Oral History of Mark Kryder

Interviewed by:
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Bajorek: This oral history records Professor Kryder's contributions in three areas: the establishment of one of the most successful data storage, joint university industry and government research centers in the United States at Carnegie Mellon University; his tenure as chief technology officer at Seagate; and his contributions as an individual and leader to the advancement of data storage technology especially prototyping, development and commercialization of perpendicular magnetic recording, full disk encryption and heat assisted magnetic recording.

I'd like us to start, Professor Kryder by having you tell us about your family background, where you were born, where you grew up and which schools have you attended.

Kryder: All right. Well, I grew up out in a little town called Milwaukie, Oregon. Not Milwaukee, Wisconsin, but Milwaukie, Oregon. And, actually, I lived well outside of Milwaukie, Oregon. Our address was Milwaukie, Oregon, but we lived in a rural area outside the town. Milwaukie, Oregon at that time had about 5,000 people in it, but I was probably five to ten miles outside of Milwaukie itself. We had about five acres of land, all wooded. That was the mid-forties. My dad was an electrical engineer at the Bonneville Power Administration, which supplies most all of the hydroelectric power in Oregon. He built our house. Not in the way people talk about building houses today or having your house built. My dad built our house. He laid every brick. He did everything in the place along with my brother and sister. I was the youngest in the group. My brother is ten years old than I am. My sister is eight years older. And I was about two or three while he was building the house with radiant heat. It was a slab house with a single fireplace in the middle. And there was a coil of copper tubing wound in a conical shape above the fire in the chimney. Then that copper tubing was run through the slab in the house. My job was to sit on the bucket while he wound the copper tubing around the bucket so it would go back and forth every 15 inches. So I got an early introduction to practical engineering so to speak while my dad was building that place.

I grew up in that area. You know, in a rural area like that you somehow-- you're very independent. I mean I can remember that quite often, I'd go out in the morning and I'd come back at five or six at night, and I was a little kid. You know I'd probably be over at a neighbor's house in a cherry tree throwing cherries at other kids and having cherry fights or something. We played a lot of cowboys and Indians, all kinds of stuff in the woods. So I had a pretty freewheeling background. My dad did take the effort and bought dry cell batteries and had some knife switches and taught me a little bit about how electrical circuits worked and so forth. Ultimately, I became interested in ham radio. This was after World War II, and there was a shipyard in Portland, Oregon, not too far from where we lived called Zidell. They had torn apart a lot of old ships from World War II. I used to go down and rummage around in there. They had huge piles of electronics/radio equipment off of the ships. And I used to scrounge parts and put together a ham radio system with them. So that was sort of my early introduction into electrical engineering, I guess.

Bajorek: Technology.

Kryder: That's right. And from there, yeah, I went to high school in Oregon, played football. I was on the all-state team in football. I actually played a little bit at Stanford, just briefly. And then after high school I went to college at Stanford and majored in electrical engineering. After that I applied for admission to grad school, and, as you know, ended up going to Caltech. The two universites were a great combination, quite frankly in my view, fantastic. Stanford offered you a very broad education. Caltech was really focused on whatever you were doing your Ph.D in with a lot of personal attention.

When I was at Caltech I made up my mind that I really wanted to go into teaching. But one of my observations was that almost all of the really good professors that I knew at Caltech in engineering had worked at one industrial firm or another for a period of time. My advisor, Floyd Humphrey worked at Bell Labs for a period of time. But a lot of the faculty had had experiences like that.

And also while I was at Caltech we had a visiting professor by the name of Horst Hoffman-who was well known in the magnetics community. At Caltech in the sixties I built a system with which one could take ten-nanosecond photographs, using a Q-switched laser as a light source, of magnetic domain patterns using the Kerr magneto optic effect in thin films. Now, we were working on thin film memory, and in those days the main-- the computer memory that was used consisted of ferrite cores, little donuts of ferrite with a couple of wires run through them. Depending on whether the ferrite was magnetized clockwise or counterclockwise that was a one or a zero. You could read it out when it switched. But thin film memory had the potential to switch faster than ferrite core. And so we worked on-- we were doing research on thin film memory. I was trying to understand the mechanisms by which the thin film switched. So we built this camera that would take pictures of the magnetic domains with a ten nanosecond exposure time, which in those days was pretty short. It also required your having a Q-switched laser in order to do it. And in those days you didn't go out and buy a laser. You built lasers. So everything was hands on.

When I went to Caltech I spent the first summer there helping to prepare one of the laboratories before I had even entered the school, and I was working for a professor by the name of Mark Nicolet, who is in the semiconductor solid state area. In Caltech's graduate program, the first year you just do courses working on a master's degree and then, assuming you were going on, to select a PhD advisor. I ended up getting into magnetics after that first year. I had the opportunity to work with Floyd Humphrey, though Mark Nicolet wanted me to work with him too. But the thing that attracted me to magnetics was actually the fact that Floyd had this system that he was trying to put together that had all of the required very high-speed pulsers, lasers, all kinds of stuff. To me that was appealing. I liked hardware and tackling tough problems with stuff like that. Also, Mark Nicolet and Floyd Humphrey had different personalitiies. Floyd Humphrey was pretty hands-off when advising graduate students. Mark Nicolet, in my observation, was a little bit more of a micromanager of the people who were working for him. And like I said, I grew up being independentent and able to do what I wanted, more or less as a kid. So Floyd's hands-off approach appealed to me.

Consequently I ended up getting a Ph.D. by taking high speed photographs of thin film switching and was trying to understand, to develop a model for describing the switching that was going on which was quite complex. It wasn't a coherent rotation. It was very incoherent. At that time, Professor Horst Hoffman came to Caltech as a visiting professor from Germany. He had a theory that he called the ripple theory. By working together and applying ripple theory, we were able to explain some of the results and build a model for what we were observing. And as he was leaving Caltech, he volunteered that if I ever wanted a position in Germany, that I could go there and have a job with him in Germany. I stayed on at Caltech for a couple more years as a post doc, but the attraction of going to Germany was great, because I knew that once I got a real job, I wasn't going to be able to go live in Europe for a period of time and have all of the experiences that one would have. In those days I honestly was not particularly concerned about being able to get a job in the U.S. And so I took him up on his offer and went to Germany and did a post doc there at the University of Regensburg. Officially, a visiting scientist position they called it.

Before I discuss my experiences in Regensburg, I should say that, although I wasn't working on magnetic recording, I do recall while I was there at Caltech going out and visiting Burroughs Corporation. At the time Burroughs was trying to make 36-inch disks. They had huge plating baths, and I saw 36-inch hard disks being plated up which gives you an idea of the sort of changes we observed since those days.

Anyway, in 1971 after spending two years as a post-doc at Caltech, I went to Regensburg. The University of Regensburg was a brand-new university at that time, and it had been well funded by the German government. They had a huge budget for capital equipment because it was a new university, and Professor Hoffman wanted me to build a high speed camera system for them. This time I took another approach, because by then commercial lasers were available and I ended up buying a commercial laser.

We used the high speed camera system in Regensburg not for studying flat film memory, but for studying magneto-optic recording, because in 1969 all interest in studying flat film memory died when IBM decided to use semiconductor memory instead. The way in which this happened is significant in a way to the history of magnetic recording research at universities. What happened was in 1969,when I was completing my PhD, Floyd Humphrey and I were looking for funding for my post doc, and we submitted a proposal to IBM for funding additional work with the high-speed camera that I had built. They responded positively and sent back a reply that yes they wanted to fund it, but they wanted some changes to the intellectual property agreement. So we worked those details out from the Caltech side, and I paid a visit to Burlington, Vermont where IBM was building the flat film memories. (They had actually shipped two IBM 360 computers out to Moffett Field using flat film memory.) However, by the time I got there IBM had decided they were going to use DRAM instead of flat film memory in future computers. So they were busy closing down the Burlington factory and converting it over to making DRAM. As a result we never got the funding because we never managed to sign the contract in a suitable time, and the impact of that was enormous on US universities.

There were many universities that had been working on thin film memory technology, but when the decision was made to go with DRAM, most of the universities switched over to working on semiconductors. Dick Barker, who had been leading magnetics research at Yale, for example, totally switched into working on semiconductors. Floyd Humphrey and Chuck Wilts at Caltech, somewhat uniquely, continued doing magnetics research, but they chose to work on magnetic bubble memory, which then was a new idea that had come from Andrew Bobeck at Bell Labs. Fritz Friedlaender at Purdue also stayed in magnetics and did some work on bubbles and magnetic separation. There was also a small effort on magnetic recording at the University of Minnesota led by Jack Judy, but generally magnetic recording was then not seen as a field for doing research at a university, because “it was “too mature a technology.” It didn’t make sense to be doing work on that in a university.” That was the scenario in the late sixties. So anyway I went off then to the University of Regensburg…

Bajorek: But you never got the post doc funding at Caltech?

Kryder: We didn’t get funding from IBM, but we got funding from the NSF and some other sources.

Bajorek: And you continued to study the…

Kryder: We used the high-speed camera for studying magnetic bubble memory and things of that nature. But then I went to the University of Regensburg and at that time there was a lot of interest in magneto optic recording. And so at the University of Regensburg after building this high-speed camera, what we used it for was trying to take dynamic pictures of the magneto-optic recording process in manganese bismuth films, which at that time were one of the promising candidates for magneto-optic recording. This was before the invention of the amorphous rare-earth transition metal films. Or at least in parallel with them. And so we worked on that at the University of Regensburg. And to be honest, yeah, I worked hard at the University of Regensburg, but I also had a very good time at the University of Regensburg and we did a lot of traveling all over Europe while we were there. I had a three-month-old daughter when we went there. And we spent a year-and-a-half there. And then I looked for jobs back in the States. I actually considered staying because we were having such a good time in Germany. But I decided to come back to the States and look for jobs. I considered myself capable of…

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Bajorek: So pick up you were thinking what to do after Regensburg.

Kryder: Right. So after Regensburg I was trying to-- I was planning on coming back to the States. And I looked at various options. I mean I threw the doors open, really. I didn’t-- I wasn’t focused on necessarily staying in magnetics even. I interviewed at North American Rockwell Science Center out in California, and a large number of other places. And had job offers from a number of them working in the semiconductor field, because Caltech, if you went there, you got an education in all aspects of solid state, in my particular case, and magnetics was one part of it. But in the end, I had the opportunity to work at IBM on magnetic bubble memory. And I decided to stick with magnetics, which I think was a good choice.

Actually, one of the things that Floyd Humphrey had said to me when I was-- way back when I was a grad student-- was that one had the choice of being a small fish in a large pond by working in semiconductors, or the opportunity to be a big fish in a small pond by working in magnetics. To me the latter sounded more attractive and that was, to some extent, why I ended up sticking with magnetics.

As it turned out I think it was the right choice. I went to IBM. I got there shortly after the rare-earth transition metal thin films had been invented by Chaudhari, Cuomo and Gambino. Up until that time all magnetic bubble work was being done on single crystal garnets, and the idea that you could possibly make a bubble memory using a glass substrate and rare-earth transition metal sputter deposited films was pretty attractive, because garnets were expensive. So that was the project, which I took on while I was at IBM.

When IBM interviewed me in 1973, they gave me a job offer, but the statement was “well, I could come, but they didn’t want me to build another high-speed camera”. But within about a year-and-a-half they wanted me to build a camera, and I did. I built another camera that I could use there for studying magnetic bubble memory. I became a first level manager there in the bubble memory area. I was actually asked on various occasions whether I wanted to become a second level manager, but I told them no. The reason was that my observation was, as a first level manager at IBM you’re in the lab at least as much as you’re managing, probably more. And you could still stay really technical and on top of what you were working on. At second level you’re technically involved but you’re removed from the lab. You aren’t anymore working in the lab. And I really loved working with the hardware. And so I had no aspiration to do that. In the back of my mind was still this view that I was going to go off and work in academia.

You know, my thought in advance of joining IBM was “well, I’ll spend three years there.” It seemed about the right time. It’s enough to learn how things work in industry and then go off. Well, I ended up spending five-and-a-half. I did look around for an academic position at that point in time. And I had various offers from various universities. I decided to go to Carnegie Mellon. At the time, I thought I was half crazy to be doing this. I mean it was a big move, a huge move in a way because I had a very good reputation at IBM, as I said. I was a first level manager and at that period of time, they more or less offered me a second level position, but I turned it down. The magnetic bubble memory program was right in the midst of moving a large portion of it out to San Jose and they very much wanted me to come out to San Jose. I really wasn’t terribly interested in going to San Jose. I was more interested in trying to chase new magnetic storage technologies and I had some ideas of things to do. And Ralph Gomory (Then Director of IBM Research) was willing to say, “Okay, you can stay here. I’ll give you a team and you can have your own group to do whatever you want here.” I had all of the opportunities at IBM that I could possibly ask for. But somehow I had always planned on trying to go into teaching, and I just decided, well, it's time to do it. I bit the bullet and I went to Carnegie Mellon.

The reason I chose Carnegie Mellon is that interestingly enough they had had a very strong semiconductor solid state program having made some of the real contributions early on. They also had a history of working in magnetics and a couple of professors who still worked in magnetics. But what had happened is that one of the semiconductor professors had gone off to work at SERI, the Solar Energy Research Institute. One of them had become the head of the electrical engineering department. Another one was still there doing work on gallium arsenide technology. But they'd largely lost this huge thrust on semiconductor solid state. Yet, they had a "cleanroom" and I put that in quotes because it was a rather rudimentary cleanroom. But they had a cleanroom, and they had a fairly new Perkin Elmer sputtering system, which was in those days a fairly good research tool. And they didn't have anybody to use it. So in the end I ended up going there. There were universities which in some respects had better reputations than Carnegie Mellon, although Carnegie Mellon even then wasn't a bad place, and they too were after me to come. But the problem I saw was that if I went to the other universities I'd have been one more professor trying to make my way through the crowd. On the other hand, I really felt that Carnegie Mellon wanted me to come. So I ended up joining Carnegie Mellon.

I didn't care whether I had tenure or not. It didn't make any difference to me. My attitude was if they wanted me to stay, fine. If they didn't want me to say I wasn't worried about it, because I knew I could get a job. I wasn't going to burn bridges at IBM. I figured I could go back if I needed to, but the bottom line is within a year they made me full professor and gave me tenure.

The first year I didn't work on magnetic recording. I worked solely on magnetic bubble memory. By the second year I think it was either the second or the third year, Mark Re came in as one of my students. And that was the beginning of working on magnetic recording. I also had quite a program on magneto-optic recording. And the thing that I found going to Carnegie Mellon was that I could greatly broaden the scope of things that I worked on. At IBM I had a design group, design and testing for magnetic bubble memory, and there was another group who would do the fabrication. At CMU the great thing was I had to do the fabrication. I had to do design and testing. I had to do all of this stuff. And I could look at all of these different technologies, bubbles, magneto optic recording, magnetic recording, and sort of work in all of them and get a sense of where they stacked up against each other, understand the problems and tackle them.. I really enjoyed that. So it gave me-- it allowed me to gain scope relative to what one would typically do in companies. I mean to be honest with you, I'm not being critical of companies, but I think what companies necessarily do is focus you in one area. Why would a company take somebody who is really good at working on heads and put him over in media? Companies sometimes do that as they try to develop people for upper level positions, but you don't have quite as much latitude in doing that, I don't think, at a company as you do by going to a university, assuming you can get funding. And I was very successful in getting funding. So within three of four years, I had built up a very sizeable program and I had all of the funding I could use myself.

Like I said, when I went there, there were a couple of faculty, Stan Charap and Joe Artman, who worked in the magnetics area. I recruited a couple more Zoltan Cendes, a finite element modeling guy, Al Thiele came along with Floyd Humphrey who joined me from Caltech. So we had a core group of people doing work on magnetics. This was against the backdrop where if you went to an Intermag conference, or a 3M conference you could literally count the number of papers from US academics on the fingers of one hand. I mean there was almost no one from an academic institution working on magnetic recording in the U.S. Yet, from Japan and elsewhere there were significant numbers still involved, but in the U.S. there weren't any.

Another thing is that I listened to, John Linvill at an NSF meeting talking about the Center for Integrated Systems that they were putting together at Stanford. His argument was that at Stanford it cost about a quarter million dollars per year to-- excuse me, it cost about $50,000 a year to support a grad student. He took the example of Ted Hoff, who invented the microprocessor at Intel, and he pointed out that it took him five years to get his Ph.D. Intel hires him and look what they end up with. And his argument was that companies joining their center really ought to be willing to put in a quarter million dollars because that's equivalent to paying for one Ph.D. student per year on an ongoing basis. He felt that was a rationale for setting the number at that level. I heard that and I thought well, you know, it makes sense, and decided that that was what we ought to do at CMU, but in the field of magnetics. So I invited about twenty of the people that I considered to be the technical gurus in magnetics to Carnegie Mellon for a two-day workshop.

Bajorek: They were from industry or from academia?

Kryder: They were almost all from industry, names that are well known like Jim Lemke and Dave Thompson, and Neal Bertram and so forth. And they came. The focus of the meeting was to try to figure out what it was that an academic center would do in the magnetics area that would really benefit the industry, but would not pit grad students head-to-head against IBM researchers trying to solve the same problem. And so you look for problems that industry is interested in, but they're not going to provide the resources to get it done. Maybe it's a little bit more science-based than engineering-based. Or maybe it's just a little too far out for them to bother to do. We identified thirty topics, which fit that description. We held that meeting in April of 1982, and I then took the time to write up a proposal based upon those topics. I went back to-- essentially I sent it to all of the attendees who had been at the meeting, but I also went back to those companies, requesting that they fund a Magnetics Technology C center at CMU. I put together the concept of a center. We had the top level-- we had to have it get going. You don't produce wonders with a grad student in one year. So I knew that if we were going to make this successful we had to have some sustained funding. So I put it into the proposal that in order to join at the highest level, you had to commit to three years of funding at a quarter-million-dollars a year each. Now, that's a tough requirement to some extent for some companies, because to sign something like that, they basically have to sign it, and then sequester those funds. They come off their bottom line that year, not as they pay them out. And that was an obstacle, but I really felt it was important. If we were going to get this going, we had to have the resources to do it. I always tried to run the Center like a business. I mean I saw it as a business and felt it was appropriate to run it like a business.

It took me about a year to gain industrial support. I had great support from the administration at Carnegie Mellon. Angel Jordan, who had been the electrical engineering department head when I joined, had been promoted to be dean. Dick Cyert was the president. And Angel talked to Cyert about me and my program and actually even before that, Cyert came to the workshop. And I can remember Jim Lemke asking him at the end of the workshop, "How much money do you think you need to do this?" And Cyert didn't really have an answer, but he sort of looked at me.–And we quoted a multimillion dollar number which was not unreasonable. It was an appropriate sort of number. And so Dick was very much onboard. Through the year my approach to going after companies was that I would send my proposal to all of the people that I knew well that I thought were well placed. Now, the people I knew well were not high level people, but they were very respected in their companies. They were first level and second level managers in technical areas. They knew me. I knew them. I think we had some credibility with them. I actually met weekly for most of that year with both Dick Cyert and Angel Jordan and we would figure out the strategy for going after each company. The approach was that I tried to get people at the midlevel management level to understand what it was we were trying to do and get their support. But I had lived at IBM, and I knew that from a second level manager's position it's not the easiest thing in the world (and a lot of people wouldn't even attempt) to push our proposal up high enough to get a company to possibly cough up three-quarter-million-dollars for a university. So, in the case of Seagate where the entire company was focused on data storage, Dick Cyert, the President of CMU, sent a letter to Al Shugart and requested funding. On the other hand, at IBM only a division of the company was focused on data storage. So, Angel Jordan, as the dean, wrote to Art Armstrong, who was the vice president in charge of the division responsible for magnetic recording. Thus, we tried to contact the companies at corresponding levels of people. But the interesting thing is a company president or vice president, if they receive a letter from a president of a university or a dean, who is sort of a vice president at a university, they don't just discard it. They say, "This President (or Dean) of a University is asking for all of this money. I don't know if it makes sense." So, they send it down the line and ask the question "does this make any sense? Should we do this?" I think where we were successful was if I had done my work to get the technical guys familiar with the proposal, when the President or Vice President asked the question, it bounced back with an affirmative reply, because then it wasn't a guy from below trying to push it up. It was the question being asked at a high level of the people at a lower level. They could respond and they could say, "Yeah, it makes some sense from our point of view. Why don't we take a look at it." And that worked. Dick Cyert and Angel Jordan actually flew out with me on numerous trips. We went to Minneapolis. We went to Denver to meet with various companies in order to try to sell them on this. And in May of 1983 both IBM and 3M committed to funding us at three-quarter-million-dollars each. The other thing I should say, though, is that even without the funding, Carnegie Mellon built a brand new 10,000-square foot cleanroom for the center. They built it, actually, in the old coal plant in Hamerschlag Hall where the old…

Bajorek: This was like the power generating building.

Kryder: That's right. A steam plant.

Bajorek: Steam plant. Okay.

Kryder: And they ripped all of that out and then built a clean room down in that space. And they did that with university funds. Frankly, I can remember when 3M came around they sent about four or five VPs of their various data storage groups. They could see the construction going on. I think people got convinced that yeah, we were serious about doing this. And so yes, IBM and 3M kicked it off. And within a year we had over $5 million in funding from a wide variety of companies because once IBM and 3M kicked in everything fell in place.

Bajorek: Do you remember not all of them, but beyond IBM and 3M who were some of the early joiners?

Kryder: Yeah, well, Kodak. Seagate. Seagate didn't join at a high level at first. They joined at a-- we had a menu of memberships. And actually, I think a lot of our success is because we had that menu. A quarter-million-dollars a year in 1983 was quite a bit of money for a company. And so the fact that we offered a membership without getting any patent rights or things like that at $50,000 a year to companies that were smaller was an attraction. And we had different membership levels. Fifty thousand basically got you access to what was going on, but you were going to have to pay for patent rights. There were some intermediate levels where I mean they were designed to some extent for specific companies. If you were only interested in media, you didn't want patents on heads and so forth, well, we'd cut a deal. We'd do it for maybe $125,000 or something like that. So there was a spectrum of ways that companies could get involved. And the other comment is that during this time there was another center trying to be formed. Jim Lemke was trying to get it going.

Bajorek: He wanted to copy your idea.

Kryder: Jim wanted to do it. It may honestly have been both people doing things at the same time. I don't know. But he wanted to do it. He tried to sell it at Stanford. Stanford wasn't interested. Eventually, Jim was able to get it to go-- get the University of California at San Diego to commit to do it. But the University of California at San Diego didn't have anybody who knew anything about magnetics. And so they were going to have to hire all of their faculty. And another guy well-known in the magnetic recording field Al Hoagland was selected by IBM to try to figure out how to fund these various centers. And Al, I think, I don't know the details of how all of this was working. But Al, at one point, prior to IBM's actually funding it, offered us $75,000 instead of three-quarter-million as sort of a token thing to get started. And I ended up turning it down, telling them if I accepted $75,000 from IBM we were never going to get the center off the ground. I understand from some IBMers whom I know that I was called "haughty" for having done that. But I think it was the right decision because in reality, how could I go to a Seagate or some other company if IBM weren't willing to do the thing on the scale that was appropriate. And fortunately, due to you, Chris, and Dave Thompson played a big role in these things, you know, IBM did end up funding the center. And 3M did, also. And like I said, Kodak joined. Digital Equipment Corporation joined, Seagate joined. Kodak and Digital Equipment both joined at the quarter-million-dollar a year level. But there were a horde of small player companies who joined the center and supported it. A lot of them were just suppliers. I mean substrate manufacturers – Alcoa. Large numbers of companies joined and supported the center.

Bajorek: That was interesting that you were able to attract the food chain, the whole supply chain and not just the end users of those components.

Kryder: That's right.

Bajorek: I don't mean to distract you. I want you to keep going. But I think you may have also been familiar at that time with the change in feeling in the industry about Japan's competitiveness in this field. If you could talk a little bit about that.

Kryder: No, that's good. Yeah, that was actually part of the proposal and quite honestly it's good that you reminded me. You know, in the eighties what was happening in the U.S. was we lost the TV business. Right? No TVs manufactured in the U.S. We lost the DRAM business. That was all being done in Japan. And hard drives were being threatened very severely particularly by, at that time what I remember, is the Fujitsu Eagle drive was selling like hotcakes in the U.S. and Hitachi wasn't bad either. So they were-- the Japanese were threatening to take away the industry in the U.S. And I utilized that extensively in my pitches to the companies. And the companies were responsive to that. There wasn't any question. They were a little bit concerned about that. And there were many workshops. The NSF sponsored some. The DoD sponsored some at which I managed to get people from companies like IBM to come. And I can remember them pointing out that there are no academics working in magnetic recording. And like I said, the view had been hard drives are a mature technology. Never mind that they went, I don't know how many, probably five or six orders of magnitude beyond what they were at that time in areal density but that was the view at that time. And so the companies responded by supporting the center. And when we formed the center, the University of California San Diego also got funding at the same time, but they went out and they hired faculty. And became a good magnetic recording center. There were other universities like the University of California at Berkeley. They had been working on the mechanical aspects of hard drives, but not on the magnetic aspects. And they too formed a center. The University of Alabama decided to get into the business and put together the faculty to do so. The University of Minnesota greatly expanded their program. So there were a lot of copycat centers that then joined in. So in a way we-- I don't think there's much doubt about the fact that we sort of were the nucleus for starting centers throughout the U.S. in academic institutions working on magnetic data storage in one form or another. And most of them were addressing magnetic recording problems.

Bajorek: It seems like you had-- because of the timing and base you started, you sort of had a head start. Right?

Kryder: Yes, absolutely.

Bajorek: San Diego had to start with a cornfield. They had to build a building before they could do anything.

Kryder: Right. No, we had everything in place. And we were able to move quickly. But I also think that our-- universities have, in my view, a bit of a problem running like a company. And I really tried to run the Magnetics Technology Center, as it was called, in the early days like a business and our business was R&D. But to run it like a business. And all too often I think that academic professors are willing to take that $75,000 and try to do something instead of demanding what they ought to be getting for it. And so when I say I ran it like a business, I wanted to get paid an appropriate amount for what it was worth and what we were doing. But the other side is I watched out for the sponsors' interests as much as I possibly could.

Bajorek: You wanted to create deliverables.

Kryder: Absolutely. And I was very responsive to the sponsors in terms of what it was they wanted to do. And I would fight for them, if there were intellectual property issues, things like that. But the other thing is that I, on the other hand, the way we were running the center, we wouldn't allow exclusive rights for contracted work done within the center. If you were going to work with us, you could come in. You could fund the research. We'd give you a license if you joined at a high enough level, but we were going to own it. And we were going to make it available to others, but not for free. They'd pay a royalty for it, which I think is a good model for ways that academia can operate because it didn't encumber us. If you take on an exclusive project, then people can't talk to each other. It doesn't work.

Later on, after I joined Seagate, I looked at that from the other side. If a company has a project which it wants done and wants exclusive rights to it, why on earth are you doing it through a university? I mean you ought to do it with your own resources.

Bajorek: You want to take a break, Mark? Are you okay?

Kryder: I'm okay. I'm fine.

Bajorek: No problem. I just want to make sure you're comfortable. Initially, I think as you said the center was primarily funded by the university itself and industry.

Kryder: Right.

Bajorek: Later on, I think you persuaded government institutions to join.

Kryder: Yes, right.

Bajorek: Is this the right time to talk about that?

Kryder: Well, I'll give you another thing before that and then we'll come to that.

Bajorek: Okay.

Kryder: The other thing that happened with the centers was that the companies began to get concerned that the centers would be duplicating each other, because we spawned centers at all of these different universities. Now, my view is there was no reason for the concern because no two professors will try to do the same thing. Professors hate doing what somebody else is doing. They always try to do something different.

Bajorek: Original. Original work.

Kryder: And have their own new original way of trying to do something. But that was the motivation. The companies were concerned that we were going to try to duplicate what Berkeley was doing, for instance, because we were getting-- we were doing stuff-- we tried to do everything across the board in tribology and all of these different areas. And they were concerned about that. And so they decided that they wanted to form a consortium called the National Storage Industry Consortium (NSIC) in order to sort of direct these centers into different spaces. It was actually an excellent mechanism as it turned out because NSIC was able to hold regular quarterly meetings, at which the academic institutions reported on their work, and the companies would contribute too. The companies were not giving away their secrets or their crown jewels, but you can't put an IBM guy and a Seagate guy in the same room and start talking technical issues with a third party, i.e., the universities, without there being sort of a three-way exchange. And so, the universities got good guidance out of the companies from this. Moreover, the universities learned what the other universities were doing and like I say, the professors didn't want to-- the last thing they wanted to do is to do the same thing somebody else is doing. So, they would position themselves to avoid it. And this became a very excellent vehicle for pushing the frontiers in magnetic recording in the U.S.

Now, in that same timeframe, the NSF started what they called the Engineering Research Centers' program, and Carnegie Mellon was successful in getting one of the first ones in the space of engineering design. So, CMU already had an Engineering Design Research Center. These are centers that are funded initially for a five-year period at something in $3 to $4 million per year range --substantial funding for a university at the time. They were renewable for an additional three. And then they were renewable for another three, so eleven years total could be funded, assuming they were making good progress. And the way it was done was that they actually were reviewed after three years and the decision was then made whether to fund it for an additional three. So then by reviewing after three, they'd extend it to eight after that. But if they weren't going to extend it, then they'd taper down in two years, so that grad students would not be left without funding.

Bajorek: They gave you a warning light.

Kryder: Right. That's right. But these were a big deal. And Carnegie Mellon was one of, I think, there were about eight in the whole nation who already had one. But I went to the NSF and I asked them whether they thought-- whether it was possible to get a second one. And they said "Well, if you have the best proposal yeah, we'll fund it." And so, I'm stubborn. I took a shot at it. And frankly, the first year I screwed up. We made it-- our written proposal sailed through. We got to-- they then have a site visit. They came to the site visit. I had not-- what I had done when we created the original Magnetics Technology Center, we created an advisory board. But to be on the advisory board you had to be one of the Associate Members, the highest paying members. The lower paying members could not be on the advisory board. So that was one of the benefits that we were carving out for the highest paying members. And they were the ones who got automatic license to patents and so forth. So, what we proposed in our proposal technically they (the NSF) loved. But we had a structure, which was in a way untenable because what I wanted to do was to maintain the Magnetics Technology Center as this industrial run center. And all of the engineering research centers are supposed to have good industrial funding. And then I wanted the separate NSF Center doing the NSF topics on the side. And I sort of tied them together under one umbrella but I'd leave the structure the same for the old Magnetics Technology Center. Well, that, quite honestly, is not a very viable way of trying to do it and there are a lot of intellectual property issues that you can imagine cropping up in that scenario. And that came out in the site visit. I knew that I didn't want that question asked at the site visit but, of course, it was asked. I didn't have a good answer as to how to handle it. And we ended up not getting funded that year. So, I learned something. But I didn't let that deter me. I went back again. Actually, I can remember when I was made an IEEE Fellow or a Member of the National Academy of Engineering, Angel Jordan made some comments and said, "One of the things about Mark is Mark is awfully stubborn". I prefer the word persistent. In any case, I didn't let our lack of success the first time deter me. I went back to our major industrial sponsors and said, look, you're the heavy hitters in this, but we've got to open this up and make a structure where all of the small companies can participate. They don't have to get patent rights, but they've got to be on the advisory board. We've got to do things differently. So, we restructured the proposal. And the next time I went back, I knew there wasn't a single question that they could ask me for which I didn't have an answer.

Bajorek: You had a good answer for.

Kryder: Right. I knew it. We passed through the site visit. Then the final thing is you've got to go-- the director goes to Washington and meets with the blue-ribbon committee, about twelve technical people, asking you all kinds of questions about how to run the center. You go alone and you're interrogated. But the day I did that was at a time when Congress had gotten quite upset because they learned that companies from Japan were able to join MIT's associates program, (I don't know the program name) and get access to the MIT professors who would go over and talk to them. At the time, the UC Berkeley group was allowing Japanese sponsors in. And at that time, we still were not. In fact, Alcan Aluminum, which is a Canadian company, was interested in joining the center, but when I brought up their interest to the advisory board, they wouldn't allow them to join because they were afraid of the precedent. So, we were American. That was it. But anyway, that morning, before I even went to this blue-ribbon panel I was asked to go up to Congress to talk to them about funding of universities. They tried to get me to say that I was upset at UC Berkeley for accepting foreign sponsors because they knew that we were only accepting U.S. companies. Quite honestly, I told them what my own view was. I said, "you know, every university has to figure out its own optimal plan for figuring out how it's going to do this. And figure out how they're going to maximize their impact on what they're trying to do." I didn't put down Berkeley for doing that.

Bajorek: Was it Berkeley or MIT?

Kryder: No, it was Berkeley. That was Berkeley they were comparing to. MIT is the reason they were looking at it. But they knew about Berkeley and they sort of were trying to get me-- they figured that I would offer some support for their position by comparing us to Berkeley.

Bajorek: You didn't take the bait.

Kryder: I didn't take the bait. I refused to do it. But anyway, it all went well. And it was obvious at the blue-ribbon meeting that we were going to get funded. And we were.

Bajorek: Can you quantify the nature of the funding and its duration?

Kryder: We got, like I said, it was in the $3 to $4 million per year range directly from the NSF. The good thing, though, was that the NSF also required cost sharing of some sort from the university. And what I did, again, you have to think these thing through. But what is hard to get at a university? You can get funds for students. You know, companies are willing to give you funds for students. Right? You can go to the NSF. They want to fund students. Everybody wants to fund students. But it's awfully hard to get funds for equipment. And so what I did in putting our proposal together was that I made all of the cost sharing in the form of capital equipment so that whatever I spent from the NSF, CMU had to match. And that gave us a real big boost in terms of our ability to have what we needed to do state of the art research in an academic institution. I think that was another good choice.

Bajorek: And the university went into that with enthusiasm?

Kryder: Yeah. They were. I mean the truth is they didn't mind what I spent. They were going to have to cost share a certain amount. And I wrote into the proposal that it would be all spent on equipment. And so that became the mode of operation. By the way that's another thing that occurred way back if I back up a minute, when we were recruiting the initial companies, we had written into the agreement with the companies the associate members would have a license to patents. But when Digital Equipment joined they asked the question, "Well, what happens to royalties that come from these patents?" They were going to join as an associate member but their attorney had enough sense to ask that question. Now, this was prior to the Bayh-Dole Act, which made it possible for the universities to obtain patents on U.S. government funded work. And Dick Cyert who was the president of Carnegie Mellon, he had very little interest in intellectual property. His view was he'd far rather give companies the license to the patent in exchange for an additional amount of overhead. So, what he preferred to do was rather than say a 55 or 60 percent overhead rate which is sort of typical for a lot of universities, he'd want maybe 80 or 85 and then give them all of the rights they want. That was his model. And so, I wasn't even involved in this decision. Al Brannick was the general counsel for CMU at the time. And when DEC came back and asked the question "what happens to the royalties?" he simply wrote into the agreement, that the royalties would go back to the center to be used for future research. And that was the mode in which the center was founded.

Bajorek: And NSF was supportive of that.

Kryder: Oh NSF was happy about that. So, there were a number of issues like that.

Bajorek: Wasn't there also a Department of Commerce initiative that may have come later that also contributed to the center? Or was that…

Kryder: Well, most of the department…

Bajorek: I'm just thinking we're on the topic, you might want to sweep in any additional government involvement or funding.

Kryder: The things I think about more than the Department of Commerce was actually-- yeah, there is a role in the Department of Commerce too. The center itself, even prior to the NSF funding, had pretty good support out of a lot of the DoD agencies. The Air Force Office of Scientific Research sponsored a lot of work in the center. And those were, you know, focused research programs. But we got a lot of leverage out of that. The Department of Commerce piece, really impacted NSIC because what happened was when NSIC was looking for funds, the Department of Commerce had what they called the Advanced Technology Program, the ATP program. And we wrote proposals for work on magneto optic recording and for work on GMR heads. IBM was the first one to put GMR heads, as you know, into product. But there isn't much-- I mean I think that even IBM benefited from the NSIC program on GMR. And all of the companies and universities were working on GMR heads in a cooperative fashion. It was an amazingly quick timespan between when GMR was first discovered and GMR heads actually became…

Bajorek: Commercialized.

Kryder: That's right.

Bajorek: So the model was the Department of Commerce would assign funding managed by NSIC that would flow to these different centers including the center at CMU.

Kryder: That's correct.

Bajorek: And the GMR work was spearheaded out of CMU?

Kryder: Yes. Most of the programs were spearheaded out of CMU. I actually-- when we were looking for funding at one point in time for NSIC programs, I walked in the door at DARPA, the Defense Advanced Research Project Agency, and I just gave them my usual pitch about what we were trying to do. And the guy looked at me and he said, "I have to have a proposal to Congress within like a week," or something like that. And he said, "I got this money. This sounds great. But I've got to have a proposal that I can put together." And I went back from Washington. I talked to the guys at NSIC. I wrote the proposal. We got it to them. And that funded NSIC for several years. And that was the ultra-high density recording program which ultimately that was-- let's see, I have to remember whether that one-- that one was probably 100-gigabits per square inch if I recall correctly. We took various steps in NSIC and they were typically order of magnitude steps. The first one was ten gigabits. The next one was 100. And then we were shooting for a terabit per square inch and so on.

Bajorek: So Mark, when we took a break, we were talking about the NSIC and the start of the high density…

Kryder: Recording programs.

Bajorek: Recording programs. Could you pick it up from there?

Kryder: Yeah. Okay. The program that first got it in a big way in magnetic recording was UHDR or what we called UHDR, Ultra-High-Density Recording. If I recall correctly, I'm pretty sure that one was targeting ten gigabits per square inch. We got through that. Then we moved on to doing the EHDR, Extra High-Density Recording program. The goal there was a hundred gigabits per square inch, and it was during that time though that Stan Charap came out with his prediction-- he and a graduate student (Pu-Ling Lu) came out with the prediction that, if we kept scaling the way we were, magnetic recording would hit a hard limit at 36 gigabits per square inch where superparamagnetism would make the recordings no longer viable. Now at the time that came out—

Bajorek: Can you pinpoint the time frame--

Kryder: That would have been about '85 I think, something like that, and my recollection is that at the time it came out the areal density in products was around a gigabit per square inch. So, we were a factor of 36 away and you'd sort of say, "Well, that isn't a big deal. You got a long way to go," but the way the industry was moving a factor of 36 doesn't take too long and, if you work it out in years, five years from now you've got a problem and that's not very long at all on the horizon of companies, so that was a big concern.

Bajorek: Excuse me. I think you meant 1995.

Kryder: Maybe you're right. Yeah, I think you're right. It was 1995. You're right.

Bajorek: --density numbers are correct--

Kryder: My density numbers are correct—

Bajorek: --'95, '96 time frame.

Kryder: Right, and the-- but yeah, the density numbers are correct, 36 gigabits per square inch and I think the areal density then was about 1 on the product. And in response to that I scheduled a workshop. We held it in San Jose-- actually, we held it at the Seagate facility that I think was being closed down at the time in San Jose, and we had a workshop, invited all the companies and the universities that were involved in INSIC and in two days-- I mean two days of time-- in real time there we really worked out that there were two solutions. One was to change the bit aspect ratio because at that point 30:1 was pretty common for the ratio of the track widths to the bit lengths, and if you reduced that bit aspect ratio, then you could reduce the demag fields in the transition and you could go further because those demag fields work against stability of the recordings, and so that was one approach. The other approach: It was recognized at the same time that perpendicular recording could go considerably further because we could get higher head fields. You can use a soft underlayer with perpendicular recording, which gives you essentially an image of the pole of your head and effectively allows you to double the head field, which means that you can now have twice the anisotropy in the material which makes it more stable against superparamagnetism. So, both of those, the industry and the academics working in INSIC, came out with that in real time in a two-day workshop, which sort of shows you the value-- in my view this really shows the value of everybody getting together and talking about these problems, It was pretty good in two days to come up with a major solution.

I think that it shows very clearly the value of INSIC and academia working with industry in that we could have a major problem like this and then have a two-day workshop where you bring in people from industry and academia and in real time during that two-day workshop come out with two alternative solutions to the problem that turned out to be correct in reality for the entire industry. What industry chose to do, and you'd sort of say "of course," was to pursue the bit aspect ratio approach and if you look at the bit aspect ratio as a function of time you can plot it out and have what it was on average drives and you'll find that very shortly after that time frame the bit aspect ratio starts getting smaller and smaller on a steady basis. And it is true that ultimately longitudinal recording went to about a hundred gigabits per square inch, and I would argue it probably could have gone further. It didn't need to though, because as it turned out, perpendicular recording could take over in that time frame.

INSIC did not pursue perpendicular, because the industry wanted to do a hundred gigabits per square inch using longitudinal, and so the focus of INSIC's work was primarily on longitudinal recording, and they went after doing a hundred gigabits per square inch with longitudinal. There were some programs within INSIC focused on trying to do some stuff with perpendicular and media and things of that nature. And of course, Jack Judy at the University of Minnesota did work on perpendicular, we did some stuff on perpendicular in Pittsburgh at Carnegie Mellon so there was-- there were things going on in perpendicular, but not in a major way to be quite honest. I ran the INSIC EHDR program almost essentially from the start for ten years or through VHDR , UHDR and EHDR,

Bajorek: That's about a ten-year--

Kryder: --between--

Bajorek: A 10-, 15-year period.

Kryder: --time period.

Bajorek: Before we move off that, you said early on in longitudinal recording the bit aspect ratio was about 30:1. I think it ended up below 10--

Kryder: Yeah, it ran around 6 or 7 to 1.

Bajorek: Six or seven to one so that was a major change.

Kryder: Yeah, a tremendous change. And the reason perpendicular recording enables you to go further to a large extent is the fact-- I mean the simple-minded thing is that you have a soft underlayer and you can double the head field. There are other aspects of it, but I think that's the biggest one. It really allows you to push it further, but the industry was busy pursuing a hundred gigabits per square inch using longitudinal recording.

It was around that time, actually in May of '97, Seagate approached me and asked me to come out for an interview, because they were looking for a new CTO. And I went out for an interview with no intention of accepting the job, but they were a major sponsor of the center, and you don't thumb your nose at a major sponsor of the center. So I went out for the interview and quite honestly, I was very up front about it. I told them I couldn't imagine why I would go join Seagate. I had a great position at Carnegie Mellon, and I felt like -- not that I wanted to, but that-- I could go to sleep at Carnegie Mellon for the next five years and nobody would bother me. So why would I want to go work at Seagate as CTO? I also pointed out to them that, although I'd always worked in magnetic data storage I'd always worked in research and it seemed to me that, if you're going to be CTO for a company like Seagate, you needed to know something about transitioning product from R&D into production, and I'd never done that in my life; I'd always worked in research. And so I made it very clear I wasn't interested, and I went back to Carnegie Mellon knowing that I wasn't going to get an offer and they understood that I didn't want an offer, and we were all happy, but while I was there I made the comment to Steve Luczo that the only thing that I could imagine would attract me to Seagate was if they decided they wanted to start a research division like IBM had in Almaden and that maybe that'd be something I might want to do. Well, that was May of '97. Then what happened is Seagate went ahead; they hired Tom Porter as CTO after they talked with me, and Tom had all the credentials that I said I didn't have, because Tom had worked in development at IBM, he'd done a lot of transferring of technology into production They made the right choice. And then what happened is that in January or February of '98 I get a call from Tom Porter. He was actually talking to Jim Williams at the time, my Associate Director there at CMU, and Jim asked him if he wanted to speak to me, and Tom said, "Yeah, let me talk to Mark." And he explained to me that he was interested in talking to me about starting a research division at Seagate. I honestly thought "oh, shit, I have to go out and do another interview." And so anyway after a lot of back and forth, their coming to Pittsburgh and my going out there and so forth and so on, I became convinced that they were really serious, they were really going to do it right, and so I joined Seagate as Senior Vice President of Research. I started from zero. I was the first employee of Seagate Research. We didn't have a building. Actually, part of the negotiation was where were we going to do this and—

Bajorek: That's a key factor, right, that surely influenced your decision--

Kryder: I wasn't willing and-- well, I was and I wasn't. It was sort of interesting because I talked with my wife, Sandy, who is a psychoanalyst and had her own private practice, about the job, and I knew where Seagate's facilities were. We sort of agreed that we didn't want to go to Minneapolis, but we thought well, Boulder isn't so bad; we like skiing and the Scotts Valley area isn't a bad area.. So, we thought well, it's doable and she was willing to give up her practice, but in the end, I sort of came to the conclusion I didn't want to leave CMU in the lurch, and I sort of felt like I had always been the guy going out, getting funding, doing everything at CMU in terms of driving the DSSC forward. I wasn't worried about the fact that there wasn't a competent faculty there, I knew they were a very good faculty who could do all the research and so forth, but any organization like that needs a leader and I didn't know what'd happen once I walked out the door.. I figured that if I stayed in Pittsburgh it wasn't that I could be Senior Vice President at Seagate and influence things at CMU in a big way, but on the other hand if CMU wanted to call and ask me, "What do you do about this or that?" that I could be available to reply and do things like that. So I'd sort of decided that-- less from a personal point of view, but more from the point of view that, having built an institution like the Data Storage Systems Center at Carnegie Mellon, I didn't want to see it blow up. So I decided I was going to force the thing to be in Pittsburgh or I wasn't going to do it, but what happened was when Tom Porter finally came to me with the offer, he had talked with Ed Skalko and some of the other people at Seagate, and they had already convinced him that it made sense to do it in Pittsburgh. And so, the reality is that Tom offered me the job and told me, at the same time, that we could do it in Pittsburgh.

So when I started we had no building and I was literally starting from zero. There was one guy (Jon Cave) whom they assigned to help me get it started, and he had actually just shut down another lab at Seagate. He'd been living in Arizona. He came in, and he was sort of the person who knew how Seagate operated. If you're going to start up a division like that, you have got to know how to order things; you have got to know how to get the IT people going; you have got to know how to do all this stuff. He knew the infrastructure and was able to do all this kind of stuff, so he was invaluable to me. We contracted an HR firm in New York City and they assigned somebody to me to be my HR person for a while until we could get somebody, and we just started hiring. Early on we got an HR person. My approach quite honestly from the beginning was that I wanted to hire an experienced research management team, but my view was the best people to do the actual research were new grads, bright, new Ph.D.'s, and the reason for that is that my observation was if you ask the average recording engineer how to do a factor of twenty-five to a hundred higher density than what you're doing today he'll give you five reasons why it can't be done. And the nice thing about a new grad student is he's sort of at the peak of his naïve view of how—

Bajorek: --enthusiasm--

Kryder: Right. He can do anything and you bring them in and they just do it. So what I was hoping to hire were key guys—

Bajorek: As managers.

Kryder: as managers who had been in research quite a while and knew how it worked, but populate the place with new Ph.D.'s, and that's basically what we did.

Bajorek: I was also curious. Were there any people within Seagate at that time that you could draft into the research division--

Kryder: There were a few that we tried to get but very few that we actually got over. There were almost none, so basically we hired everybody from the outside and most of them were new grads. Some of them had been post docs at NIST or something like that, things of that nature, but that was how we actually got the whole thing going. And I mean to be honest with you, of course, my experience in research was at IBM in companies and what I tried to do was adopt what I considered the best practices at IBM Research and to get rid of the things that I think didn't work at IBM Research, and it seemed to end up being a pretty good mix.

What we started doing from day one was we decided we were going to target a hundred gigabits per square inch, but we were going to do it with perpendicular recording in research, while I led a corporate wide program on a hundred gigabits per square inch. So research was doing perpendicular, the development divisions were doing longitudinal, and so we were sort of competing but not really; I mean it was just two approaches to trying to solve the same problem. So, from day one, August of '98, we started working on perpendicular recording at Seagate and by 2001 we had demonstrated nearly-- we didn't get to a hundred; we got to ninety gigabits per square inch using perpendicular recording.

Maybe a year prior to that some of the advanced development guys in the development division working for Nigel Macleod at the time wanted to work on perpendicular recording. We knew that perpendicular would go-- how far it would go and we knew how far longitudinal would go. We were quite confident of our numbers; we had good models. We knew what could be done-- what we could do with each one but the guys in the advanced development group wanted to work on perpendicular. I called Nigel up and I told him, "Listen. If your guys are going to start working on perpendicular, let me know because I will shift my whole effort to longitudinal because it's got more legs to go yet before it's time to switch over" and he shut them down, which was the right thing to do at the time because—

Bajorek: --corporate viewpoint--

Kryder: That's right and we continued working and got to the 90 gigabits per square inch and once we hit 90 gigabits per square inch at the spin stand level we didn't do our hundred, but I mean that just would have taken more time. We would have gotten there; it's just that we didn't quite have the head.. It would have taken another generation of heads basically and that wasn't fast so we decided okay, and I called up Nigel again and I said, “Nigel, now’s the time for you guys to get into it” and we produced 500 pages of documents on how it all came together and worked diligently for the next six months to a year transferring the technology into the development division.

One of the things I will say having gone through this process: The guys in the development division at first were glad to have our help, but probably a year after we had gotten out of perpendicular recording, if you would have talked to them they said, “Oh, we did it all. Research had nothing to do with it.” I’m not being critical because I actually think that that’s a natural thing and they did do a lot, I mean they- they’re the ones who got a lot of the bugs out and made things work. There’s a huge amount of work that has to be done beyond the first spin stand demo. And in fact, I learned in that process that you know you’ve done your job in research -- which includes tech transfer -- when the people you’re transferring it to now say, “Research didn’t do anything”. At that point you now know “I’ve done my job” and you’re free to go on and do other things, and that’s the way we treated it.

In early 2001, we had to figure out what we were going to do next, because I took the viewpoint from the beginning that if all we did was add a hundred and fifty to two hundred manpower people to Seagate, who has five thousand engineers anyway, I mean they may as well work in the development division. So there’s no sense in doing that. And that was why I let Nigel know “to keep your guys out of this” until we’re ready to transfer it. So once we had really begun transferring it we had to figure out what we were going to do next, and we had to do something different. So this was a time when Seagate was into trying to apply Six Sigma to everything they were doing. And so, we used Six Sigma approaches to trying to select our next set of projects, and we evaluated Heat-Assisted Magnetic Recording (HAMR), we evaluated probe recording, we evaluated patterned media recording and some others. We did this as a group, the entire group. We had a huge-- a meeting of all hands basically, everybody sitting around talking about the various technologies. Then we subdivided and different groups went off to address different technologies and then we weighted things like all the different application spaces, Mobile, PCs, Enterprise and what are they good for, and then we weighted characteristics like access time, what did we believe we could get to, which would have the best density, what would have data rate, what-- shock resistance, I mean all the parameters that you think of in doing magnetic recording. And we went through and did an analysis and figured out that well, it wasn’t a hundred percent clear, but we picked out HAMR, probe recording and patterned media recording and we went after those then in parallel with each other with subsets of the team working on each of them.

And along the way a guy, Bob Thibadeau, from Carnegie Mellon, started talking to me about the fact (Bob was out of the computer science department.) that he felt we really needed also to take a look at full-disk encryption. At that time, by the way, about every week you’d read in the newspaper that someone had lost a laptop that had 5000 Social Security numbers on it. So, one could see the need for encrypting the drive so somebody could not get into it if they stole your computer, and so we started a program also on full-disk encryption. We hired Bob; he had a patent on it and we hired him and came to an agreement with him on a reimbursement scheme for his patent and we started the program on full-disk encryption at that time.

Subsequently, then you have to ask yourself when did-- how did we manage to bring perpendicular to product within Seagate. Well, what happened is development took over in 2001, and it wasn't until roughly 2005 that we began sampling drives. 2006 was really high-volume production, and we moved to perpendicular across the whole product line as fast as we could manage to change products. I mean it was just any new product was going to be perpendicular after 2006. We did not choose to-- I'd been-- where did I hear-- yeah, it was-- well, I think I was still at IBM or maybe I heard about it at some of the advisory board meetings about when IBM was introducing new technologies they tried to quite often introduce them in-- certain products where—

Bajorek: It wasn't critical to the business.

Kryder: That's right. It wasn't critical to the business. Seagate didn't do that. Basically, we looked at it and we said-- the way Seagate operates on product development is that they have these core teams, and they'll put together a core team for a particular product and then they just develop it. It just happened to be that there was a core team available, their product didn't deliver, and so they chose some of those people, put them together and this was the next one up and it happened to be an Enterprise drive, and that's Seagate's bread and butter, and so it was a different approach but that was what they were trying to do. What we would do at Seagate was have quarterly technical reviews, and then they became semiannual, but at those meetings we'd have reports on all the different technologies, the heads, the media and so forth, but the way the decision was made to make the crossover was we did have teams working on perpendicular media, trying to improve signal-noise ratio. We had teams working on longitudinal, also measuring signal-to-noise ratio. We were plotting the signal-to-noise ratio in dB as a function of time--

Bajorek: For each of those approaches.

Kryder: for both of those approaches and you could look at it and you could see longitudinal was coming up with this slope, perpendicular was coming up with this slope, and you could look at it and say, "Well, gee, they're going to cross here. Oh, well, that's where we go with perpendicular" and that's what we did. That was the way we made the choice and that's why I say longitudinal could have gone further, there's no question, but it was going to take longer than if we switched over to perpendicular because perpendicular was there and ready to go.

Bajorek: I think it may be worth clarifying the importance of what signal-to-noise ratio means, right. It's sort of the bread and butter of a recording system, right.

Kryder: Yes.

Bajorek: It determines the reliability of the data, right.

Kryder: Yes, absolutely. No. I—

Bajorek: You might want to expand a little bit about that.

Kryder: Well, signal-to-noise ratio is sort of everything in the drive. I mean if you have the signal-to-noise ratio, your servo works, the signal processing works, the data rate can be cranked up, so forth and so on, but if you don't meet the requirements then you aren't going to make it. So, it is the key criterion that one is usually struggling to get and it's a function of heads and of media, okay, both in terms of what you're looking at but that's fundamental to being able to take your density to a higher level.

Bajorek: And it probably is a key competitive edge. The company that can deliver the best signal-to-noise ratio, if it does everything else right delivers the best product.

Kryder: Absolutely. You can spend it either way you want to. You can take higher density, okay, which obviously gets you a bonus, but you can also spend it on reliability and either one gets you a competitive edge absolutely, no question about it. So that—

Bajorek: And so you must have predicted that that crossover would have occurred around 2005, 2006.

Kryder: That's exactly right.

Bajorek: That's what drove--

Kryder: That's right.

Bajorek: --then follow through. The products showed up in volume in 2006.

Kryder: Yeah, 2006, and it was the smoothest transition. Actually I think you had suggested I might try to address what one of the key issues was in trying to bring perpendicular together. I mean yeah, I guess I could say signal-to-noise ratio, but the reality is that if I'm thinking about the perpendicular program it wasn't one thing. It was getting everything running and it was the integration, it was the teamwork, it was people working together to do the whole thing, and it turned out to be an amazingly easy transition. I mean I'll have to say that compared to a lot of technological changes that one went very, very smoothly.

Bajorek: And I think in the context of the industry that the Seagate team was the one that was either first or one of the first--

Kryder: Yes. No. In volume production, Seagate was the first; there's no question about it. Toshiba brought out a perpendicular drive and put it in a product, a not very good product, a Walkman-style music recorder, and I don't mean to put them down, but that was what it was used in. Seagate brought out-- as soon as they brought out their high-volume drives, like I said, they just started putting it clear across the product line.

Bajorek: --Toshiba chose the conventional approach of-- may have chosen-- of not betting the whole business, trying it in a low-risk product. I think you guys at Seagate chose to bet the business on it and that was gutsy.

Kryder: Yeah, and it paid off well and the products showed that to us 'cause we'd had drives for a long time and we were sampling them, and it was working; there wasn't any question about it.

Bajorek: A lot of this was now being done by development. You stayed connected with it through these product reviews?

Kryder: Well, yes, that's correct -- all the technical reviews I was part of -- So yes, absolutely, yeah.

Bajorek: You mentioned you had this three-horse race, HAMR, probe recording and patterned media recording. How did that sort itself out or did it--

Kryder: It sorted out-- itself out very well. What happened was that we worked on those three. Now you got to realize that we started this in 2001, So in that timeframe the Department of Commerce still had their ATP program, Advanced Technology Program and would fund industrial projects that, you know, were high-risk. And- and so we in Seagate Research decided to go after one of these and got funding for it to support HAMR or Heat Assisted Magnetic Recording. They would match funds that Seagate put in up to a certain limit. And we involved the universities in that-- Carnegie Mellon, University of Minnesota. University of Arizona were part of it. Various universities as well as ourselves doing this advanced program. And as I saw at IBM, which IBM did on occasion -- would get outside funding like this, they would usually leverage it into a larger program, and we were able to do that at Seagate too. I don't, you know -- to be honest with you, had we not had the ATP funding I could have had a HAMR program, but it wouldn't have been as successful as ….you know, as it was. It wouldn't have ever scaled up the way it did. Because of the government funding we were able to pursue it very aggressively.

About two years into the program, so maybe 2003, 2004 timeframe, we-- I looked at it and oh, actually that's right. There was-- there was a thing, which triggered the review. And that is that this was a time when Seagate was getting pinched in their margins. And the reason was, and this would've been at, I think, it was when we were at 60 gigabyte drives, if I'm not mistaken, somewhere in that range and timeframe. And what had happened is that prior to that if you brought out a new drive at higher density, of course, than the previous generation, the OEMs were always willing to engineer it into their new product. But at this particular time Michael Dell and others in the industry, were only willing to move it in if it was actually at a lower cost than the previous drive. And, you know, Seagate had hired me because they realized that they needed to be a technology leader. Their model had been to be a low-cost producer. With the areal density going up so fast, when they were late by three months they figured they'd lost a billion dollars in sales and that they couldn't be late anymore. And so that was why they hired me to be Senior Vice President of Research was that they wanted a research division so that they could be a technology leader. And, you know, Tom Porter, in order to create research had closed down all of their work on mobile drives. He just closed down the entire factories, because without having the areal density leadership, there wasn't any use in being in mobile.

Bajorek: Right.

Kryder: And so, you know, they hired me specifically to be able to do that. And we did get the areal density lead and got back in the mobile space and so forth. So, the-- I lost my train of thought--

Bajorek: Well, yeah, I think you're--

Kryder: -- for that.

Bajorek: Seagate was at a pinch point.

Kryder: Ah, yes. Seagate was at a pinch point-- and what had happened was we always, you know, we-we were making-- we had to make money on the front end now that we're doing research.

Bajorek: Yeah, yeah. But I think you need to come back and clarify that--

Kryder: Okay. Okay.

Bajorek: -- why was a pinch. I think you're-- I think you have to make the point that--

Kryder: Okay. The pinch point--

Bajorek: -- the customers.

Kryder: The customers suddenly said--

Bajorek: Changed their modus operandi.

Kryder: Exactly-- that's right. The customers suddenly were not willing to pay for the highest, you know, the new high-density product, it had to be a lower cost than the previous one. And the net result was, instead of having a six-month product cycle, the thing lasted for a year or so. And that really killed Seagate because they were suddenly -- you know, they made money off the front end by a having a higher price and paying for things. Now they were competing on a long timescale and it let everyone else -- gave everyone else a chance to have a breather. So, what happened is Seagate was pinched and we got-- went into a mode of internal, you know, product review in a major way and took some cutbacks. Always before I'd been protected from these cutbacks and was actually allowed to continue hiring. This time it was across the board. And so, you know, we just all agreed we'd take a five-- I don't remember, I think it was five percent cut in headcount. And so, my attitude was if I'm going to be asked to cut five percent, I'm not going to cut five percent out of the thing across the program. I'm-- my attitude is I'll take rifle shots. I won't use a shotgun, but I'm going to figure out--

Bajorek: Which programs to kill.

Kryder: -- which programs to kill. And at that time I killed bit-patterned media at Seagate. And it was I think the right decision.

Bajorek: Yeah, but in hindsight.

Kryder: And the reason is I could not fathom how one could make that a low-cost technology. I mean it -- to me it was not going to do it. Dieter Weller was there at the time and Dieter Weller agreed with me. Okay. Now subsequently, Dieter went out and worked in- in--he left-- actually, his wife wanted to go back to Germany. And I talked him into going back out to San Jose and, which his wife was willing to do, and he went to work in the media division out there. And they then initiated the program.

Bajorek: On Patterned media?

Kryder: On patterned media. Though, Dieter agreed with me that it should be killed at research before that. But that's more of a political thing within Seagate at the time by another-- the head of the media division. And so, we in research continued pursuing HAMR and we continued working on Probe. I think the CEO was honestly quite interested in Probe technology. The CEO at that time was Bill Watkins and was quite interested in trying to make something more akin to a solid-state type drive technology. I honestly did not sell Probe hard, because I saw it as further out than HAMR. But I, you know you have got to evaluate it, see whether it makes sense or not. And we had a different approach to doing it than what IBM had done with their Millipede project. And we'd gone over and talked to IBM too about theirs and, you know, had a good exchange and so forth. So we kept those two programs going for quite a while. But by the time I left we had killed the Probe program and HAMR was up at, you know, closing in on a terabit per square inch on spin-stand density demos. HAMR today is still not in product. Of course, what happened at Seagate, by the way, was that we got through that crunch time with the- the cut, and then I retired from Seagate in 2007, early 2007. 2008 occurred with the recession.

Bajorek: With the recession.

Kryder: And Seagate closed down Seagate Research in 2009, which I think was a real shame, to be honest. And Steve Luczo, I'm told said that, had Bill Watkins been let go six months sooner he wouldn't have closed it down. But it was already closing by the time Luczo took over again.

Bajorek: Fate accompli by then.

Kryder: And-- that's right. And the reason I think it was such a shame is that the only reason HAMR got off the ground is because of the fact that we had heads, we had media, we had the optics of the near-field optical guys, we had signal processing, we had servo, we had it all in a team of a hundred or so people and different groups addressing all those issues. You could tackle a technology like that in that sort of mode somewhat uniquely. Because in a HAMR drive the head is-- I mean the media is part of the head. I mean it really is because it's got to have optical coatings on it; it's got to have thermal stuff to handle those things.

Bajorek: There's no silver bullet. You have to deliver all of those building blocks.

Kryder: Exactly. And you can't treat them independently. I mean you can't go off and work on one part of it in one place and another part somewhere else and expect them to work together. You want the inventions to happen. You got to have people banging their heads together on a-- on a daily basis. And I think, you know, it probably cost Seagate a couple of years in- in bringing out HAMR. So, you know, I don't know when HAMR will be out for sure. I am told that--

Bajorek: But it seems to be this--

Kryder: I am told that Seagate is--

Bajorek: It is being worked on, right?

Kryder: Oh, yes, it's being worked. Moreover, I'm told that Seagate is sampling a few drives. And the claim is that they'll probably have it in product by 2018. To me that's credible, mainly because I know something about how Seagate operates, and Seagate has a three-year product plan. And that product planning, that three-year planning process, basically when a product is put into that it, you know, 90 something percent of those come out the other end on time. And, you know, that doesn't say all of them do, but most of them do. So, you know, if they-- when they're saying it's three years away, I say oh, they don't know when this thing is coming out. But when they say it's a year away, I suddenly begin to think ah, yeah--

Bajorek: It may actually happen.

Kryder: -- beginning of the-- yeah, they may actually make it happen. So, you know, that's the situation. And full disk encryption, of course, that actually went into product while I was still there in 2007 and, you know, they continued--

Bajorek: And how did it evolve from Pittsburgh, the research center then to development?

Kryder: They worked very closely with the drive development divisions. And Bob Thibadeau was key at making, you know, making the whole thing happen. But that's all been taken over by the drive guys and, you know, that was a very successful program also for the company, it's no doubt about.

Bajorek: I'm not very familiar with the- that method of encrypting. But is it now used across all drives by Seagate in this?

Kryder: You can get it in all drives, but you don't have to get it.

Bajorek: Yes. So it's an option.

Kryder: It's an option. And it's used very extensively. The NSA insists that that, you know, you're going to work on-- you use--

Bajorek: The drives are used?

Kryder: That's right. The government uses a lot of the full disk encrypted drives. Basically, it protects the data at rest and, you know, it's also good in cloud systems because you can have a single drive with data from multiple customers on it. And you can ensure that Customer A is not going to be able to see Customer B's data because he's not -- just doesn't have the password to--

Bajorek: To access data.

Kryder: -- to access.

Bajorek: Very interesting.

Kryder: It's just public private keys and the data is protected. And, you know, you can set it up so that there's two passwords. You can give one to your IT guy so he can break it if the individual forgets.

Bajorek: Who owns the data forgets or somehow.

Kryder: Right.

Bajorek: Yeah.

Kryder: And that was another successful product that came out.

Bajorek: And the timeframe in which it was actually commercialized, roughly?

Kryder: I would say around 2006, seven, eight, that timeframe. Started coming into-- first it was introduced in mobile drives, but now it's being used a lot in enterprise drives.

Bajorek: Well, congratulations on that series of transfers, right? And which are all also in a way rooted at CMU.

Kryder: No, they are. Very definitely. The HAMR program without question came out of CMU. I mean that's again, in a way, it's rooted clear back to Caltech because high-speed laser--

Bajorek: Yeah.

Kryder: -- and then doing small spot. I wrote a paper with Eric Betzig, and Eric Betzig is a recent Nobel Prize winner in chemistry, interestingly, for his work on near-field optical microscope. And I wrote a paper with him in about, I don't know, it would've been '80s somewhere, in which we did near-field magneto optical recording at an extremely high density at the time. I don't remember what it was, but it was, you know, an incredibly high density. In reality, It wasn't commercializeable because you couldn't possibly-- the head was so inefficient that--

Bajorek: Yes.

Kryder: -- that you couldn't get enough power down it to do what you need to do. But that knowledge, the fact that you could make a near-field fiber that would produce a 10 or so nanometer size spot is a big reason why we had the HAMR program. Because once I recognized--

Bajorek: Yeah, gave you the confidence. Yeah.

Kryder: I suddenly said, well, wait a minute, we got to find a more efficient transducer and hired some smart guys who knew something about doing that and that gave life to the HAMR program. So, HAMR evolved out of, really, out of CMU. Perpendicular certainly came out of CMU to a large extent, then-- and then the INSIC program.

Bajorek: And I would-- I would say, look, I mean if you really--

Kryder: And full disc encryption

Bajorek: But if you really look at it, right, the prerequisite was to have that center, a joint--

Kryder: Yeah.

Bajorek: -- industry academia--

Kryder: Yeah, absolutely.

Bajorek: -- government center, right?

Kryder: Absolutely.

Bajorek: Because that laid the foundation.

Kryder: Yeah.

Bajorek: Without that foundation, none of this-- none of these other events could have followed.

Kryder: The other thing is that the CMU contribution, which shouldn't be overlooked, is just the number of students who passed through the place.

Bajorek: Yeah, I was going to ask you--

Kryder: When I was there, we had about a hundred grad students, typically. So, you can just do the math and you can figure out that with a five-year lifetime for a PhD we were graduating, you know, 20 grad students a year, PhDs, and most of them went to work in the industry. And today's leaders in the industry, a large percentage of them came out of CMU.

Bajorek: Yeah, I would characterize that a little bit more in the sense that prior to the existence of these academic centers, the industry hired engineers.

Kryder: Yeah.

Bajorek: But they--

Kryder: And then retrained them.

Bajorek: -- came in knowing nothing--

Kryder: Right.

Bajorek: -- about magnetic recording.

Kryder: Right.

Bajorek: And then they would have to train them.

Kryder: Right.

Bajorek: And that added, right, to their ability to contribute?

Kryder: Right.

Bajorek: Delayed.

Kryder: Yup.

Bajorek: Whereas by training them in these centers, they could--

Kryder: They land on-- they landed on their feet.

Bajorek: -- start contributing much earlier.

Kryder: Yup, landed on their feet running. And they also, I mean if they came through CMU, the thing about-- and this was the advantage of the NSF funding, by the way. The NSF really wanted a systems approach. And under the Magnetics Technology Center, prior to the NSF, at that transition we renamed the center and it became the Data Storage System Center, but prior to that it was the Magnetics Technology Center. And under the Magnetics Technology Center basically the companies sort of told us what they wanted us to be doing. And the truth is the companies weren't as wise about that as they could have been, because they all have their hot buttons and they tried to direct you toward whatever they happened to see as their pet project. Now, I was sort of stubborn and did some, you know, took some of the money and said oh, hell with them. I'm going to do this any way. And, you know, I can remember, I mean one project that came out of that was what Mark Re did, which was build that high-speed scanning Kerr effect microscope, which Kodak, IBM copied, and then Phase Metrics turned into a product and then sold to most companies in the industry. That product probably wouldn't have happened had I listened solely to the industry. So, I knew that I had to try to do some of these things on my own. But the thing that the NSF money allowed us to do was not just be a magnetics center, but rather they really wanted us to take a system approach. And it is that system approach, okay, having signal processing, having servos, and tribology, doing the whole thing, not packaging but everything up to spin stand level and a little beyond. Taking that systems approach-- without that we would never had the José Moura-Alek Kavcic patent. Okay? I'm the one who got José involved in the center and tackling these problems and he learned a lot from the industry. He had prior-students -- prior to Alek Kavcic. He was trying to do multiple read heads so that you can, you know, get rid of the intertrack-interference by- by doing--

Bajorek: Parallel, the reading.

Kryder: -- parallel reading and things of that nature. But, you know, in the course of doing all those projects he gets smarter and smarter and figures out what can really work. And then the result was the algorithm that, you know, Marvell eventually utilized in their product and obviously had a big impact on the industry. And so, the NSF, funding enabled us to do that. And that's the other characteristic I think that most people talk about, about the CMU grads is that they came out, you know, you might have hired somebody to work on media but he understood why signal and noise was important and what it was and so forth. And if he hired somebody to work on signal processing, he had an idea of what it was in the media that was causing the problem. It wasn't just a noise background--

Bajorek: Right.

Kryder: -- that he was dealing with. He could-- he could interact and talk with the media people or the head people or whatever, and to be able to actually understand what was going on. And I think that- that was a key contribution that came out of it.

Bajorek: There were other patents, right, that I remember there were some media patents?

Kryder: Yup. Yup.

Bajorek: That became--

Kryder: They--

Bajorek: -- in the industry.

Kryder: Dave Lambeth and Dave Laughlin developed the nickel aluminum under layer for- for glass substrates--

Bajorek: That enabled some of the early mobile drives.

Kryder: Absolutely. Absolutely.

Bajorek: So, but coming back to the Moura-Kavcic invention, I think in the end, CMU was able to get what may have been the highest royalty revenue case involving academia, right, out of--

Kryder: Could be. I don't know. It may be. I wouldn't be surprised.

Bajorek: It is. Can you share with us what was the--

Kryder: Well, I think- I think it was something like-- so I don't know the exact numbers. But the total amount was about 750 million dollars.

Bajorek: That's what I remember.

Kryder: And 250 of that I think went to the attorneys.

Bajorek: Yup.

Kryder: And, you know, Moura and Kavcic got half of the remainder, and the rest of it went to the university.

Bajorek: Yeah, I had read that there were larger royalty cases in-industry, particularly among pharmaceutical companies.

Kryder: Oh, yeah.

Bajorek: But that this was the highest ever involving an aca--

Kryder: I didn't know that.

Bajorek: -- from academia.

Kryder: But I wouldn't be surprised.

Bajorek: That's what I heard.

Kryder: Yeah.

Bajorek: And--

Kryder: Well, it's a big number. That's for sure.

Bajorek: But you should be proud of the fact that's- that's one other measure--

Kryder: Yeah. Yeah.

Bajorek: -- of the value of what that- the center created over time. Well, let me--as you now integrate what you just covered for us, can you give us some examples of what were some of the most difficult challenges you had to--

Kryder: Sure.

Bajorek: -- overcome and in across the gamut of your assignments. You know--

Kryder: Okay. Well--

Bajorek: -- it would be fun to share that.

Kryder: -- To be honest with you, Chris, you're not going to get the answer I think you expect. What I'm going to tell you -- although I know you know this -- is that the technical problems were all rather straightforward. And I mean, yeah, there were challenges. No question about it. The technical problems, I mean, you know, "how do you build a near-field head?" You need something drastically more efficient than what we had, and those are tough technical issues. But, you know, you get clever people, you put them to work on it. And you keep prodding things and asking questions and assemble a team of people to make things happen. The hard, hard part in these things are the challenges of the personnel issues -- getting things running right. My argument is that I think the place where I've been able probably to make more of a contribution -- and I have, I don't know, 400 papers and, you know, 25 or 30 patents or something like that. So, I've made my share of the technical contributions but--but that isn't the issue. The key thing I think is being able to get -- facilitate the team really working together and addressing these issues. And I think that's where industry quite often falls down. I actually think that that's a distinguishing feature of companies who can run a research and development operation and the ones that cannot. We all know that there's a lot less R&D done today or a lot less R done today, for sure, in industry than there was 30 years ago or 40 years ago when you and I were at IBM together. I think that part of the reason is they really don't know how to run those programs and how do you encourage people to work together to solve these problems. I can remember so many times talking to all the people at Seagate Research ,when we would have some cutbacks at the company, I'd have an all-hands meeting and whether it was going to affect somebody there or not -- they would be, of course, talking about it.

Bajorek: Yes.

Kryder: -- I guess in a company, they smell the tough times and everybody is worrying about it. They're spending all their time worrying about it. I'd have an all-hands meeting, lay it all on table and say, “look, the last thing I want is you guys spending your time talking about this crap”. It's, It's just it’s useless. Get it through your head that what I want you doing is when you're taking a shower, I want you to be thinking about the problems we got to solve here. And- and try to get your enthusiasm, talk to each other. We designed the building at Seagate so that people would bump into each other. I wasn't as clever as Steve Jobs in locating the heads in the atrium area. But I did put in an atrium with the common steps all the way up and down, because I'd observed that at the Alcoa headquarters in Pittsburgh and thought that's a very clever way of, you know--

Bajorek: Stimulating--

Kryder: -- getting people to interact and make things happen. Steve Jobs had done the same thing. I learned that by reading the biography on him, but he was even more clever. He put the heads into that same area, because that's where everybody goes all the time. But creating, figuring out how to get teams of people to work well together was a challenge and, you know, we had our- our problems. There were situations where, you know, people would be discontent with their manager or whatever, and I operated on an open-door policy. People could come in and talk to me. I didn't encourage it because I think what you want to do is help get the managers talking with their employees. But if somebody wanted to come in and talk, because they really had a problem, then I wanted to talk with them and then, you know, then my approach was to hear them out. I'd talk to their manager individually and then I'd bring them both together and say, okay, this is what I hear from you, this is what I hear from you. You know, they-- usually they're- they each have different viewpoints but they probably haven't told each other. And what I found was, quite often in that situation, if you just, you know, listen to this guy, listen to this guy and then you put them together, you know, and point out to them, well this guy says this and this one says this, all at once you start getting some good communication--

Bajorek: Communication.

Kryder: -- and things happen. And so, creating-- my biggest challenges were always how do you create the environment where people were enthusiastic about their job and- and would interact

Bajorek: Willing to cooperate with each other.

Kryder: -- in order to make things happen. That's right.

Bajorek: Another aspect of your experience that you might be able to share with us, what are some of the- your favorite stories from the industry or-- from your experience. Again--

Kryder: Well--

Bajorek: -- are any of them humorous or can share something with us?

Kryder: Well, I mean to be honest with you, I thought about that. I mean you and I had talked a little bit about some questions in advance and I'd seen that one. I honestly can't remember something specific to myself or my own experience. The thing that's what I remember mostly that were humorous about the industry were the characters that existed in the industry back in its early days, I mean Al Shugart, there are so many stories about Al Shugart that are absolutely incredible. And, you know, he ran his dog for Congress and -- I think he came close to getting elected.

Bajorek: Actually got quite a few votes.

Kryder: Right <laughs>. Right <laughs>. I--

Bajorek: I remember that.

Kryder: You know, there- there, you know, those are the- I mean it's there are more personalities than anything else than any one specific comment.

Bajorek: And you may have covered this earlier on in your comments. But were there some people who stand out in your memory as having the most influence in your decision-making--

Kryder: Well--

Bajorek: -- career choices?

Kryder: Sure. Sure. Well, I mean my advisor, Floyd Humphrey, clearly had a huge influence. Floyd was, like I said, sort of a hands-off guy to some extent. He challenged you. He'd ask you questions and so forth. He was, you know, a great guy to interact with. So, I always highly valued him, because, he was that way. He taught me how to give talks and he taught me how to write and he was good at that, good at teaching it. And I've tried to do that with all my students in the same mode. And to some extent he's the one who motivated me to have the view that I wanted to go into academia. He didn't try to sell me on that. It was just, more or less, what I observed, you know, him and he had valuable experiences at Bell Labs, but he was teaching now, and I liked that. That was a nice model.

Bajorek: He was a role model--

Kryder: That's right.

Bajorek: -- in that regard.

Kryder: That's right. I mean quite honestly, Chris, if I think back over time of people who've made an impact in the industry, too, I would have-- and influenced me, another one is Dave Thompson. And Dave-- and you did also in the early days when we were getting the center going. It was really as far as I could tell, you and Dave who managed to make this whole thing happen. Without that the MTC wouldn't have existed. And without the MTC we probably wouldn't have the DSSC. And we, you know, all of these things that have occurred wouldn't have happened. Dave probably had more of an influence than you because Dave was continually-- you went off to Komag and Dave was continually the representative for IBM at the MTC, DSSC right up through his retirement and could always be counted on to provide a, you know -- the best technical, you know, inputs we could get on, you know, and explaining. He knew how far. He knew what you- what he could say and what he couldn't say from IBM's point of view and maybe he stretched it sometimes, I don't know. But it always, he was a channel of information and--

Bajorek: A good advisor.

Kryder: -- and a very good advisor.

Bajorek: I think he had another dimension though. He was a CMU--

Kryder: Yeah. Oh, that's true, too. Yeah, he was a CMU graduate.

Bajorek: CMU had a special--

Kryder: Yes.

Bajorek: -- was a special place--

Kryder: Yes. No, that's--

Bajorek: -- in his thinking--

Kryder: -- that's absolutely right. And then another one has to be Tom Porter, because I wouldn't have gone off to join Seagate without Tom Porter. And I mean, you know, Tom, I mean quite honestly Tom had- I mean he was used to the IBM model, right? He knows development. He knows advanced development and he knows research, even though he had never worked in research, and he understood that model. And there isn't any doubt, but that that was what he tried to set up at Seagate when- when he came there and was told to try to make it become the, you know, a technology leader. And he, you know, hired- first put Nigel *MacLeod* in charge of Adcon, okay, the--

Bajorek: The Adtech.

Kryder: Adtech effort, and- and it was called the Advanced Concepts Lab--

Bajorek: Yes.

Kryder: -- there in Seagate. And then he hired me to put research in place. And he, you know, so he was using the IBM model and had the vision for going ahead and doing it. So, I have to give him a lot of credit. And, by the way, I made the point that Seagate first interviewed me to be CTO and I, of course, turned down the job. And then Tom hired me to be Senior Vice President of Research, which is what I wanted to do. But then Tom retired in 2003 and, by that time, I felt comfortable taking over the CTO's job and which is what happened.

Bajorek: Very nice. Can you share with us any- the special honors and awards you received in recognition of your contributions?

Kryder: <laughs>

Bajorek: And give us some idea of- of those honors.

Kryder: Yeah, there's a lot of them. More than perhaps I should have deserved. But yeah, I was-- let's see. I got the- the IEEE Distinguished Lecturer twice. And I don't think there are very many people who've done that. One of them, I was doing magneto-optic recording. The other one, I think I was doing ultra-high-density recording, and then I was made an IEEE Fellow, American Physical Society Fellow. I got the Pake Prize from the American Physical Society. Like you I got one of the Millennium Medals from the IEEE. I got the Reynold B. Johnson Information Storage Award. Another thing that I did, by the way, was that, shortly after we got the Data Storage Systems Center, Singapore wanted to start a center in the field of magnetic recording because that was for a time I think the largest outside industry that they had; it was roughly 20 percent of their GNP for a period of time. And they realized that Malaysia and China and so forth were going to become lower-cost places to produce drives, and they wouldn't have this forever unless they could manage to keep a knowledge base in Singapore that would attract the industry. And so, they asked me to help them start up what ultimately became the Data Storage Institute, but it started out as the Magnetics Technology Centre also named after ours except that of course they spelled “Center” differently but “R-E” instead of “E-R” <laughs>, but otherwise-- and so I worked with them for-- I don’t know-- at least a decade and did get a public service medal from the president of Singapore for my contributions there.

Bajorek: How nice.

Kryder: And then most recently in 2014 the Franklin Institute in Philadelphia awarded Professor Iwasaki and me the Franklin Medal for having developed perpendicular recording, and the Franklin Institute expects not only outstanding science but also commercialization when they talk about these things and so in a way it was the combination. And Iwasaki, I have to say, did far more on perpendicular recording fundamentals than I did, but on the other hand, I think that I had a lot to do with bringing perpendicular recording to market and ultimately proving that in fact it did work. An interesting comment by the way is that Iwasaki, I think, would say that we should have switched sooner to perpendicular recording, because he brought out perpendicular recording in 1978, but we didn’t turn it into product until 2005, 2006. And the thing is I actually don’t agree with him on that. I think we did it-- at the time it made sense. There was no need to do perpendicular recording when the industry looked at it, and the industry did look at it very hard in the ‘80s, but in the ‘80s the issue for getting higher density on hard drives was how do you get lower head-media spacing, and the perpendicular media are rougher than longitudinal media. There was no advantage at that timeframe to going to perpendicular recording. What ultimately changed that was reaching close to the superparamagnetic limit in longitudinal recording, and once we reached that, then perpendicular recording was available and made it possible to go further. So, I think we did the transition at the time when it needed to be done and that we did it about as well as we possibly could have done. I don’t think we could have managed to do it much sooner than we did under any circumstance.

Bajorek: I imagine that the underpinnings like -- you needed film media. It would have been very difficult to do with particulate media.

Kryder: Oh, absolutely.

Bajorek: So the other key building blocks that had to be--

Kryder: Yeah, and—

Bajorek: --matured, that had to mature in order to be able to--

Kryder: That's right. We needed the oxide media, okay. The early work on perpendicular recording-- and actually it's sort-- it was surprising to me but I can remember listening-- learning about the oxide media and that to us was a real breakthrough because with cobalt chrome you had a nucleation field which was--

Bajorek: Inadequate.

Kryder: --too low, it was inadequate, and as a result you were always going to have noise-- a tremendous amount of bit shift type noise on your disk, and -- one of Iwasaki's compatriots in perpendicular recording really didn't understand that; he really didn't. I can remember in a discussion at one of the perpendicular recording conferences where he just didn't believe that you had to have a nucleation field that-- he didn't think it mattered that it was still going up when you were at zero remanence -- I mean at zero field. And it was only the oxide media which made that possible, and once that occurred then all the other things could come together and make things happen.

Bajorek: I understand one of the latest courses you taught. By the way, have you ever retired from CMU?

Kryder: Yes, August 31st I retired of last year.

Bajorek: I think before you retired you put together a course on leading and managing R&D. Could you share some of the golden nuggets--

Kryder: Yeah. It's a course that actually when I went back to CMU in 2000-- I guess it was 2008 actually when I headed back to CMU. I took some time off, between Seagate and CMU, and the-- or wait a minute was it-- no, it was 2007; it was fall 2007, yeah. I retired in February. Fall 2007 I went back to CMU and I was taken aback when I went in and talked with Ed Schlesinger, who was then the head of the department, and he looked at me and he said, "While you were gone the last nine years to Seagate, we instituted a program in the engineering college on management of technology and innovation." And he said, "You have been in Seagate as CTO for this period of time and I'd like you to teach a course on how R&D is done in the industry," and that really threw me for a loop to be honest with you. In an Engineering class usually you get up there, you write down Maxwell's equations or whatnot and explain what's going on. Occasionally, somebody asks a question, not very often but occasionally somebody asks a question, you answer the question, and you go on, continue your lecture, and I was used to giving those kinds of lectures, but I couldn't profess to know how to do R&-- manage R&D. I had never had a course -- except for what IBM puts you through -- a first-level manager's course for a week. I'd never had a course in management. I had a lot of anecdotal experience, but to teach that course -- to me, the only way I could imagine doing it would be to run it as sort of a wide-open-discussion sort of course and that- that's not only material I didn't necessarily know, but it's also a style of teaching which is very different than anything that I had done before. But anyway he said, “Don’t worry. We’ll get help for you.” And I met three times during that semester with about-- I don’t know-- a half dozen to a dozen faculty, some from the Heinz school, which is the School of Urban and Public Affairs, the business school, some from engineering and public policy, various people who taught courses of that ilk at CMU.

And the first time I met with them I just wrote down a random list of topics that I thought I could-- I could see the need for the course, okay, because I’d just hired a couple hundred Ph.D.’s, all right, who were all mostly new Ph.D.’s and they were some of the brightest technical minds that you could get, but these guys didn’t have the foggiest idea of how a company operated or what marketing was for or that it mattered-- they tend to think well, gee, if it’s a gadget that I’d be interested in why in the hell doesn’t the company want to make it, never mind that it’s a consumer product and your company only markets to OEMs or something; I mean they don’t understand these things. And so, I could see the need for this sort of a course and so I just wrote down a list of all the things that I thought might be of use to the students, and then I met with this group of faculty and they made comments about the list, but they also asked about other things. I mean one of them had a hot button about globalization and I hadn’t put anything in there on globalization, but of course I knew a lot about globalization.

Bajorek: You had lived it.

Kryder: Right, and so the next time then I decided well, I have got to try to organize this into a course; I’ve got to-- I can’t just have a random list. And so, I spent time trying to put together this-- to organize it so it was a course, but I couldn’t figure out “what do I teach first?”, and I finally decided well, I had an experience while I was at Seagate, and I started from zero building up Seagate Research. I had to go out and find a location, I had to hire the HR people, I had to put the IT support in place, I had to get-- do everything, and why don’t I just go through this in sort of the chronological order that I did things. And some of them wouldn’t have been logical necessarily, but I could move them around if it made more sense somewhere else. So I structured that one basically in that fashion, and they had a few more suggestions, but by that time I was getting zeroed in on what it was. And they also made the comment, the other faculty did, they said, “You know, you say you don’t know this material; you haven’t ever had a course in it.” They said, “That isn’t an issue.” They said, “We know all the books and we can steer you to all the books. In fact, we can give you a TA --- who can tell you where all the books are and so forth and so on. The trouble is we don’t know whether any of the books are any good or whether they work or what’s going on so what you know is—

Bajorek: What works.

Kryder: ---what works.” And so, I went through and structured it on that basis. So basically the course was a list of all the topics, sort of the chronology. It’s a case study of what I did at Seagate but with a lot of stuff brought in from Harvard Business Review cases or others as well, Stanford and so forth, business cases which I could throw in to try to say, “Okay. Here’s an example of what I did. Here’s somebody facing a similar situation and he did it differently or maybe he did it the same or whatnot, why are they different, so forth, so on,” and that’s the kind of stuff we’d talk about. So that really was a very fun course to teach, and I learned a lot from it myself, but I think the students who took it benefited in quite a big way too.

Bajorek: How many semesters were you able to--

Kryder: I did that about five or six years.

Bajorek: Oh, very good.

Kryder: Yeah. It was continually well subscribed to and a lot of students taking it.

So for some of the nuggets that I learned through this whole experience -- some of it is industry and some of it is teaching, there are a lot of different things, but one that I-- actually I learned this one-- I can remember Floyd Humphrey taking this point of view, that if what you really want to do is good research then you ought to go to industry, because -- industry will support you, and they have all the capabilities of doing things. That may be a little less true today than it was when you and I were grad students at Caltech, but I still think it’s basically true. If your motivation is really solely the research, then go work in industry. That isn’t a reason to go to academia. You ought to be going to academia because you really enjoy teaching and teaching doesn’t mean just courses; it means teaching grad students who are doing research who are learning to do stuff, and so I think that’s a very important thing that I look at.

And I’ve heard this from another professor, but I firmly believe it and that is the secret to being a good professor is to have students who are smarter than you, and you have got to have the very best students there, and fortunately CMU has good students and you don’t need a whip to get them going. They are highly motivated to begin with. They want to break down barriers. They want to do new things. They really are motivated on their own.

The other thing is-- and I made reference to this earlier with regard to universities-- I put it in the context of run it like a business, but a big part of that is value your products correctly. And I think there is-- I mean yeah, industry might not like to hear me saying that I think academics quite often sell their wares too cheaply, but I’m not sure that industry wouldn’t be ahead if they didn’t have more academics who really expected to get paid appropriately for what they’re actually delivering and then take it as seriously as I did to try to deliver it, and the industry would be better off if all of those pieces came together like that.

And then in terms of-- and this applies whether it's academia or industry but I talked about this earlier -- assemble a good team and then teach them how to be-- how to work together. That's where I think my strength was more than anything else. It was not my technical contributions. Again, I'm proud of my technical contributions, but the things that I'm most proud of are the people who've worked for me and have gone on to do things that I didn't necessarily visualize or create or anything else, but were given the opportunity and so I somehow played a role in facilitating their being able to break down barriers that wouldn't have happened otherwise.

Don't be afraid to fail;-I mean that's one thing. I mean I can remember discussions with Dave Wickersham at Seagate and what he wanted to try to measure Research by was what percentage of products-- or what percentage of projects end up being successful in a certain period of time, and my measure would be that my God, if more than 30 percent of my projects are being successful, I'm really screwing up because I'm not doing research at that point. I mean-- and Dave of course is an operations guy and if you're trying to build drives then yeah, you want—certainty.

Kryder: --but that isn't what you should be doing in research. If I really squeezed that down, then you don't need research; you better just take all these people and absorb them into development and you've added not another dimension. I mean we were only a hundred and fifty to two hundred people in Pittsburgh. Again, there's five thousand engineers at Seagate. What difference does it make? You may as well just put them in there unless we're really going to do something different in which case you do it.

Allow others to fail, as well, i.e., delegate. Don't try to do everything yourself and don't try to micromanage everything that's going on. Hire good people and let them do the work. Yeah, you got to supervise it and you do have to know what's going on. One of the things I learned at IBM-- when I was at IBM I was asked to go out for a week with Seymour Keller-- there were-- all the senior management in Research; there were seven of us, one picked from each of the departments-- to go out to Montauk Point with them for a week. And one of the things that they talked about was the fact that you needed to have-- they gave us a business case actually-- and one of the cases it compared people doing-- there were-- you had a job-- a personnel choice and your-- the choice was between somebody who was really good at dealing with people, and somebody else who was a bit rough around the edges with people, but was really good technically. And the issue was which one do you put in place in order to get the job done, and the right answer from their point of view is that you took the one who wasn't-- who was a little rough around the edges. You want somebody who knows what it is that he's trying to do and then counsel him and try to get him to be better at-- it's easier to smooth up his rough edges—

Bajorek: --rough edges--

Kryder: --than it is to—

Bajorek: --create--

Kryder: That's right, and I think that that's important.

Another thing is don't be afraid to piss people off. I see so many cases where if you bring people in and you end up treating everybody alike -- you have a bad performer, you're afraid to really go after him, you know who the people are you're going to lose because you weren't willing to? You're going to lose the best ones in your organization because they are the ones who are going to get pissed and that always happens. So you've got to be willing to---- pleasing everyone is simply a sign of mediocrity and it doesn't work. Yeah, you got to-- I had an open-door policy. You got to encourage your employees to bring their problems to you. Don't be afraid to question. I could point at people at Seagate who I would certainly say fall into this category, but if you've got somebody who's a yes-man reporting to you, either you or him are redundant and one of you has got to go. It's really true and some people-- some managers expect that sort of thing. They pounce on people. They want yes-people working for them and you don't need them and probably it's the guy who wants the yes-person that is the wrong one to have around.

Remember your customers. It doesn't matter whether it's academia or-- industry or academia who are your customers. I can remember telling one of my grad students that-- referring to him as one of our customers and he never thought of himself as one of our customers, but I mean they're one of our customers, but the companies who are sponsoring the research are also one of our customers. And you get-- you need to keep all of those people in mind. In industry, of course, the customer is king and everybody knows that sort of thing, but people don't necessarily think of it that way in the academic world, but people have to think about that. So whoever it is who receives your product or service or whatever it is.

One of the important ones, I think, is that leadership is a lonely thing to be doing. You need someone whom you can bounce ideas off and confide in and try and figure out whom you can trust to be a sounding board. In my case, I had two people for two different purposes; so I used them both quite often in those situations. One's my wife who is a psychoanalyst, and she was invaluable when I'd bring her some of the personnel problems, and the other one was Jim Williams who was my right hand. He could step in. The way we'd handle things eventually sort of merged in our thinking and he understood me very well. If I had tough problems to go after-- like I say I was never bothered by the technical problems, but problems that are tough are getting the team working right and handling the personnel issues. Jim was very good at seeing that sort of thing too and hearing me out, trying to-- just bringing it up, talking about it, figuring out-- doing those things. And in a leadership position you've got to have somebody like that whom you can trust, otherwise you're just doing everything on your own, freewheeling. Discussing, and getting another point of view is I think critical in a lot of situations.

Another one is don't be afraid to challenge the pros even in their own backyard, that you have good ideas and you can make things happen, and again my experience is most things succeed or fail based on the people who are driving those projects. I mean you have to take responsibility for what you're trying to pursue. In R&D I personally don't try-- even with my students or -- professionally at Seagate I never tried to over-direct things. I might have a different technical view than the other person. I'd ask questions which would cause them to think about those issues, but I've been wrong many times, and sometimes I found it was quicker if this guy was really motivated-- I thought it was the wrong way to go but he was really motivated to do it, I'd decide okay, go do it; see what happens. And if he then ran into a brick wall he'd come back very quickly and go the way I wanted him to; whereas if I push him down my route he doesn't believe in it anyway and nothing happens; it doesn't get there. So, it's a fine balance, but you got to somehow develop the skill to be able to assess those and figure out when one is appropriate and when another isn't.

Optimism is a great force multiplier. Give me an optimist any day over a pessimist. Actually, I wanted can-do people like I said; new grad students were fantastic that way generally. There was one student I had and he had gone off and worked in industry and we eventually hired him at Seagate. He was a very bright guy, and I knew who I was hiring when I hired him, but he was-- he was a pessimist, and I wouldn't have hired him, but he was so good technically that I decided I'd go ahead, but what I put him in charge of was testing and he did a bang-up job. I mean he was skeptical. I mean he was the one who would test the hard drive, the perpendicular drives until they-- you were blue in the face trying to prove that they weren't going to work and came up with all kinds of new tests. That was a place where I actually found a use for a pessimist, but generally I'd much rather have somebody who thinks he can do it and fails than the one who's always throwing rocks at whatever others are trying—

Bajorek: It can't be done. Right?

Kryder: Right. That's right. And then of course have fun. Take time to have fun yourself. Don't always go at a breakneck pace and expect others to do the same thing. I mean they've all got to take time to enjoy life. So those are sort of the—

Bajorek: The nuggets—

Kryder: Yeah.

Bajorek: --your experience shared via that course. Was this taught at a graduate level or--

Kryder: Yeah, graduate students.

Bajorek: One last question: What are you doing now in retirement? I recall when you were at Caltech I think you did some sailing.

Kryder: Yeah.

Bajorek: You wouldn't do any sailing now, would you?

Kryder: Yeah. Well, I'm doing a number of things in retirement, but since you brought up sailing yes, I'm very much involved in sailing. Well, you and I used to go sailing together when we were at Caltech. Floyd Humphrey would take Arnold Beckman's boat over to Catalina for a weekend, and you and I were his first mates on board on the boat and so we got to go over quite frequently when he'd take a couple faculty members and a couple grad students and a couple undergrads over, and that was great fun. And my brother used to race Flying Dutchmen and I raced with him when I was younger, but then we bought a place up in Maine and in 2005 I bought a 36-foot Morris M36 and sailed that quite a bit. The M36 is not really a cruising boat; it's more of a day sailor; though Sandy and I did take it out for a couple days at a time-- two or three days at a time, but it doesn't have a big galley or anything like that and there are some bunks you can sleep in and there was some space where we could set up a stove and do a little bit of cooking, but then a couple of years ago-- almost three years ago now I bought a 48-foot Morris Ocean Series 48—

Bajorek: Is that a sloop?

Kryder: It's a sloop-- a 48-foot sloop. Actually, if you get I think it's April-- probably April 2015 I think issue of Sail magazine and Cruising World it's on the cover of both of them. It's a custom boat, which I largely designed, with choices and then you sort of tell them how-- the way that you want things done. It won a prize from Sail magazine for having the most innovative electrical systems. And the thing that's innovative about them is that in a typical cruising boat what happens when you go cruising is that you sail all day, but you've got batteries on board the boat that you're running when you're cruising all day and sailing and having a good time. At night you want to turn the lights on and you got to have-- you've got a GPS, an electronics package and you've got a lot of things on there that use electricity. So now you've sailed all day, it's time to come in to a port, a nice place where you're going to anchor and maybe have a beer or something and sit and watch the sun set-- go down and you have got to turn the damn engine on, because you have got to charge the batteries. So you let the engine run for a couple hours. It's the stupidest thing in the world because it's just when you don't want to have an engine running you end up—

Bajorek: Having to run--

Kryder: Right, having to run the engine because you have got to charge the batteries. The design that we worked up totally reverses that. The other thing is that generally what they do is that they run a generator, okay, an AC generator on board the boat and so if you need alternating current you turn on the generator so if you want to run a hair dryer or you want to turn on some air conditioning or whatever, you have got to turn on the AC generator. On our boat, it's a DC boat and what we do is that we have a DC generator. The DC generator is nearly silent; it's very quiet. If it comes on while you're sailing, you probably won't notice it, but every now and then you think oh, I feel a vibration on my feet, oh, that's the generator running. It dawns on you but you don't notice it right away and it's—

Bajorek: It runs by diesel fuel?

Kryder: Yes, it's a diesel fuel power and it charges the batteries, okay—

Bajorek: While you're running.

Kryder: --while we're running. Anytime it senses when the batteries are getting low, then it comes on and charges, and so it's just totally run by DC and then we have inverters running off the batteries to create the AC so we have AC a hundred percent of the time. We can go down and use our AC stuff any time we want to. So, it's a different design and we got a little award for it, so I applied my-- maybe my most practical electrical engineering was the—

Bajorek: --the electrical system on this boat.

Kryder: --the boat.

I'm also serving on the board of-- Schoodic Institute, which is an institute that recently got started up at Acadia National Park in Maine. This is nearby where we have a home, and Schoodic Institute is based on national park lands; it was actually lands that used to belong to the Navy. The Navy had a submarine-detection facility there, and they closed it down. And the Schoodic Institute took it over and the Schoodic Institute is looking into questions of both forest and marine ecology and how climate change is affecting all of these things -- do studies on what happens when if the climate changes the berries on the tree ripen a couple weeks early, but those berries normally coincide with when a particular bird comes, are we going to lose all those birds, is the whole population going to disappear, and various questions of that type.

Bajorek: What could you do to ameliorate the problem?

Kryder: Exactly, and one of the studies that they did do, which I thought were very clever is that they wanted to get some measure of the spread of mercury up in Maine. We know that mercury is spread--being spread around the world from pollution, but how do you measure this-- concentrations of mercury. Well, you know how you can do it in fish, right, because you can measure. You know that -- the large predator fish tend to concentrate the amount of mercury in them because they eat the smaller ones and eventually you get a measure of it by catching the sharks or whatnot. Well, they came up with the idea well, if that's true for fish maybe we can do it with insects and what are the insects that are predators? Dragonflies. And so, during the summer up in Maine they recruit kids-- young kids who are up there at Acadia National Park trying to see the park, and they want nothing more than to go muck around in a pond to collect dragonflies and things of that nature so they get hundreds of—

Bajorek: Samples.

Kryder: --kids doing that. They collected them all and darned if it didn't work; they could measure the amount of mercury in it and determine the relative levels of contamination in various areas, and now this is-- it was highly successful. It has now spread all over the national park system in the United States and is being used by all kinds of people to try to understand better how mercury is being spread around in the United States. So, they're making some interesting contributions, not related to magnetics, but I'm having fun doing it nevertheless because of my background and they do have grad students and faculty and so forth involved in all these sorts of things so yeah, I'm doing that. And I'm on the board of another company who's trying to make diesel particulate filters and all kinds of ventures of that nature.

Bajorek: It's good to know that you continue to approach this world with enthusiasm--

Kryder: Yeah.

Bajorek: --like that.

END OF THE INTERVIEW

