Oral History Panel on Advanced Thin Film Disk Heads

Interviewed by:
Ian Croll

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Abstract: *The development of thin film magnetic recording heads made possible the disk drive industry's continuing evolution to higher recording densities and drives with higher capacities. Panel members participated in the key development activities, mostly by IBM, in various laboratories, which eventually resulted in the MR and GMR heads used throughout the disk drive industry.*

Ian Croll: We're here to talk about the development of thin film magnetic recording heads. And with me, to introduce yourselves, on my left,

Bob Jones: I'm Bob Jones, part of the effort to build a film head, primarily in development.

Bob Hempstead: Bob Hempstead. I joined IBM Research. I worked there for about six years on mostly MR heads, and then joined Jones's group in San Jose Development Lab, and worked on the inductive thin film head, and then went into manufacturing to try to make them in volume.

Ron MacDonald: I'm Ron MacDonald. And I was in the Advanced Technology Group, associated with San Jose, and I began in the thin film head in that time frame.

Dave Thompson: I'm Dave Thompson. I was in IBM Research for thirty-two years, and pretty much with the heads the whole way.

Croll: OK, thank you Bob. Since I think I'm older than anybody else here, I would like to set the stage of how, at least within IBM, the effort started for thin film heads. In San Jose I had the responsibility for disk drive development. And there was a project called the Zeus Program, a 2305, which was a one head per track project for fast access. And I was at Yorktown Research at the time, and for a long time there had been work on thin film memories, permalloy, mostly by evaporation or sputtering, but mostly sputtering. But I was responsible for doing some electroplating at of permalloy. And that was made possible actually by a fellow by Irv Wolf at General Electric, who found that you could plate a permalloy fairly thickly and still have a low coercivity if you put saccharine as an additive. And that lowered the stress. So that was what sort of made us want to investigate plating. At that time, the Zeus head was having a problem under Eric Solyst with chipping and some bonding problems. And so Denis Mee, who was in the ad tech function at San Jose, called me up, because he knew of the plating and of the strip-line plating that we were doing, and asked if there was any way that that might be turned into a thin film head transducer. That was at a very appropriate time, because we had a lot of magnetic and thin film expertise being built up in the company. But it looked like semiconductor memory was going to out trump both cores and film memory. So that was something I really jumped at. Now, the first blush. Gene Albert was doing plating for what was called a couple film memory. And the reason it was plating was because it would wrap around the copper strip line, and therefore was good in terms of reducing demagnetization fields. So I asked him if he could do that, and make a sample head. Now, the question was, how do you make a gap in that.

And I ran into a fellow, Alec Broers, who was doing electron beam exposure. And with his help, he was able to expose the negative photo resist, and then we plate everywhere except where that photo resist was. And I don't if know if the camera can pick this up, but that is an example of the gap of that first type of head. And it was a very good resolution of gap, and naturally was the beginnings of what became very important, which was additive plating. That is, instead of using photo resist mask and subtractively etch the material, it would plate everywhere where the mask wasn't. And that turned out to be important, because you could get quite high aspect ratios, with rather thick films. Now, this next picture shows downside of that head. It was called a horizontal head, and the flat surface faced the recorded media. And it was very susceptible to wear and picking up debris. So the next person that came along was a fellow called Paul Landler in Sindlefingen, Germany. They also were working on film memories, and he came up with the idea of what was called a vertical head. And that is, a gap was defined by the conductor between two permalloy sheets. It was very conducive to making the thick fixed head arrangement, that is, the head per track. Certainly it seemed better than that horizontal head, and that scheme was used, but using plating rather than evaporation, because it was very difficult at that time to do evaporated films that thick without getting high coercivities. So with that, the main thing I wanted to get across, it was very interesting because from different laboratories, from different parts of the world, and people skilled in different types of technology, there was a confluence right at that time. So perhaps we could pick it up now. Perhaps, Dave, you could go ahead with what was wrong with these single turn heads, as we call called it, the vertical head. What was it, Ron? Did you say it was a five dollar head and a fifty dollar driver to work it, something like that? But anyway, yes. So you might give some background there.

Thompson: All right. Well, backing up just a bit, I had joined IBM in '68 to work on thin film memory, as a magnetics and electrical designer. And when film memory went down the tubes I had a choice under Hsu Chang of going to work with him on magnetic bubbles, or find my own project. And since I really didn't like bubbles, I mean not that they weren't interesting, but I thought they were being way oversold. So I started a project on magnetic recording with Luby Romankiw, who was our fabrications specialist there. The problem with one turn heads is that they have a very low read-back signal, and they take a very high write current. In fact, both of those were numbers that were completely unacceptable at the time to the device designers. So we set out to make what we thought would be the first multi-turn head. In 1970 we made a five turn inductive head, and by the end of that year we were making eight turn heads with track widths on the order of fifty and a hundred microns. We made them on silicon wafers. And of course they were vertical heads. We couldn't test them in Yorktown, so we immediately started casting around for partners who could help us see if they were any good. And that's where the guys in San Jose came into the picture. We also were looking for partners in the tape business. And in 1970, we also invented, or thought we invented, magnetoresistive heads. We made a bunch of those. Actually we had a big coup with that, because it turned out we broke the Bell Labs monopoly on bubble patents with that MR head. We used it as the first MR bubble sensor. But we also hooked up with some of the consumer guys, the ASDD division. And I think the first MR heads that actually went into production were in the Macy's price tag readers. Anyway, we got in touch with the guys in San Jose. And they're sitting here. I'll let them tell that part of the story.

MacDonald: Well, I think the thing is that in the late 60s we started in to do some magnetic recording in the advanced tech activity. And initially, all of the ad tech laboratories that were scattered around the country in the different development labs were established to help move things into development in some way. Historically in San Jose we did all kinds of activities that did not help magnetic recording. We had programs that were designed to put magnetic recording out of business, because everyone was concerned about the fact that there was a big gap in speed between the memory and the hard disk drives. So we had all kinds of things. With the advent of Denis Mee at San Jose, that brought some culture of magnetic recording into our advanced technology area. And we had, at that time, probably Denis and one other person, Dan Chapman, that knew something about magnetic recording as such. We began initially to follow the lead in research in Yorktown that Croll talked a little bit about, where we started to look at the horizontal heads. But then shortly thereafter we began to move into the single turn heads that Dave has talked about. And as he indicated, they were a little bit low on weak signals, on read signals. They were also hard to write with. We used to charge up a coax line, and diskharge the coax line in order to get the heads to write. Then we had a person by the name of Lester Shew who would look at some of the recording that we had done. And the recording surface that we used at that time was tape media. And the net result of that we couldn't read the signal inductively from the recording we had done. But we demonstrated with putting magnetic solutions there that we had actually written. <laughter> And we featured this in our monthly reports on occasion, that we had now written with single turn heads. We moved on shortly thereafter to try and do multi turn heads, that Dave talked about. We had a little bit of a restriction there, in that we had really no process people within the advanced technology area. Fortunately, Jones had come out from the components division a short time before that, and he was interested in taking some kind of staff assignment up at Menlo Park. And I managed to find out that he knew something about sputtering. And I shortchanged that, and had him transferred to the advanced technology lab, and we made him our sputtering person. And he started to formulate a little group of people that had some process capability. And that included Paul Simon, who interacted with Luby Romankiw in Yorktown. And Luby was able to transfer electroplating technology to Simon. And they worked out pretty well as a team. We went around with that for a short period of time, when there had been a program that was created up in Menlo Park by Glen Bacon. There was the need for a couple of components to make that magnetic recording drive work. So Jack Harker called a group of us together. He had now taken over the technology and development laboratory. He called a group of us together, and said, "What I want is a program to do development work on a head and on a disk." So we all went in, and at that timeframe, the 3330 was just about there. And we came out of that meeting, and said, "OK, we've got this ferrite head, and this brown and round disk that you can use, and we'll ship that up when we get it in shape to Glen Bacon in Menlo Park." And Jack said, he rocked on his heels, and went into this deep coughing phase, and said, "That's the wrong answer. <laughter> What you want is a thin film head and a thin film disk." So we had never made a functioning head at that time. And we got in touch with some of the people up in Menlo Park, and Harry Hill and some guy, Suite, came down to see us. And we laid out our plan. <laughter> And you have to realize that none of us had any idea of what it would take to take something from the idea of the inventor or the designer through to development and into manufacturing. And we laid out schedules for it. We had never made a head. We laid out schedules that today you can only laugh at, but we were serious about them. And people would ask, "Is that a commitment, or is that just — " And we'd say, "It's a commitment." <laughter> Anyway, we were fortunately saved by the fact that the Menlo Park facility went down. We didn't have to produce what we had promised we would produce. And we were kind of saved by liar's dice in that case. Subsequently, we went back to Mr. Harker, he had the responsibility for technology and development, and said, "What we really lack in this group that we have are process people." And he said, "Well, I'll go and fight to get you materials people and process people. Tell me what you want." So we laid out this great list of skills that we needed, and he went and fought the battle and obtained a head count for us. Just before that came to fruition, there was a Dr. Kelley, who had formerly run the Bell Labs facility in Murray Hill, had retired, and been taken on by Mr. Watson as a consultant. And Mr. Watson wanted to know how we should do materials work in IBM. And Mr. Kelley, in visiting our laboratory, concluded that we should create a material science complex separate from the development laboratory, which would be a composite of research people and development people, not only from San Jose, but from elsewhere in the company, as a matter of fact. So we created that phenomena. And the head count that was going to come to us to make processing capability went to the material science effort. And so we had to struggle on for a few more years without very great depth in that area. So we went into the thin film head business in the ad tech area, and subsequently managed to get it to the point where we could transfer it to development. And maybe Bob or somebody can carry on that little bit at that.

Croll: Well, Bob, I remember when I came out to San Jose I think you had one person per technology; one sputterer, one plater…

Jones: Well, actually, Ian, we were a little bit down from that. But we were struggling to get to where you had one for per technology. And speaking for the development lab, how that got started, I think the big event there was the demise of the flat film memory in Burlington. And there were a lot of technical people in Burlington looking for work. And we brought out-- well, I say we, IBM brought out something like five to seven people, including Paul Albert , Carl Anderson, Shell Meyboom, George Tzanavaras, John Papadopoulos. And there probably were others that I'm forgetting. But anyway, that was the event that got us up to the point you're talking about, where we had one development person for each of the things you needed. And what you needed was plating. And to get to the technology from Luby, you needed somebody in the sputtering business to deposit various thin films as a base for that. And you needed photo resist technology. And that's essentially what we had. And from that, I guess we had an awful lot of failures. As a matter of fact, I can think of years where about all we were doing were cataloging failures. But the company was patient with us. And maybe the other thing to say was that nothing else much better came along.

Croll: And one more time, the film head was produced on the grave of film memory, both in research and development. <laughter> And the second thing was, as you say, there was, I think, some unexpected support from above for the program. I remember actually in Yorktown the -- San Jose Research had gotten an exclusive right to do storage technology research. And there I was with a thin film head program in Research. Well, I went to my boss, who was Don Rosenheim at the time, and I said, "What am I going to do on this?" And I realized why he was my boss. And his wise reply was, "Keep working on it. If it doesn't work, nobody will know, and if it works, nobody will care." <laughter> And I thought, well, that was pretty wise council. And I think Jack Harker was pushing, protecting. I should say at this point, I'd like to mention, I had mentioned Gene Albert, who did that very first head, but right away went to Burlington for the development program in the film memory. And that's when Luby Romankiw had been working on looking at different properties of permalloy. And he picked that up. And it's too bad that he's not here, because he was certainly a very strong person in the program for years and years. But two of the really striking contributions was he took that additive plating process and developed it to an extremely high degree, in terms of the photo resist walls being straight, and this and that. And he also developed, because we wanted to plate more than one wafer at a time, but the idea of getting uniform thickness and uniform composition depended on very good agitation. And I would say, taking a very Edisonian approach, he came up with what was called a paddle cell, which to my knowledge, is still used, I think, in manufacturing film heads. It was not something that a regular chemist would have come up with, or even a mechanical engineer, I think. And the theory of it really was worked out many years after, <laughter> in terms of the diffusion mechanics. But it worked beautifully. And those two single things, I think, were very important in the development of that. Yes, Dave.

Thompson: A couple words, speaking as a magnetics designer. Lots of companies had thought up thin film heads. I mean it's an easy concept to just take a conventional head and squash it in one dimension or the other. And some of them had pretty nice programs, like the one at Honeywell or Siemens. But what they didn't have was the ability to get the magnetics of the permalloy part right. And that's where Luby really shines. The main control, from a designer's point of view, was everything in these heads, that and dimensional control. And you can't overemphasize how important it is to get, not only the plating through the mask right, but to get the composition correct almost to the edge of the element. Plating through a mask had been around for things like copper, but it took the pulse paddle plating cell, or whatever it's called, to really get the right kind of composition in and stress control all the way across the magnetic element, and get the thing to work. And I think, if you look at it in development, now, speaking as a magnetic designer, the main control was the magnetic bottleneck in getting heads to work for plenty of dimensional, and corrosion, and other aspects. But that was the big one from my point of view. And Luby was just super in solving those problems.

Jones: I'd like to add one other thing that Yorktown gave us in a very meandering path, and that was the full resist insulation. And my first exposure to that was by way of Luby on the silicon wafers. Luby had no sputtering capability or evaporation capability. So as an insulator, he used full resist insulation.

Thompson: He had plenty of evaporated glass Schott glasses, one of his favorites. In fact, you ought to hear what he used to say when you would always propose sputtered alumina or sputtered silica, and he was championing the organics. Because what it did to the stress in the next layer of permalloy. Whether or not you could pad in the stuff, that wasn't my specialty, but whether the permalloy on top of it was any good, that was my specialty. And the great thing about the organic photo resist was it left a kind of smooth, rounded surface that you needed for the next layer of permalloy.

Croll: Incidentally, things don't always go by plan. The first use of photo resist as the insulation, was really because we had made a commitment to ship films to San Jose. And we didn't have any other kind of insulation to use. So at least at that time, my thought was, well, we'll bake it, and then something better will come along. But it never did. It turned out, for reasons you say, to be one of the best things going. And it turned out that coverage in sputtering -- I think, Bob, you were doing silicon dioxide or something, weren't you?

Jones: Yeah.

Croll: And that turned out to be really a problem to make them equally--

Jones: Oh, yeah. Well, I remember when the effort was transferred, I guess from Ron's ad tech group over to the development group, the official plan was to use sputtered insulator insulation. And I guess I was as much responsible for that as anybody, because that's what I had learned working in the semiconductor business. Well, that lasted, I would say, until the end of the first day when we tried it. <laughter> It was a flaming disaster. It could not hold any kind of dimension in etching those layers whatsoever. And so there was nothing in the running. I mean at the end of that first day I remember this terrible mess under the microscope, and saying, "We'll never make a product that way." And then we fell back on the photo resist insulation. But the wisdom of the day said that you couldn't use photo resist insulation and sputter on it, because the glow discharge of the sputtering chamber would disintegrate the photo resist. And it scared me, because I remember when I was working on transistors and doing sputtering that if you found a wafer that had photo resist on it you couldn't wait to throw it away. I mean you just as quickly as possible heaved it into the waste paper basket. So at the end of one day we found out we couldn't do sputtered Silicon dioxide or sputtered alumina, and what was left? Well, what was left was photo resist insulation. And I remember telling Tom Kennedy, who was sputtering alumina at that day, I said, "Tom, I guess we're going to have to try sputtering on top of photo resist insulation." He said, "No problem." And of course I was quaking in my boots. I thought this was a sure road to the second big failure of the day. He just stuck it into a sputtering system, turned the thing on, it worked beautifully.

M: For people in the window shade business.

Jones: And we never looked back after that. I mean in terms of properties, the sputtered insulators were superior and still are to this day, but they're unusable in that context where you have to etch and hold very precise patterns. So the photoresist had it, and as you looked at it more carefully, it had a smooth surface, it had a fine forty-five degree or so angle coming up that was about the best you could hope to do for the magnetics. And all in all, I presume it's still being used with no prospects of it being replaced, to my knowledge.

Croll: Is that right?

Hempstead: Actually, it finally has been replaced. The whole advent of the CMP and different techniques has -- We've really gone to like a dual damascene kind of process for the heads.

Thompson: But it took the perfection of chemical mechanical polishing for that to be possible.

Hempstead: Exactly.

Thompson: Until you could polish things flat, and basically remove all the junk, the stuff that sputtering put in. And that was many years later. Until that you just couldn't do it. But it was interesting to watch in the literature and in products our competitors, the Hitachi's and the Fujitsu's, and those guys. They would always start out with an inorganic insulation. By the time they got it into a product it always looked just like ours.

Hempstead: Well, I also remember IBM used to love technical audits. And so every six months they would set up an audit. And this team from the semiconductor divisions would come out, and they'd all look at what we were doing. And universally they all said, "Well, of course photo resist insulation will never work. And you can't ever put that into a product. So when are you going to change it?" And we would just smile and nod, and just keep using it.

Croll: My standard thing, well, we're working on it, but… <laughter> But we did do a heck of a lot of baking and reliability testing and all that sort of thing. You coined a phrase that saved me a lot, because somebody asked the question, "What is the insulation?"

Jones: Ah, yes.

Croll: And you said, "Well, it's much like Bakelite.

Jones: I don't know whether that's true or not. <laughter>

Croll: I don't know if it is or not. But people would say, "Oh, Bakelite. Yeah, that's an insulator." And I mean I've used that phrase of yours over and over again, because it seemed to sell.

Hempstead: Although and later when we got into manufacturing then we had halos, if you remember.

Jones: Yes.

Hempstead: Where we discovered that sputtering alumina on photo resist insulation was not as easy as we thought. And it took us about a year, I think, to figure out how cool we had to keep those wafers to prevent that from happening.

Jones: Well, that was a technique that, again, that's picked up from my experience in East Fishkill with transistors. We did gallium, indium gallium or gallium itself, on the back of the wafers, and you put it down to a plate, and had water it. We felt we had to do it for the integrity of the transistors that we were packaging at the time. But there were a lot of details that had to be learned to handle the long sputtering times, like eight hours, or ten hours, or something like that.

Croll: I remember there was quite a detective story attached with solving that halo program; looking at different sputtering places, and different units.

Hempstead: Yeah, and in the end the way we actually solved it was to go to the center target. It turned out that the hot pressed alumina targets, the surface was getting so hot that just the radiant heat getting on the heads, there was enough thermal resistance down to the gallium indium that some of the parts would get too hot. And when we finally converted from the hot pressed to the sintered targets that problem went away. But until then, you couldn't one hundred percent of the time keep those parts cool enough to avoid halos.

Croll: Yeah, and as I recall, one sputtering unit would be different than another one, and one position would be different than another one.

Hempstead: Right.

Croll: In fact I used that one time. I went to one of these business school, you know, quickie things, and they wanted to put case studies on something. And I used that one as a case study of how to go through one of those programs, so I did get some positive things from that. <laughter>

Hempstead: I think another thing we should talk about is the sputtered alumina. You had really the beginning of that where you started out with SiO2, and that didn't work. And then Tom Kennedy developed, I believe, the sputtered alumina process.

Jones: I had done one or two, but again, the old friend for me, was SiO2, and I was going to use it till something made me not use it. And Tom was very easy to talk into doing something new, whereas I used to lose sleep over it. And he said, "Yeah, we'll do alumina." And we had figured out that the expansion coefficient of the commonly available inorganic insulators of the day was, alumina was the closest match to the other constituents of the head. And Tom stepped up and optimized it, and made it work.

Hempstead: Right. And this was another thing, that our competition was sticking to SiO2 far too long, and couldn't get it to work. And all the gurus from our semiconductor division would come and say, "We can't use alumina because it's such a lousy oxide, and it's not going to last in the field." Today all thin film heads are made with sputtered alumina, and no one has ever found a material that comes close to that.

MacDonald: I think of all of the groups, in terms of the advice that you got while things were going on, we always got very bad advice from the components division. <laughter> And Bloch himself would come out. And in the early phases we made heads in chemical fume hoods. And he would look at these in the scans, and say, "You can't make this product in this kind of thing." And of course we weren't even thinking of product at the time. But subsequently we had Bob Knight that put together a facility as a pilot line kind of operation that looked a little bit better with all kinds of things that you would expect to see in a semiconductor type of operation. And when Bloch saw that, then he thought it was too much of an effort because we were only going to make a few heads anyway. <laughter> So we always got bad advice from the components division. We had one dependence on them, however, and that was when we got these wonderful multiple turn heads, we had to make a mask somewhere. And initially in the development laboratory, the facility that did reproduction for you, you know, you took a piece of paper in, you wanted six copies made, was the place that made masks for us. We didn't have a very good mask capability. And we had to depend on Burlington for our masks. And unfortunately we were like number twenty-seven on the list of priorities in Burlington. And whenever a design would change on the head, I mean Bob's life would turn upside down. He and Mark Church had to redesign a mask set, and do the CADAM and all of that, then get on bended knees for Burlington to make it. Meanwhile, you had product guys and the integration group beating on your back to say, "Give us some heads. We want heads that look like this now." So I think, when you look at the overall situation there were design issues related to the head, there were process issues, there was were materials themselves issues, dimensional controls. And as we started into that, all of those things were kind of hidden underneath it. And as you peeled one layer after another off of this onion, you always found another problem layer underneath. <laughter>

Croll: Well, something like this, there's a tendency to emphasize the magnetic characteristics and the electrical. But not the least of the problems -- What?

Thompson: What could be more important? <laughter> That's my specialty, but go ahead.

Croll: Right. But in terms of getting things out the door, some of them were just machining issues, and as you mentioned, dimensional control, and choosing a substrate, because silicon was not necessarily the best.

Thompson: I was trying to get a word in a minute ago. The two things in Yorktown that we always viewed with wonder, and which nothing from Yorktown contributed, was the thick alumina and the substrate. And we were always kind of in awe of those two particular things, because they were not the things you would think to try the first time around. Thick alumina you would swear it could be so stressed it would just flake off. And the substrate they picked, that stuff was developed for use in drilling rock for oil. It's the toughest ceramic known to man, and there are two reasons why it really prevailed. One is, it's difficult to injure in processing because it's so tough, and the other is that the guys on the disk side were putting alumina in the disk coating to make the disk more durable, and it wore the dickens out of any normal head.

Hempstead: I think the substrate story is an interesting one to tell, because we started out with, I think it was a Corning Glass ceramic.

Jones: Corning Glass ceramic. And glass ceramics on the face of it, a good story. It's void free. It's really a glass which has been turned into a ceramic. Sounds ideal, except that of course the stuff was very dear. In other words, Corning was going to charge us millions just to get into the business.

Hempstead: Well, yeah, we were getting these substrates.

MacDonald: We were getting them one at a time and things of that nature. And when we began to get serious it turned out that they were going to have to basically build the facility to do -- They had a little pilot line arrangement that some chemists in the background made this stuff for us.

Hempstead: Well, actually let me tell the story, because it's a fascinating story. It turned out that NASA had paid a million dollars for a boule of this stuff, and then had not used it. And when IBM showed up and started asking for this, they were just cutting through this boule. And in manufacturing we were tearing our hair out, because some substrates were clearly very good and wouldn't chip, and some of them would chip like crazy. So finally we had them come in to say, "Look, what's wrong with these substrates? Why do you have some good ones and some bad ones?" And they finally fessed up to the fact that they had this one boule that they had made, and that had different regions. Some were good, and some were bad. They had no idea why. And the really bad news was they had come to the end of this boule, and it was going to cost IBM another million dollars to make another boule. And they could make no assurance that this second boule would be any different from the first boule. In fact, they weren't even sure they could reproduce the first boule. And so we had Walt Jacobs, who was in charge of the substrates, in that meeting. And after they left we turned to Walt, and we said, "A, we're not going to spend a million dollars on this," because in those days a million dollars was a tremendous amount of money, "So what else do we have?" Well, Walt said, "Well, we've got this titanium carbide, aluminum oxide, that we've been using to make burnish heads for these disks." And Bob said, "Well, we've tried to make some heads on this, and we can't find anything disastrous in them. Do we have any other substrate that we have any data on?" "No." "Well, I guess we're using aluminum oxide, titanium carbide." About a year later, or maybe two years later, Hitachi showed up and said, and we were just terrible competitors at this time, and didn't talk to one another. But they said, "Could we please talk to you about the choice of your substrate?" This was after we had shipped the 3370. And so we said, "OK, you can come and talk to us." So they came, and for an hour they presented us data on fifty different substrate materials that they had reams and reams of data. And their conclusion was this was the worst one you could choose. And their question was, why did you choose this one? <laughter> And we hid behind IBM confidentiality, but the fact was, we were embarrassed to admit that we chose it because we didn't have any alternative at the time, and we had none of this data that they had gotten. And today we still use that material, and in spite of the fact that people have spent millions of dollars trying to find a better material, the fact is that because we've invested so much money in this material it's been developed to the point where nothing else can get over the hump to compete with it. And so it is still the material we're using today. Somewhat improved, but the basic material is the same.

Jones: Well, one of the things about -- Let me add some numbers here. The original glass ceramic from Corning was Z16, and N58 was the IBM mixture of this tool bit material. And there was one guy that really got something out of this tool bit material, and that was Miles Cook and the machining people. It turns out that mixed material, the N58, held an edge very, very well as you machined the rails into the substrate. And he was enthusiastic for it. In my quarter, my boss in the early days was Paul Albert. And Paul was inclined to, I'd say, overreact sometimes in defending the old way of doing things. And I think he wanted to have the --

<break in tape>

Jones: Was in the substrate story, but…

Crew: Just start from the beginning.

Jones: OK. Let me tell you this. I can remember to say this. There was one man who really liked the N58 composite, the alumina titanium carbide composite, and that was Miles Cook, because he had the job of machining rails into the sliders. And he found that material with the saws available would hold a very good edge, better than the other materials that we'd been using. I was going to finish a little bit on the substrate. There was a little bit of warfare going on there. My boss at the time, Paul Albert, was very much in favor of having Corning make this material, because we had had some experience in processing with it. And there was also, maybe in a slightly earlier timeframe, the thickness of the substrate changed. Initially the idea was to build a thin film head in the middle of a composite. There would be thick substrate and then you'd put on top of that a thinner substrate, so that you got the head to be at the point on the air bearing where you wanted it, really using the Merlin air bearing of the day.

Croll: The cylindrical air bearing.

Jones: Cylindrical air bearing. So that required a two substrate assembly at the end. And as a matter of fact, when we were talking dual heads, like putting an MR in there as well as a write head, it became a three substrate assembly. And of course, at this point in time I'd tend to get chicken, I guess is the right word. <laughter> And so when somebody said, "We're going to use a four millimeter long substrate, and make it all on Winchester," I thought that's great. But Paul resisted that. He says, "We have to change every tool in the place to accommodate the thicker substrate." Furthermore, being thicker meant it was going to be more likely to have thermal problems. And he resisted this. In the end, the idea of getting away from the composite slider to make the Merlin air bearing to a simple one substrate idea was really a winner. I mean it simplified the process at a time when we needed as much simplicity as we could find.

Croll: Well, that brings a point, because at the beginning what was happening was, we were gluing on these silicon wafer-based substrates on to a Winchester slider. The idea, and was it you, Bob? I don't know who came up with it, but using the substrate to be machined into the slider was brilliant. I mean that was certainly better than having to glue all these things on.

Jones: Well, my recollection, I heard that idea, and it was sort of floating around, but I heard the idea first from Jim Money. Jim says, "Hey, we'll just do this, we'll make the Winchester air bearing, slap those babies on the end, and you'll turn them out by the zillions." I mean it was oversell, because there were ten thousand problems yet to be solved.

Croll: But, nevertheless, the idea of using the substrate for the slider material was big.

Hempstead: And Jim had some ideas that were really amazing. And in retrospect, it's like, why didn't anybody else think of it? But it turned out when you went to that air bearing you had two rails, and the plan was to put the thin film head on one of the rails. And so, Jim said, "Well, put one on the second rail, and we can decide at the end which one we like." And that actually saved the whole economics of the thin film head. If we had not had that redundancy in the early days we could not have economically produced those heads.

Thompson: And, of course, up heads and down heads were the same at that time, so you could the left ones for up and the right ones for down, or vice versa, right?

Hempstead: Right.

Thompson: But let me go back to comment, of why did other people have more trouble, or how did people go into the ditch? Every time we got an inventor from the ceramics area or the ferrite head area or the semiconductor area, their head was full of inventions that didn't recognize the low temperature requirement for anything using permalloy and photo resist. And so things like this composite head, well, ferrite heads were glassed together. Glass is a glue that works. It doesn't work for thin film heads. It destroys the element. So we were always trying to correct people whose background caused them to invent people things where the temperature processing was just not acceptable. And I think every competing company went through this same cycle, because they of course had people with the same kinds of backgrounds.

Jones: Well, I was in the center of semiconductor background. I had prejudices in favor of all the semiconductor ideas. The only thing I'd say in my own defense is that they didn't last past the first time we tried it. <laughter> And at that point in time, you realize you'd either better watch your project go down the tubes or change your mind, one or the other. And we invariably went back to something closer to what you had done at your place.

Thompson: If you can't do it at three hundred degrees C or less it isn't worth doing. And that was kind of the mantra we evolved over those years.

Croll: Well, let's go to another -- One of the original thoughts, or early on thought, was to do separate read and write elements. And thereby, prior to getting the optimal pole tip configuration for the write element and for the read element, and further do write wide, read narrow to avoid adjacent track interference. And I think, Bob, I mean this led to attempts to put basically just two thin film heads right on top of another. And really, the solution in the end was the piggy back MR head. But maybe you could talk about something about doing the two thin film heads.

Jones: Well, most of the ideas that we tried was, the MR head of course is thinner, so we would lay it down first. Then we had to do something to provide contacts to the outside world. And then we were going to lap it flat, and then deposit our write head on top of it. We marched to that idea for a year or so. And the problem was this business of laying the second down on the first. You had to do a flattening operation in between to make the surface suitable for thin film work. And that was not easy to do with the tools of the day. You could do ones and twos, because you could find a good place on the substrate. But you couldn't do the substrate uniformly in lapping back to make that second surface to start with.

Thompson: Now, there's something that probably is not obvious that really hurt us in all of these schemes involving separate read and write head, and that is, at the high end the computer architecture that we used, was such that you didn't know whether you were going to read or write until you got the had on station and the sector came up. It seems really stupid, and it was historically based, but if you didn't know then the servo system couldn't put the right element on track properly. And it wasn't until sector servo and the five and a quarter inch small drives came around, and servo systems got the capabilities to handle this kind of thing, that we could use separate read write elements.

Croll: Yes, the other side of the thing, on the fabrications side before the MR was introduced, that the problem of putting the separate multi turn, spiral turn heads on, you had to have a thick, thick layer in between, and there was all sorts of interference and write through and that sort of thing. And not only that, you'd put that much stuff together and it would tend to peel apart. But going back to how things came in from different directions, I recall that Hunt came up with the element, an unshielded MR element. And then I think in Yorktown, George Almasi working on film memories thought that might be a good idea to use that at as a sensor for the sense line…

Thompson: In a coupled film string, right?

Croll: In coupled films. It turned out the signal to noise was awful.

Thompson: It's only a two percent effect, and so you could read it an individual element, but he wanted to read a thousand in series. And that didn't work.

Croll: Yeah, it just went down. But being on the same aisle, <laughter> that tended to translate into… Amazingly enough, the MR effort in Yorktown really started maybe a year or two after the inductive head work. It just took a long time to get there.

Thompson: From my point of view, the multi turn head and the MR head were simultaneous. We built our first ones in 1970. The wafers were side by side in a line.

Croll: But also, Bob, something you worked on much later came in. I think you were the first person to do ferromagnetic antiferromagnetic coupling on a film. It had been done in particles, but you did it on a film.

Hempstead: Right. And it had been seen at relatively low temperatures, like in cryogenics and the whole bit. I think it was really Dave came into my office one day, and said, "Could I try evaporating some iron manganese?" Because there was theoretically this effect.

Thompson: Worked bingo. But let me back up a little, the story before that one. All the way from thesis at Carnegie Tech, we had been looking for exchange coupled films that had the right set of properties, the most important being that the exchange field was less than the increase in coercivity when you put this sandwich together. Every known material at room temperature at that time, when you tried it, yes, you got some offset of the loop, but the increase in coercivity was even more than the offset. So when you turned off the field it didn't go back to one state. And when I hired Bajorek, one of his assignments was to look through materials. He looked at magnetite, he looked at all the oxides of iron. We looked at various copper chrome permalloy alloys. And then one day I was just sitting in the library reading one of these journals that the U.S. government put out of translated Russian technical articles. They used to actually do that. And there was this article about somebody who had put some film sandwiches together of iron manganese, and then zapped them with a laser for no obvious reason, and looked at their magnetic properties, and there was some loop shift. So then I went storming down to find a guy who could do it, and it was him.

Hempstead: Yeah, it turns out I had a dual gun evaporator, and you couldn't evaporate it as an alloy. You needed two separate guns. And so sure enough, I put iron in one, manganese in the other, and evaporated these films, put nickel iron on, and we got these beautiful loops. I mean it was one of these things where the first day it worked. And the second day we found something even better. And it was just like within a month we had figured this whole thing out, and it just worked beautifully. And then we sat down and wrote a patent on this. And MR heads kind of were, there was a lot of disinterest because they weren't commercially used in such a long time. And so maybe ten years later, let's see, now I've got to remember, it was Ken Lee, who was in Research, came to me and he said -- I had left IBM by that time. And he said, "You know, I started working on iron manganese, and we did all this great work, and we sat down to write invention disclosures, and we looked at that patent that you wrote with," Dave was one of the co-inventors, and he said, "You covered everything. Everything was claimed. <laughter> There was nothing that we could claim." The irony is that IBM commercialized that a year before the patent expired. <laughter>

Croll: Maybe this is a good point to start getting into MR, I mean magnetoresistive technology, because as you mentioned, Dave, it was almost simultaneously, at least with the multi-turn head. But it came in one heck of a lot later. And part of the reason was noise, right? And Barkhausen effects, and this sort of thing.

Thompson: That's true, and exchange coupling is a primary tool in controlling the main noise. But I think actually it was used in tape heads earlier than in hard disk heads, because in the tape business the stripes are so long that—

Croll: It was a small percentage.

Thompson: Most of the other domain control schemes don't work. You need a local one like exchange coupling.

Croll: Well, maybe we need to branch to Ron there, who managed that tape head program, right?

MacDonald: Yes.

Croll: And just to set the stage, at that time eight tracks, I think, were immutable. And now I understand they're up to about a thousand tracks because of its…

MacDonald: I think, like with the rest of the thin film head business, we started off very optimistically with the magneto resistive stuff. I think that the original proposal was something like thirty-two tracks, to jump from the standard eight to thirty-two tracks. And this was in a timeframe in which if you looked at the yield we were getting in the inductive thin film head business it was very low, if at anything. And how were we going to make thirty-two in row, where every one of them had to be good? And I think maybe Dave was instrumental in convincing people that maybe we should go just say to eighteen or something like that. <laughter> I mean which was still, on the order of magnitude, better than anything we had ever thought about. And so we did begin a program with that as the objective, to replace the standard eight track tape heads that had been there. And there was a product program put in place, which essentially was a reel to reel type of tape drive, in which we would use the MR head eventually. And like with the other programs, a number of people passed through the programs, both on the product end and on the film development activities. And it was eventually transferred to Tucson, and made into a product there. It had a lot of problems with trying to make the eighteen tracks all good at the same time, and try to get the margins such that, even if it was good, that it had the amplitude roughly as the other heads had. And it featured the fact that with a very thin film also, the magnetoresistive element, that all of the boogie men and concerns about corrosion and things of that nature on permalloy would be a real concern for the tape heads. And one of the ways around that, we thought, was that if you kept a small current flowing through the head that, even when it wasn't on the drive, that would help keep the temperature up high enough. And that would seem to be a way to alleviate any concerns about corrosion. We certainly proposed this to the product group, and there's a lot of resistance to that. And the other thing is that in some locations, in certain countries in particular, when you hit a power off switch, the emergency switch, all of the power is supposed to go off.

Thompson: Well, it's more than that. By law, you have to shut it off overnight to save energy. So literally they had to turn the drives off at night, which in this country we never did. Things stayed on twenty-four hours a day.

MacDonald: So I think that was a big concern. And of course corrosion as a whole was a concern for all of the thin film heads. And Ian maybe talked can talk a little bit about that, but we had a tremendous effort to prove that the heads would live. And even when one of the programs, I guess it was that NFP, the 3370, went into product test initially, there was a servo problem. We didn't have any head problem per say se, but they wouldn't let us go into product test because they said that the heads were subject to corrosion, and we hadn't proven that they didn't corrode. We said, "Well, what about the servo problem?" They said, "Well, we always have servo problems when we go into product test, and we always solve those." <laughter>

Hempstead: Well, the guy who really worked on corrosion was Don Rice, who I think has done the definitive work on corrosion of magnetic thin films that has never been equaled. And since then, we've sort of just been living off of the legacy of the work that he did.

Croll: Well, I have a little story about the product test thing. You're right. Don Rice had an analytical program that just would predict everything. But the product test manager said, "Well, it's only a theory. It's like Darwin, right? It's only a theory." <laughter> And anyway so the guy that was in charge of product test told me, I said, "Look, we'll just funnel this great gas mixture that's made for corrosion. We'll funnel into a disk assemble assembly unit, and we'll run it for this time. And if it's OK, then it will be OK, right?" And he said, "Well, yeah, I guess so." And I had remembered a person, when I was working on my thesis who was next to me, was trying to -- Chlorine was the big bad person for corrosion. And he was trying to do kinetics in a glass rack with hydrogen chloride gas. And he could never keep the concentration up in the tube, because HCL just absorbs on every surface. So deep down, where it wasn't theoretical maybe, I was pretty confident that the hydrochloric acid gas etcetera would just go on the -- But anyway, the guy at product test thought that was a marvelous test, even though Don Rice's was a much more definitive one. It was the old business of, well, just blast it through, and if it still works then that's OK. <laughter> But there's a lot of these interesting things like that. But now, Chris Bajorek has joined us. Perhaps you could introduce yourself, and give a little bit of your background.

Chris Bajorek: Well, first I apologize for being a little late, but I'm glad to be here. I have a degree from Cal Tech in electric engineering, and I joined IBM in 1971. I was in line with Hempstead to get our badges. Is that right? Same day. Probably got our badges within two minutes of each other. And my first manager was Thompson, sitting to my right. And my first assignment was to read a paper by Hunt on his paper about the magnetoresistive head proposal. And Dave said, "Read this, and then let's figure out what we could do with this." And that was the beginning of a twenty-five year career at IBM.

Croll: Well, that's probably a good place to start with the first product, the MR product, right? You and Dave had the wand, right? The Macy's?

Thompson: It would have the Macy's wand, and there was also a bank check reader of about that vintage, right?

Chris Bajorek: Right, but the one that made it as a product was the consumer transaction wand, and early on even Hempstead was very much involved in that project. Because one of the surprises of trying to apply a magneto resistive head for consumer applications in a department store like Macy's was that it would be subjected to abuses that were very hard to predict. People want to glue these magnetic tags on clothing, on ceramic parts, on steel hardware, right? And very early on we found out that the original choices of materials, namely pure silicon chip to carry the head, was too fragile. Secondly, these heads would be subject what we called thermal asperity disturbances. These magnetoresistive film resistors were not only responsive to magnetic fields, but they were thermistors basically. And if you frictionally rubbed them you will change their resistance with frictional heat. And we quickly had to turn around and figure out how could we make a robust head with minimal thermal asperity disturbance. And we went into various multiple design iterations, dual stripe elements with cancellation features, and this and that. And we finally zoomed in on the use of a single crystal sapphire substrate with a specific crystal orientation with a silicon chip making a sandwich, but the basic mechanical structure, thermal properties, were driven by the sapphire substrate. And that actually enabled us to create a product that was commercialized from a product division that was then in Boulder, Colorado. And made it to about, it was a product that was active till about ten years ago, from 1971 till about '95. In '95 you could still go to some Macy's stores and see this wand attached to the cash registers <laughter> reading magnetic price tags. But eventually the optical character, the optical barcodes, became standard. And so that was a short-lived application, but it was I think the first mass produced MR head product in the history of magnetoresistive heads.

Croll: You mentioned the thermal asperities, which makes me want wonder. Remember, there was the problem with tape heads with thermal asperities, and I thought you did a rather elegant thing. Did you not use a laser to simulate the thermal?

Hempstead: Yeah, when it first came up we saw this extra noise. And just from the time constant of the noise it seemed to me it might be a thermal issue. So I went down to the library and found a book that had an infinite number of solutions of the thermal equation, and sure enough, one of them matched the geometry that we had. And sure enough, you put in the thermal properties of the substrate and you got that time constant. So then everybody wanted to see some kind of experimental verification. So I ended up rigging a laser light that would just be scanned across the head, and prove that those were the thermal time constants that you got. And that convinced everybody that we were seeing thermal asperities.

Croll: Yeah, I remember that distinctly. It seemed rather clever.

Hempstead: And then the sapphire came up, and so we tested that to show that it was effective…

Chris Bajorek: It managed to get us enough margin, right, for that particular product.

Croll: Then I guess the question came up along the Holy Grail being a dual element, separate read and write heads, and MR heads were thought to be the solution. And perhaps, Chris, you could go through some of the -- Well, you and Dave did that original piggyback number.

Thompson: I mentioned that it came out in the small disks first, because they had sector servo, and the high end was still saddled with this wretched architecture. As I said, they didn't know if they were going to read or they were going to write till they got there. So the first products came out of the Rochester lab, right? The five and a quarter inch.

Chris Bajorek: Yeah, it was three and a half actually. Well, the first product, but it didn't ship in volume till there later was a Summit five and a quarter inch drive out of San Jose. And about a year later in very large volume was a three and a half inch, I think, Corsair.

Thompson: '91, I believe.

Chris Bajorek: '91. That was designed and developed in Minnesota. Rochester, Minnesota. But early on, although we did these consumer transactions wands, and then we had a detour into the second product, which is really a tape recording product, right?

Hempstead: Uh-huh. Ron's product.

Chris Bajorek: That's right. And in parallel we were trying to, always the goal was to see if we could make it work in a disk drive. And the dilemma was how to make an MR head work for a very, very narrow track width. It was naturally easy to get decent signals, a signal to noise ratio, in spite of the thermal asperity problems, in spite of if basic mechanism that we call Barkhausen noise. As long as the track width was long enough one could get average of these effects in a manner that made it practical. So this consumer transaction wand and the tape drive application were successful early because they had relatively large track widths. But already at the time of our experimental work the typical drive track width was about twenty-five microns, about a mil, right? We were almost at a thousand tracks per inch. And the challenge of suppressing these various deleterious effects, like Barkhausen noise or thermo thermal asperities, especially Barkhausen noise was much tougher in these narrow track width heads. And in fact, in Yorktown Heights we went through about a seven year process of trying to come up with a design that would simultaneously linearize the response of the very narrow track width head, that would make that head compatible with a piggyback design on top of an inductive write head, and that would be Barkhausen noise free. And we did not succeed in that first seven year endeavor. And we had to declare a break. At that time there was a mission transfer to move more of the magnetic recording work in the research division to San Jose. By then the inductive film head work in San Jose, from a development point of view, had matured to the point where there was quite a bit of expertise that could now pay attention to MR heads as well. And in the late 70s, in fact there was a couple of key San Jose people, I think it was Tom Beaulieau and I think Otto Voegeli, who invented the soft film bias technique, which turned out to be by far the best way to linearize the response of the head. And Dave and I among others in Yorktown had invented something we didn't quite know how to make use of, but we called exchange bias suppression.

Thompson: And permanent magnet bias.

Chris Bajorek: Yeah, but we never made those successful until there was a realization in '81, early '82 in San Jose now, that if we were to truly open up our thinking and say, look, we're going to make a piggyback head, and we're going to try to take advantage of exchange bias by making this -- Even though the track width was going to be very narrow, make the sensor very, very long, overlap the ends of that sensor with its conductive metallurgy, interpose an antiferrimagnetic material between those leads and the sensor. We had opted the a soft film bias. And by taking those drastic moves, which are a little bit counterintuitive, right? Because we were always thinking how to make these things smaller. We were not necessarily driven to making them longer. And lo and behold, through the work of people like Ching Tsang, Go Jen Zhu, and others, we managed to come up with a device that worked remarkably well. And that was encouraging enough to launch a formal product development effort jointly with the General Products Division. I think many of you are in fact involved in that. <laughter> And that broke wide open the possibility of now applying that technology to hard drives. Now, that was the beginning of an arduous process, because along the way we discovered you never know what you don't know, right? And you discover these things when you finally scale up and work in statistically significant numbers. And we ran into thermal asperities again in a hard drive configuration. And the preamp guys had to go figure out how to design a custom pre preamp to suppress or deal with those thermal asperities. We had to change how we formatted the data, the header information. We had to add redundancy and all of that. So even though it was in the early 80s that a lot of these things came together, it wasn't till '90 or '91 that we were able to launch this thing in products.

Thompson: Polishing the substrate of the electronics, so that it was exactly at zero because of shorting effects. But you know, I mentioned that people would come and complain about N58, and why did we use it, and why did they therefore have to use it in our competitors. The MR head was even better. When that drive came out in 1991, for the next two or three years at conferences people would come up to me and ask why we did this to them. Because all of them had to try to make an MR head work too, and they had to do all of the same terrible things. It was endless agonies for the industry.

Croll: Yeah, the question, looking backwards as the film head went into product, and other people would say, "Well, why are you doing that, because a ferrite head would do just as well?" And I guess the final use of the thin film head technology was with the introduction of the MR head. But perhaps, Bob, you could give a few other product applications. Why the heck did everybody feel they had to have a thin film head before the MR head came out?

Hempstead: Well, there were really two different worlds. There was the IBM world with the very large diameter drives, very high data rates, and people competing directly with IBM. And all of them were compelled, partly for advertising reasons, but partly for technology reasons, that the MIG heads, the ferrite heads wouldn't really work at those high data rates. They were forced into these inductive thin film heads, and then later the MR heads. But the Japanese that were sort of in a different marketplace, smaller drives, and not competing directly with IBM in some cases, had a lot of experience of MIG heads from the video tape recorders. And they had developed those so that, in fact, they were as good as the inductive heads in low data rate applications. So outside of the sort of IBM, large drive world those were the dominant head. And the inductive thin film heads were only used for servo heads, because they had dedicated servo surfaces. And so there was a servo head that was always working. And on the opposite side of that disk it would be writing. And if you wrote with a MIG head and you had a MIG servo head, there was too much coupling and it screwed up the servo. So the real market for inductive thin film heads outside out of the IBM and IBM competitive arena was for servo heads. It wasn't really till the MR head came along that it was so superior to the MIG heads that thin film heads made it into the small drives. And of course IBM was the one that pioneered that. And then everyone else was forced to follow along because of its superiority.

Thompson: And by the way, if you look at the aerial density versus time curve, that's exactly when there was a big kink, in 1991. And it wasn't just the MR head, but that was part of it. It was also things going on in electronics that were significant. It was when the thin film disk really got perfected, and so on and so forth. But there was a period of ten years there where the aerial density was running at a hundred percent a year, right?

Chris Bajorek: Yeah, it went from historical thirty to forty, to about sixty. Then it accelerated to a hundred. And it's now back to… Once we take the low hanging fruit <laughter>, we're back to the historic rate, maybe barely at that rate.

Thompson: Or maybe because we retired, right? <laughter>

Chris Bajorek: Yeah.

Croll: That's key dates. <laughter>

Chris Bajorek: But there was a period, Bob is actually right. Once it became clear what each of these technologies could do, and IBM had the MR head, and others did not yet have access to it, the arrows in their quiver were a film disk and an inductive film head, right? So the first advanced arrow they picked out was a combination of a film disk with an advanced ferrite head. And this was driven, remember, at about that time the PC world started. And the world needed small drives for PC, the five and a quarters, and then they came with three and a half. Then the world adapted to five and a quarters and three and a halfs for high end applications. But in that period from about '91 to '96, about a five year period, only IBM had MR heads. But inductive heads by then were available from multiple companies. So the first combination that allowed the rest of the world to stay on the sixty percent, and then accelerate to a hundred percent, was film disk ferrite head, followed by a film disk inductive head combination, and then eventually film disk MR head combination. And the whole industry, just gone on that, double Moore's Law slope.

Croll: Well, I recollect in the recording business the beginning of it really besides tape was plated metal films on drums with laminated high Mu eighty metal laminates. So that was metal to metal. Then it went <laughter> to ferrite disks and ferrite heads. And then it went to the different combination of film head on ferrite disks, and then ferrite heads on film disk. And now we're back to metal and metal, but in smaller dimensions. <laughter> But it's been an interesting somewhat winding road as you go through all this. And it's certainly clear that a lot of people contributed from a lot of different areas, using a lot of different technologies. And the ideas, sometimes what seemed like the key idea was easy to do, but filling in the rest of the dots, cleaning it up, took a remarkably long time. And that went from, what? 1968, and the first film head came out in 1979, and the first MR head came out in, what? '91, was it?

M: '91.

MacDonald: And I think the other thing in that timeframe was that there was a lot of opportunity to kill the program, and there were a lot of attempts to kill the program by various people, for whatever reasons.

And it was very difficult to really show that you were making progress when you couldn't present yield data.

Hempstead: But I think it goes back to IBM senior management. And I sort of marvel at going back. They really wouldn't let us work on ferrite heads within IBM.

MacDonald: In Research?

Hempstead: No, even in Development.

MacDonald: Really?

Hempstead: Yes. As soon as they decided to use the inductive thin film head on a 3380, they killed all development activities in ferrite heads. And we could have done the MIG heads, and done that. But I mean they had the foresight to realize that if they were going to be technology leaders they had to kill that old technology. Otherwise, it would just keep eating away at our ability to develop this technology.

MacDonald: And that was a real problem in the tape head end of the business. When we tried to put the MR head in we needed ferrite substrates. And IBM had gone out of the ferrite making business. <laughter> And so it became quite a problem for us. And I think we've finally got some work going on again in manufacturing here in San Jose. But it was, you know, one ox gets gored here and another… <laughter>

Thompson: But I mean if you were a ferrite guy, of course, you hated that decision. But there are other examples where the optical storage bigots really hurt magnetic recording and hurt some of our laboratories by deciding, for example, that tape drives were doomed, so let's get out of the tape re-box business. And in fact, when I started work on heads in Yorktown around 1970 there was no thin film head work in San Jose Research, because the head of the laboratory had publicly proclaimed that magnetic recording was a dead end, and it was going to be optical storage.

Croll: The person, who was the director, came to visit me when they got the storage, San Jose Research storage thing. And he said, "What do you want to do?" And I said, "Well, do you mind if I keep working on film heads?" And he said, "Well, that's not going anywhere, but I want to take up bubbles, and I want to take up optical recording." And I said, "OK." <laughter> And I'm glad I did. Well, I think we've covered a lot…

Croll: Well, at any rate, I think in this room we have maybe seven-eighths of the talent at one time or another. And I certainly appreciate you all coming here, and it's kind of great to get back with all of you. I think I've worked with you, each one certainly, for quite a while. When you move from Yorktown to San Jose you get to meet. And I'm wearing my suit because, as I said, this is a vintage presentation of the early 70s, <laughter> and I come across as a manager rather than a… <laughter> But at any rate…

Croll: Very good. And thank you again.

END OF INTERVIEW