, so there tends to be a pretty good turnover rate at the lower levels. You're starting to put together the picture that this is the sweet spot.

Brock: Right.

Hart: There's huge amounts of money at risk in the aggregate. These portfolios for a Wells Fargo or American International Group are in the billions.

Brock: Right.

Hart: That's the sweet spot. So, it took some months of digging around, and Steve somehow or other would figure out who these people are and get us in the door. And then I would do the interviewing and so forth. And after looking at all these different areas like foreign exchange commodity and so forth, we concluded this really was the sweet spot. It had scale. There was expertise. The expertise could be syndicated, could be distributed to lower-skilled professionals. It was not purely statistical. There's a half a dozen things, rate and weight. You know, you're a White male in this zip code, and you're driving record is this. This is your premium. And so, we jumped in. And we got preliminary-- we got partners. Sheldon Breiner did the fundraising. To his credit, we got something like six point something million dollars in the first round that included--

Brock: Did you work with the-- was most of that from a particular investor or venture fund? Was there--

Hart: No, no, we had several--

Brock: Several.

Hart: Yeah, we had several investors, but it was a blue ribbon premiere board. Our lead investor was Sequoia Capital, and Arthur Rock, who was still active at that time. It was a blue ribbon-- a blue chip group of investors. As I said, Sheldon-- Shelly Breiner was the first guy in the door because he was somewhat known in that community because he had had a successful startup at some scale. I was basically the due diligence person. Steve and I were the ones who actually knew what was going on. Sheldon had some knowledge but didn't have deep knowledge. And so, we raised I think it was six million bucks in the first round. And we quite quickly got partners from the best of the banks and the insurance companies. Wells Fargo was an early partner. And Wells, even then, notwithstanding this recent tarnishing, was-- surely still is a very good bank.

Brock: Right.

Hart: American International Group was considered the underwriter's underwriting company. They made a fetish of underwriting. There's really only two things an insurance company does. They underwrite risk, and they invest the proceeds-- and so, going back to their famous chief executive, it was the premiere underwriting company, again commercial level, what's called commercial lines of insurance, property and casualty, liability worker's compensation, that sort of thing. And we got to work. We really got to work with a will. There were some huge intellectual challenges and one huge legal challenge. The intellectual challenges, we thought we'd take Prospector, which I've described at length, and just apply it. And we discovered it was nowhere remotely applicable to the demands.

Brock: Wow, I was just--

Hart: Not remotely.

Brock: Going to ask.

Hart: So, we had two brilliant technical people. My dear colleague Rene Reboh, who was the principle programmer for Prospector and now sadly gone. And we had recruited to SRI another brilliant Swedish computer scientist named Tore Risch. And Tore left SRI and went with us. And they were the two key software people. And Dick Duda was the key theoretician. And we created a completely new language called Syntel, which I'm convinced today, if we had been academics and had generations of graduate students, I'm convinced it would be a fixture on the landscape. It was a functional programming language that used functions over relations. Relations you can think of as being like tables like in a relational database, with probability theory layered on top. So, for example if you said for example profit equals revenue minus cost, but we're talking about future profits, so we have only a probability distribution over future revenue and only a probability distribution over future cost, what's the probability distribution over future profits? Syntel will compute that for you in a heartbeat. So, it was a totally different knowledge representation, not remotely rule-based, or only by courtesy, a bit of a stretch. We had all these different types of functions including judgmental ones that allowed underwriters, the expert underwriters, to put in their judgment about how things-- one thing affected another. It was-- I didn't have too much to do with it, so it's not personally boasting for me to say I think it was brilliant, a tour de force. I think we had exactly

one refereed publication in maybe Communications of the ACM because man, we were busy trying to build stuff. We were not researchers writing papers and giving talks.

Brock: Right.

Hart: But anybody who is interested can look up this Syntel. I think the authors-- I think I was probably a coauthor. Dick Duda, Tore Risch, R-I-S-C-H, Rene Reboh, R-E-B-O-H. I think it was just masterful. It had so many refinements. It had an incremental compiler that would allow knowledge engineers to compile their knowledge base incrementally, so it would run fast. The knowledge engineering environment was LISP machines. But the run time environment was pure vanilla IBM CICS MVS. We had IBM terminals running as bitmap displays looking like a LISP machine. We had platoons of IBM visitors wasting our time because they couldn't believe that a CICS system looked like a bitmap display and not a character-oriented line-by-line--

Brock: Right.

Hart: Kind of thing. So, there were some brilliant people on the software engineering side. Eric Kinser was a key guy, Jonathan Seder another key guy, who built out the IBM environment brilliantly, so the stuff could run with the database management systems. It was a huge effort. It worked.

Brock: How long did that development take?

Hart: It must have taken us at least three years. We started in '83. I think our first real development-- it might have been '86 or something like that. It was a huge effort beginning with the recognition that Prospector had nothing to do with this. It didn't-- it was the proverbial hammer with the wrong nail.

Brock: Right.

Hart: So, we, being mostly not me-- I invented one subsystem. I take credit for the explanation subsystem, which was required to give confidence to why did it come up with this.

Brock: Right.

Hart: That was required to give confidence to the underwriters who were using it. I invented that little piece, good for me. I had nothing to do with the big chunks. It was all these other individuals who I mentioned. So, that was fabulous. I mentioned there was one legal challenge.

Brock: Yes.

Hart: So, we were patenting stuff. Now, picture this. Here we are, the AI software geniuses and the knowledge engineering. I was always the first guy in the door of knowledge engineering because I said that was kind of my intellectual personality. I could talk to anybody about their stuff, and I elicit the first things. Then eventually, we had a whole squad. We had, I don't know, a dozen or two dozen knowledge

engineers. So, now picture this. We are working with you, American International Group, to encode your knowledge in this carefully constructed knowledge base. And by the way, our Prospector experience that I described about how you sort of modularized, that was very helpful. We did the same thing except with finance. So, it's your knowledge and our software and our representation. And representation, as you know, means computationally--

Brock: Right.

Hart: A computational representation you can execute. Who do you think should own what? What rights do we have? What rights do you have? How do you figure that out?

Brock: Right because that representation is creative work and wouldn't have happened otherwise.

Hart: But it's your underwriting knowledge.

Brock: It's my experience as I'm talking to you. I see the problem.

Hart: It's your underwriting knowledge, which we've decanted and put in a computational form using our expensively developed knowledge engineering tools and our foundation, which is the knowledge representation used by the Syntel programming language.

Brock: Got it.

Hart: Enter another wonderful guy, a unique Silicon Valley talent you may never have heard of, called Mike Stern. Mike Stern is an attorney. He's an intellectual property attorney but not a patent prosecution guy. He does intellectual property deals. Mike Stern has a remarkable background. He was a professor of English literature with an I think Cambridge or something education in his background. Then he became a reporter for I think the *Wall Street Journal* in New York-- with the *Washington Post* or something like that, if I remember, Mike, before becoming a famous lawyer. He's brilliant. And here's the very best thing about Mike. I used to say that most of the lawyers that I dealt with in my business career, I used to joke would want to know exactly what I was trying to achieve so that they could tell me why it couldn't be done. I know that's unfair. It's a bit of a parody. But Mike was just the opposite. He would want to know exactly what you're trying to achieve. And he'd figure out how you could do it. And he figured out what the right license terms were. And I don't think that's too germane at this point. I don't remember the excruciating details. But Mike solved that problem, so we had mutually acceptable-- to us and our development partners, mutually acceptable terms about who owned what under what conditions. So, those license terms were figured out. I will tell you that Mike went on to other fabulous achievements. You've heard of Creative Commons?

Brock: Yes.

Hart: Mike-- as far as I know, Mike is the lawyer-- so, that was Larry Lessig's brainchild.

Brock: Yes.

Hart: But as far as I know, Mike was the lawyer who actually drafted the license term for all the Creative Commons licenses that we use. And by the way, I use them myself for my bird photographs. And I say, "Mike, which one do I need?" And he said, "Use this one." He also was close to a co-founder of General Magic, not quite, but an early employee. So, as soon as they knew Mike was leaving--

Brock: As their counsel?

Hart: His law firm to become-- then I knew. They had an unusual business plan. And Mike was a producer, executive producer, of the just released General Magic film, which just had a premiere in Silicon Valley a week or two ago in San Jose that I went to. So, Mike Stern, not a technical person, but another brilliant personality in this story who should not be overlooked or forgotten because he made tremendous intellectual contributions. And he continues to make them.

Brock: You have-- so, you've got all these agreements figured out about the knowledge engineering, and you have the new expert system, new design.

Hart: And it all worked.

Brock: And it works.

Hart: It all worked. So, here's what happened. So, by the late 1980s, the board of directors had decided that we needed a change of CEO at Syntelligence. And so, we got a CEO who was from the banking industry, a very high-end software sales executive named Don Steele, S-T-E-E-L-E. And he had a whole crew of people who had sold these very high-end systems to the banking industry and knew the players. And you have to appreciate that these are institutional sales at the highest level because this goes to a mission critical function of any bank or insurance company. So, not only is it a multi-million-dollar licensing fee and a huge install and so forth, but if you're a bank, all you do is lend money and invest it. And this is the lending part. And if you're an insurance company, you underwrite risk, insurance risk, and you invest the premium money you get. And all we're going to do is to totally revolutionize half of your core business. So, it's a very difficult sell, a very lengthy selling cycle. And every sale eventually gets signed off on by essentially the CEO, or the chairman of the board, or the deputy chairman of the board. It goes to the very, very top of the company. And getting there usually takes six months to a year. The banking/insurance company has huge institutional software systems that they run. So, they know these institutional sales. They've got to go up level by level by level until you get to the top. One time I went to Toronto for lunch, for example, to the-- I think one of the three biggest banks in Canada. And I went with our sales guy. And we had lunch. I just went for lunch. And we had lunch at the executive dining room at the top floor of the bank building in downtown Toronto, had lunch with the-- I think he was either the chairman or deputy chairman of this very large bank, the Canadian Imperial Bank of Commerce. And we're standing, chatting just before sitting down. The three of us are there, my sales guy, and me, and the chairman or whatever. And without batting an eye, this guy says-- he points to my sales guy, and he looks me in the eye. And he says to me, "I know he'll lie to me," he says to his face. He says, "I know he'll lie to

me. I want to know from you, can you deliver?" I looked him in the eye, and I said yes. And we signed the deal. The salesman got a huge commission check. He didn't worry at all about being insulted. You need a very thick skin for those kind of sales roles.

Brock: I would imagine.

Hart: In any case, so here's what happened. So, there was kind of a change at the top and the sales level, the direct sales level, which maybe was a little bit painful for a founder but did the trick. And so, by the late '80s, we had both major banks and insurance customers in four countries, basically all the major English-speaking countries: In the U.S., Canada, England, and Australia. We were profitable for several years, both profitable and cash positive for several years in the late 1980s. Not only did we have customers, we had satisfied customers. In the Canadian bank alone, we had a senior VP who had believed in our product and who had promoted the sale. And I went to this lunch and inked the deal. And a year or whatever later, I think he told us-- I can't remember whether he said we had either five thousand or ten thousand login names.

Brock: Right.

Hart: You know they had a provision for that and so forth. Either the five or ten thousand login names, they had a huge distributed kind of just sparsely populated company, very distributed. And he told us that at that point I had that conversation, they had evaluated something like ten or fifteen billion dollars of potential financial transactions using our system. Now, you might say it was only Canadian dollars. But it was still a lot of money. That's not the number of deals they did. That's the number of deals they evaluated.

Brock: Right.

Hart: So, we were successful. This unique technology worked. The IBM delivery environment, which I described, was very complicated, difficult, and ground-breaking with these bitmap displays on an IBM 3270 or whatever terminals. It all worked. And so, we started. We were on a January fiscal. And I remember a Christmas party in I guess it would have been December of '89. We were really optimistic. As I said, we had been profitable for, I can't remember, several years, cash positive as well as book profitable. We said, "You know, we can go for a year without selling a thing, we have so much cash in the bank." And that's what happened. In 1990, there was a recession, which, at the time, was considered the most-- before 2008, was considered the most serious recession of the post war era, as I recall. It was especially severe in the financial services sector, which was my market. And I remember reading these quotes from CEOs of banks and insurance companies saying, "We're not going make any capital investments for a year. We're just in a cash conserving mode. It's-- you know, it's acquire, or be acquired. And either way, we're not making capital investments. We're just conserving cash. That's it, period." And I thought this is going to be really bad. We could sell nothing for a year. And so ultimately, I was the only officer of the company willing to take Syntelligence through a Chapter Seven, which happened in '91.

Brock: I don't know what Chapter Seven is. I'm sorry.

Hart: Chapter Eleven, which you always hear about, is a reorganization, just put everything on hold, we'll consolidate debts, and we'll keep going and reorganize. Chapter Seven just means shut it down, pay off whoever you can with whatever assets you have; shut it down; it's the end. So that was the end of the dream but what I will say: It was not because of the AI winter, which it's sometimes been confused with, nothing to do with the AI winter, nothing to do with over-promising and under-delivering-- in fact my mantra was always the opposite, under-promise and over-deliver; things work out better that way-- nothing about not having happy customers, -- nothing about being ineffective. There was this huge recession and that was that. If not for that recession, I think Syntelligence undoubtedly would have continued to go up the curve and most likely would have been acquired by who knows whom.

Brock: A huge downdraft in the financial sector and with this-- bespoke sales for a system of this kind where your software kind of is the bank in some ways--

Brock: Why did you choose Seven rather than Eleven?

Hart: Chapter Eleven?

Brock: Yeah. Why--

Hart: Chapter Eleven you have to have some path forward. You have to have some financing, some reason to believe you can turn the company around. If you can put your creditors on hold you can rejuvenate the company but the typical venture company-- we're not public don't forget-- the typical venture company is not-- if it fails it goes out of business. So you may not have heard that Chapter Seven term, but every time you learn about a venture company closing it's probably that. I mean I'm not a bankruptcy specialist.

Brock: It wouldn't have been a practice to say--

Hart: Well, by that time we had raised several additional rounds. I think we raised a total of somewhere approaching $25 million in several rounds and I have given an occasional talk at places like Xerox PARC, this not recently, and I say, "Well, there is bad news and good news" and then I talk about succeeding and failing in AI in multiple decades. I talk about this chapter and what's the bad news? Well, the bad news is we lost $25 million. Well, with that bad news what could the good news possibly be? Well, it was somebody else's money so—

Brock: I mean, I guess this may be a naïve viewpoint, but you have a good system that works; you have a good group of people; you have demonstrated success. There's a sharp downturn in the financial industry. Of course, in retrospect, we know that of course you don't know how long the trough is going to last but from the investor standpoint why don't I just keep this on life support through the trough and then-

Hart: That's the—

Brock: --gear up.

Hart: That's the perfect question and that's why you're so good at this job; that's the perfect question. There was kind of a caboose to that train. So what happened was our largest customer and eventually pretty big investor was the largest commercial insurance company in Great Britain then called the Sun Alliance-- I think it might now be called the Royal and Sun Alliance after more mergers-- and they were both an investor and a major customer and they were-- as one of my opposite numbers there said, "Just because we're big doesn't mean we're not fleet," he said in perfect British English. And so they acquired the assets and the core team, not including the executive team, we were all out, and they kept that going as a separate company for a while. It might have been called Syntelligence Systems or something like that; I think they had an office in Mountain View somewhere and they did keep that going probably mainly to support their install because they had a very big install. I used to go visit them in Sussex; in fact, I set up an office in the south of England mainly to support them. It was in Redhill in Sussex; for a while I would go there. And that eventually ended but by then I was really not part of that, it was very clear, and so I don't really know exactly what happened or how it ended or where this—

Brock: One investor did make that decision to keep it going and who knows? Although it may be awkward for them to have customers in their same--

Hart: In their space?

Brock: --or spin it back out or something.

Hart: Yeah, or--

Brock: --that--

Hart: --some other country.

Brock: --that if that's--

Hart: Yeah. No. I think your question was very methodical; that's exactly right. And as I say I was probably close to persona non grata, maybe not literally but it was clear I was not part of that and so I really don't know exactly what happened, how long did it survive; where did the software go; where did the people go. I just don't know.

Brock: Well, what did you do then?

Hart: So amazingly enough just as I was taking Syntelligence through this shutdown, which I can tell you was not a pleasant experience and I don't recommend it as a steady form of entertainment, I got a call from Charlie Rosen. And as I started to say, the late 1980s was a time when lots of offshore technical companies, Japanese and European, were setting up satellite research centers somewhere around Silicon Valley. There was kind of a burst of activity in the late 1980s and against that backdrop just as I was going through this winding-down process I get a call from Charlie Rosen saying, "Hey, Pete, I'm working with some folks up at Sand Hill Road who need some help and I think you're the person who could help them". So that was timely since I could see that probably I would need to do something next. And so it turned out that Charlie had been consulting for a large or a-- well, a medium-large Japanese

company called Ricoh, spelled R-I-C-O-H, which in the consumer market was mostly known for cameras. But it was the world's largest fax manufacturer at a time when fax mattered and it was mainly in the office-machine business, which is copiers, printers and faxes with different technologies at different eras, and I had barely heard of it. They were sort of a Canon and Xerox competitor so well, I guess I should go meet. So what happened was that they had-- they-- Ricoh had set up a smallish research center on Sand Hill Road some time earlier. I don't know the exact early timing but not much earlier. And they had hired as we learned to say "a local person" to head it, local person-- I don't want to say "American" because who's American? It's somebody from-- who's in Silicon Valley. So a local person and that was itself very unusual because I was aware of any number of other as I say mainly-- well, Japanese and European labs that had been set up and they always sent an expat from the home country -- from the mother ship, from the headquarters or whatever, to be the top person in the local Silicon Valley satellite lab. And this was very unusual because here was a local person who had been hired to do this and it actually was somebody I was distantly acquainted with through some past professional interaction someplace or other. And they decided-- they-- Ricoh decided that it wasn't a good fit and so now they were looking for somebody to take over that position. That first leader at the time they contacted me, was already gone from the scene and they needed somebody to do what amounts to being a restart. It had only been a matter of months; a few people were there; they needed a restart. So that began a discussion. I met with some people here, visited the lab and two things happened with respect to the timing. One was that I really needed to finish winding down Syntelligence; I mean I had an obligation there even though it wasn't very pleasant and that was going to take some months. And the other was I would be happy to learn more about this place before throwing in my lot with them. And so we arranged for me to do a kind of-- a pretty intensive consulting gig for two or three months or whatever it was by which I spent close to a day a week-- at least a half a day if not a day a week there, really getting into things in quite some depth, talking to all the technical people at considerable depth about their projects, their technology, what they were trying to accomplish and how they were going about it, what the key challenges were, so on and so forth. So I really had a very, very good handle on exactly what was going on in this lab. There was quite a spread of talent because they-- it was a competitive recruiting environment, which is to say nothing different from ever around here, and there was some-- quite a spread of talent. And so in the end I agreed to go there full time and stayed full time in that position in some expansions that I can get into later. I stayed with Ricoh for the following about 15 years or a little more, something-- yeah, something along those lines, and that was just a fabulous gig; it was just wonderful. <laughs>

Brock: When you joined to do the restart what was kind of the boundaries or the mission of the operation? Which areas of technology were--

Hart: It was pretty much up to us. I maybe should tell you a little bit of backdrop. So as I said Ricoh was kind of a medium-large, not the biggest by any means, Japanese company but big enough to have lots of resources and operations in-- I don't know-- 80 countries or whatever it was. At the time, the executive vice president-- one of two or three executive vice presidents in the very top of the technology pyramid in the company was another one of these wonderful world citizens—a remarkable individual, sort of a Charlie Rosen-level spectacular individual named Dr. Morio Onoe, O-N-O-E. I still regard him as just a wonderful world citizen, brilliant technologist, had been at the I guess it was University of Tokyo I think, a piece of it there. I think not long after World War II he had come to U.S. if I recall his background as some

sort of student or intern, which was remarkable that soon after the war, He was just a remarkable individual. And he was the one who had decided two things: First we're going to have a local person run this operation and second when the first hire turned out to be not a bad person but just not a good match he was going to stay the course. It wasn't the wrong strategy; it was the hiring infelicity, yeah, and he in turn had sent a person-- another person who became a very dear friend of mine and also sadly passed away too young named Dr. Koichi Ejiri, spelled E-J-I-R-I, and Koichi-- as he insisted on everybody calling him-- Koichi was another one of these true world citizens whose outlook extended far beyond any nationality or country boundary or technology boundary. He was a brilliant technologist, a prolific inventor and just-- if there were more like him the world would be a better place. And so my first introductions were to these two absolutely remarkable world-class individuals and then I thought boy, if we have the right ground rules, the right institutional understandings about what's expected and what autonomy lies where, if that can be set up in some way that doesn't prevent success, then this could be great because I know how to fix all these other problems; I've got competence dribbling out of my fingertips on how you run something like this. The key thing is what's the relationship with the funding organization, the parent company; what are the expectations; what are the obligations; what is the level of trust. Those are the intangibles that are really hard to assess but that absolutely control success; if that's not right you're being set up to fail no matter what else is going on. So I rather quickly satisfied myself that this is really a fantastic opportunity; I've been around long enough I don't need to look at anything else. No one ever said, "Well, I've got to look at several things." What does any reasonable person do? Create some options, pick the best one, compare. I know what I'm looking at. Yeah. So that was accomplished and that really was a wonderful match and a wonderful experience for me that'll last for the end of my full-time professional employment; now I'm part time.

Brock: What was that relationship with the broader company? How did you negotiate those expectations and obligations?

Hart: I didn't have to negotiate anything. They, being the managers that had responsibility-- and I always taught people-- I have written some stuff for internal consumption about this, about how one of the most difficult positions for a technology manager in a corporation is the top technology person and the relationship with the top general manager because I always explain to people-- I teach people that companies have only two kinds of activities, money making and money spending. In biz school they're called profit centers and cost centers; that's just polite. We're a money-spending operation. We get funded by the money-making operations. Do you think we owe them any obligation? And so how the top technical person relates to the top general management or by extension the top layer or two but especially for the top is really critical and if there's a level of trust there then you really can be very effective. So once again I was in a position where "Don't tell me what you want; I'm going to tell you what you need. And I will figure out what you need and the things that you need I will figure out what I can contribute given realities of budget, time scale and so on and so forth, disciplinary focus, can't be all things to all people, so on and so forth." That's my responsibility and of course I'm going to recruit a lot of help from my organization to make sure we do that well; it's not just my concept but I'll make sure that that work ethic and that philosophy permeates every single person in my organization. We have a lot of rope. We did specifically manage a timed distribution of projects so there's some near-return deliverables and some much longer-term higher-payoff, higher-risk kind of things so there's a distribution and we think

about that a lot. But that's our responsibility to do that in a way that both allows us to make progress, allows us to develop our own careers and also provides benefit for the organization that's supporting us in lots of ways and of course financially but more than just financially. And what I used to tell people is that all I want is an opportunity to do good and have fun and you need both; either one by itself isn't good enough. So—

Brock: What were some of the stand-out developments--

Hart: Well—

Brock: --from that laboratory--

Hart: Well, a whole bunch.

Brock: --for you?

Hart: A whole bunch. From early on, we pretty quickly remixed the staff and I never-- I rarely if ever had to actually discharge anybody. Pretty soon people figured out this new guy, Hart, was like a different deal, maybe they should leave. And some people who just really were not suited for a research environment just figured out this is not the way. So didn't take long to really rebuild the staff with really, really good people-- really super good people. So what sort of accomplishments: So this was in the early days of JPEG. We invented a JPEG implementation-- a standard JPEG- compliant implementation that could keep up with HDTV rates that was super, super fast. And it was done by just incredibly clever engineering like half-register arithmetic and round-offs that didn't hurt anything and approximations that were still JPEG compliant. And with the chip technology of the day, an Ricoh had a chip division at that point; we can make super fast JPEG chips. We invented JPEG 2000; that was actually our sole invention and I invested probably more money than I should have over five years to make that an ISO standard. We had our reasons and the commercial outcome is whatever it is but that was something. I took my interest in expert systems and with a wonderful very young guy named Jamey Graham who was just super talented and he's gone on to accomplish so many great things. But Jamey and I were a two-man project to create a system that was intended to support Ricoh's help-desk operators. So help desks are a very big deal. They're forward facing, the corporation's face to the customer. They're often positions that have high turnover and not super high skill levels so you need very good software to support that help-desk operator in his or her, often her, interactions with a remote customer. And so Jamey and I-- and I will say I-- it was mainly my idea but Jamey did a hundred percent of the implementation-- we invented a novel twist on expert systems that we labeled Query-Free Information Retrieval. And the idea was we could complement an expert system with a new component. This is a diagnostic expert system; somebody calls the help-desk operator and says, "Hey, my copier's making black streaks. What do I do?" And the help-desk operator is supposed to get to the bottom of that as quickly as possible, have the customer fix it if possible. If essential, send a guy in a truck that but hopefully that's a last resort, etcetera. So we had a diagnostic expert system which was a Bayesian-reasoning expert system but we coupled that with an information retrieval system that hooked into Ricoh's entire maintenance library. So there's a zillion models of our widgets out there and every model has thick maintenance manuals, typically loose leaf, written with big fonts and loads of diagrams. And the technician or the help-desk

operator might need to pull the right volume out and flip it to the right part and say, “Oh”-- explain to the customer what to do. In the worst case, if you need a repairperson, the repairperson at least should go out with the right parts in the truck; otherwise it’s two trips. So we figured out this method for-- invented this-- the system that I provocatively labeled Query-Free Information Retrieval. We could figure out what piece of documentation was most relevant to the consultation at exactly that point and we could unobtrusively post some little icon that doesn’t disturb the operator or the user or kind of get in the way of the interaction. But if you want it, tap here. We’ll pull the right volume off the shelf, we’ll open it up to the right page, we’ll highlight the right paragraph or often the right figure and say, “Look over there and talk about that next.” And so that was a big deal. That system was used in our Chicago-area-- they called it the National Telecommunications Center, but it was a help desk <laughs> for I think like ten years. Ricoh, being Jamey and me, established a pretty impressive patent position on this because we thought this was a big deal. So I don’t know how many patents we have in that space; you’re familiar with patent positions or patent thickets they’re sometimes called. So we didn’t stop with one; I don’t know how many there are but several. Eventually, later I hired a wonderful young guy who had just finished a Ph.D. dissertation at the MIT Media Lab. I think he was in an area that was related to this and so he had done all the academic work and this stuf, and I asked him-- I said, “By the way”-- he said, “Oh, yeah, yeah. We know about your and Jamey’s stuff” and Jamey was next door to him at that point or whatever. I said, “You had to at least do some literature research just to check that box off on your dissertation.” He said, “Oh, yeah, yeah.” I said, “Is there anything earlier than that?” He said, “No, no. We think you’re the first ones. As far as MIT Media Lab knows, you were the first guys to do this sort of query-free, and it’s very generalizable. The only requirements in terms of that technology -- probably smarter people than me have done better things by now -- but the key requirement is the user has to be in what the user-interaction specialists sometimes call a task-oriented dialog. So you’re not just sitting down at your computer and booting it up and the computer says, “Oh, I know what you’re going to try to do,”. But if you’re engaged-- if there’s-- in a particular application where there’s a well-understood objective you’re trying to achieve-- and in our case it was diagnose the malfunction, and then prescribe a fix, so it’s task oriented; there’s a well-defined objective and you’re trying to-- you, the user, are trying to get step by step to that-- then the stuff can work because then we know-- we, the computer program, know where you are and so forth in this process and what might be most useful and so on and so forth.

Brock: The person in the call center, the help-desk operator, representative, whatever you call it, they have a computer in front of them and are they inputting answers to questions on the screen to guide that narrowing to--

Hart: Yeah, and we’re actually guiding the interaction so the first thing the operator will ask is, “Well, what model do you have, Ma’am or Sir?” “Oh, I’ve got this particular printer” or whatever it is or copier or some multifunction machine and so then they’ll have the right knowledge base and so forth. “Okay. Well, what seems to be the problem?” “Oh, it’s jamming too much” or “It’s making black streaks” or “It’s coming out with splotchy printout even though I have a new toner cartridge” or stuff like that. So our expert system-- this is before the Query-Free Information Retrieval-- will compute what’s the most valuable next question to ask. So we’ll have a procedure-- again we have an explicit network <laughs> like I mentioned for Prospector-- we will do a calculation behind the scene that says the next most valuable question in terms of narrowing the possibilities, ask the user this. And so normally it’s a very small number of questions-- it’s

not usually ten or fifteen; maybe it's I'm going to guess three or four or five or something like that, not a huge number of questions-- and that's the way the dialog will proceed. So we, the computer program, know what the user is trying to accomplish, as I've emphasized, and where we are in the diagnostic interaction and what the next best question is to ask and what documentation is relevant. The great thing about maintenance documentation is that the table of contents is extremely detailed; it's typically a multilevel, may go down four or even more levels to paragraph level, The TOC is a wonderful index into the semantics and you can do an analysis of the TOC to figure out oh, go here and so forth. And that stuff not only worked but it's really the-- it's the only research project I've ever personally participated in that worked much better than I thought it would, much sooner than I thought it would. And I just give Jamey all the credit; <laughs> he's remarkable. If somebody's watching this who can hire Jamey Graham go hire him. <laughs>

Brock: How big did the laboratory become?

Hart: I don't remember. There were multiple incarnations. The research center itself might have gotten to forty-ish or something but then we started another-- we did a number of other things. At one point, we spun the research center out of the-- of Ricoh's major North American subsidiary. Ricoh like all multinationals has hundreds of subsidiaries. There's a huge North American one and I was a senior vice president of that. We spun it out from that umbrella and became a direct subsidiary of Ricoh Company, Ltd., which is the parent in Japan, so I went from being senior vice president of the North American subsidiary to be President and Chairman of the Board of Ricoh Innovations, Inc., a California corp. that was a subsidiary of the parent in Japan. Several other things happened that made your question more complicated <laughs> than you would imagine. I had a long sequence of bosses. Practically every year there'd be a new top technical person in Japan. Dr. Onoe eventually retired. At one time, I was fortunate enough to have my direct boss, Sakurai-san who was the president of Ricoh Europe, and another one of these absolutely remarkable world citizens. I think if you take Sakurai-san and parachute him into any country on the face of the Earth in 30 days he'd be working super effectively and doing whatever needed to be-- and just a wonderfully adventurous intellect; I mean another one of these really world-class people. And he and I developed a very good relationship when he became my boss and then a year later he became the CEO of all of Ricoh and sometime after that chairman and CEO of all of Ricoh worldwide. One day I got a call from Tokyo from Sakurai-san-- his English was excellent-- "Peter-san," and we chatted a little bit and he says he wants me to do something, "Yes, Sir, Mr. Sakurai." And so what happened was he decided-- Ricoh has a category like all large Japanese companies of senior vice president, called a Shikkou Yakuin in Japanese, which is a certain type of senior vice president; it's a corporate officer of the parent company-- and he said he wanted me to become a senior vice president of the parent company,. That was in addition to being Chairman and President of the subsidiary in California. And that would involve some additional responsibilities and would I please do that. And of course I found it fabulous. I worked very comfortably in the Japanese corporate environment, very effectively. I felt, wonderful people, for the most part wonderful people I met and worked with, Sakurai-san being a great example. So I said, "Of course. I'd be happy to." And so the next thing I know I'm the first non-Japanese person to become a corporate officer of Ricoh Company, Ltd., which existed since the 1930s, and I troll around the web at the time-- this-- I don't know the exact time; it would have been mid '90s or-- I don't remember exactly-- the late '90s-- I can't remember exactly when-- but at least at that time

I could not find very many westerners who were corporate officers of major Japanese multinationals. There were some. I'm not a student of that; I didn't do an exhaustive study, but it wasn't common. And that started a new phase in my Ricoh career because then I had this kind of front-row-participant seat at the senior executive level of a major Japanese company and so that was just fabulous. I mean I contributed as best as I was able, and I learned a ton and had a wonderful time.

Brock: Was your role evaluating kind of the technical situations around the globe and all of the various operations or what--

Hart: No, not principally. Occasionally things would come up. For example, somebody would approach Ricoh, "Would you buy"-- I won't mention any names even this much later but for example some well-known technology company would-- was trying to get acquired by Ricoh and Ricoh would point that-- point an individual at me and say, "Peter-san, please evaluate this and tell us whether we should seriously pursue it" and in some cases they were household names that I don't want to repeat even now but that was just a side job. The most ongoing thing that was fascinating is that Ricoh-- I don't know how long it went on for before or after my tenure. But during my tenure Ricoh had a practice, Ricoh being Ricoh Company, Ltd., the parent in Tokyo, of having twice-a-year-- semiannual meetings they called worldwide global strategy meetings. And so the top tier of Ricoh executives worldwide-- which would be maybe 30 or 40, something like that; maybe it got a little bit bigger in later years-- would meet someplace, sometimes we'd do a complete offsite like the equivalent of Asilomar but-- a Japanese equivalent of a Asilomar or something like that or sometimes just a very nice Ricoh facility. And we'd meet for maybe two and a half days and there'd be a defined agenda with maybe two or three topics under discussion and there'd be some plenary meetings and then there'd be breakout sessions with smaller groups, half a dozen or ten or whatever it is, breakout meetings and you consider whatever topic it was and then the group would report back to the plenary thing.Very much like you've seen in symposium or technical conferences. And the first two or three that I attended-- I can't remember when it changed-- I was the only non-Japanese person in these meetings and there is a beautiful conference facility in I think it was Gotemba or someplace like that; I'm sure I was the only westerner in the village. And the first few meetings the documentation was in English because the documentation was distributed worldwide-- to executives worldwide, so English was the internal language of the company outside of Japan, of course. So the documentation was English but not the discussions, and so I always had a Japanese-- at least one, sometimes three full-time home-company Japanese support people working with me daily. Every one of them was different, but every one was fabulous, and so that person would be there to, quote, support me so they whispered the translations in my ear so I could participate. Later on when there started to be-- two European executives were nominated to this sort of exalted parent-company officer position. Then we started having UN-style simultaneous translations and Ricoh would hire a team of simultaneous translators in a soundproof booth and wireless headsets and mics and so forth and I'd speak in English in Japan and everything would be simultaneously-- but that was later so-- but I mention this partly because it-- maybe for some future audience this was really interesting and I thought it was fabulous. I mean it was so interesting and you're really working on meaty problems and everybody's really trying to make some progress or break through, what do we do about this or that opportunity or issue or whatever it might be of the day. It was just so interesting and of course the socialization was fabulous. One of the places we used to go would have a karaoke in the basement in one of these

beautiful conference grounds and afterwards the then-CEO and chairman Hamada-san would come down with me and take me by the arm. And there'd be three activities so he'd say "Peter-san, let's do a karaoke" and so we do that; we drink beer and I do a karaoke number but never in Japanese because even though I studied a lot of Japanese I would do it in Spanish because that was a neutral language-- and then it was "Oh, Peter-san, you have a good spirit of public service.

Brock: It wasn't--

Hart: And it was just so much fun and so interesting but in terms of what I contributed this was a lot of-- I'm kind of telling you about a lot of affect and you can see that I think it was wonderful--

Brock: Yeah. I really love that.

Hart: --but in terms of did I actually contribute everything-- anything I'll give you one comment. So one time one of my wonderful chuzain that's the local person who-- Japanese person who would-- sometimes they worked for as long as five or eight years, sometimes a few years-- one time Shin Adachi his name was, another one of these wonderful people who assisted me, he said to me-- coming out of one of these meetings he said to me, "Peter-san," he said, "every time your opinion or your recommendation is different from all the other Japanese executives" and I say, "Well, Shin, now that you mention it I guess you're right but I want you to know it's not because I'm trying to be different. If I'm asked for my opinion, I'm happy to offer it and if it happens to be different from everybody else's opinion, okay." So in the context of your question, David, possibly there was a contribution there and I worked very, very hard to be understandable in English. I actually wrote a chapter for my folks about how to be understandable in English—

Brock: That's very wise.

Hart: --and what to do and not do when it's easy and it just-- I'm kind of a natural mimic and I naturally adapt to any environment that I'm in; it just takes a little bit of observation. And I keep my graphics bold and my words on a slide minimal and I'll-- this-- I don't want to sound boastful but this is an-- one interesting touchstone on what is it was like then. So Hamada-san was the chairman and CEO at the time that I was first recruited and remained at least as chairman and then as executive advisor for quite a few years. One time I was giving an executive briefing on something I felt was an extremely important direction for Ricoh to move-- this would have been in the late '90s-- something called information appliances and why that was important. I'll get to that because then I eventually started a subsidiary to do that, yeah, another subsidiary. So I did this pitch in Tokyo and there might have been 30 top-level executives in the room beginning with Hamada-san who was the number-one guy and a hundred percent of the next two levels down or whatever it was and at the end—Hamada-san was famous for asking what the people called "Hamada-san questions" like "Why is the sky blue?" or something and he was famous for these-- he was a very strategic, very insightful guy, another terrific guy and he sort of wonders out loud and of course it's such a hierarchical culture that people are hushed; he speaks. Hamada-san-- it's almost like he's almost like he's talking to himself and he says, "Why"-- he was a fairly good English speaker, he understood more English than most of the Japanese people recognized, but he wasn't totally fluent. But he had a rather good level of understanding-- and he said-- and he's saying this in Japanese and my guy

is translating it for me-- he's saying "Why is it that he can understand Peter-san in English with no translation better than he can understand Ricoh Japan technical people in Japanese?"; "Why do you think that is?", a Hamada-san-style question. Nobody in the room dares answer <laughs> and he says-- he answers his own question. He says, "I think it's because Ricoh technical people use a very"-- and I'm paraphrasing a translation now so this is not exact but I think it's the burden of the message-- he says, "I think it's because our technical people use a kind of jargon or in-group kind of slang that even though it's Japanese they're the only ones who understand it" and nobody outside that group understands this kind of in-group kind of jargon and I'm trying very hard to avoid it. <laughs> So that was my high point as a communicator and it's this kind of proof that you can be understood if you really make an effort to make yourself understood.

END OF THE INTERVIEW

