Invention, Development and Commercialization of GMR Heads oral history panel:

Bernard Dieny, Bruce Gurney, Mustafa Pinarbasi and Virgil Speriosu

Interviewed by:
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Bajorek: This is the oral history of Messrs. Virgil Speriosu, Bruce Gurney, Bernard Dieny and Mustafa Pinarbasi, what I call the GMR team, about the invention through commercialization of the first GMR magnetic recording head for disk drives. Hard disk drives have survived as the information storage device of choice because of sustained and steep advances in storage density. Density advances required downward scaling of key components. The reduction of the size of the stored bits caused signal amplitude reduction, which necessitated the invention of more powerful detectors. Magnetoresistive heads were the successful answer. In 1997 the second generation of magnetoresistive heads introduced the phenomenon of giant magnetoresistance (GMR) to increase the change of resistance and thereby improve the signal to noise ratio of the recording system used for hard disk drives. Giant magnetoresistance heads sustained the advancement of hard disk drives for eight years. Key giant magnetoresistive head innovations were carried forward to present date tunneling magnetoresistive heads. This oral history records the invention through commercialization of the industry's first GMR head and the contributions of its success by the GMR team. So, I'd like to start with introducing the interviewees by each describing a summary or summarizing their personal backgrounds. And I'll start with you, Virgil.

Speriosu: So, thank you, Chris. Before we proceed into the more general questions, I do want to start on a very personal note. I want to say that I was born with a rare circulatory defect and it's not obvious to the casual observer, but it has affected me all my life. And one consequence of it is that my career lasted only 15 years. I retired just two years after IBM introduced the first GMR head that I called, and I still call a spin valve head, and then I spent years seeking and getting medical treatment for quite a few years. But it also meant that once we had a GMR spin valve sensor, I worked with a sense of urgency to see it, to help it getting transferred from research into manufacturing and to see it in a product. And it was a wonderful event for me when IBM did that. But prior to that, I was concerned for a couple of years, three years, that the manufacturing division at IBM did not have the proper equipment, and occasionally in my concern, I resorted to unorthodox methods to agitate the entire community of the need to allocate the capital expense. And I think you remember that because I gave you a hard time once, Chris.

Bajorek: I'm not sure I remember exactly that, but I do remember the fact that although I don't remember your agitation per se, I do remember your involvement and Mustafa's involvement in lobbying for the procurement of this tool.

Speriosu: Yeah. Well, it worked.

Bajorek: But I think, Mustafa, you ended up being very much responsible for bringing it up in development, right.

Pinarbasi: Correct.

Bajorek: For the early work in product development.

Speriosu: Okay.

Bajorek: You were successful.

Speriosu: Yes. So, and okay now, and so I'm going to go back to the standard flow of this interview. So, I was born in what was then Yugoslavia and it's a different country now, but ethnically, I am Romanian. It's a very interesting story. I don't know if I can-- should take the time to go into that, so I won't.

Bajorek: Please do, please do.

Speriosu: Please do?

Bajorek: Yeah.

Speriosu: Well, it was that part of Europe that is very heterogeneous. So, I come from a part of Europe where different nationalities are intermixed. And with all the empires that have been there over the flow of centuries, from the Roman Empire and so on, then the Ottoman Empire and the Austrian Empire and so on, lots of people of different nationalities settled in one area, and the borders that now exist were only formed in 1918 after World War I. So, although ethnically Romanian with a group of other Romanians, I was born -- I lived and was born and grew up in a country where the majority was of a different nation. So, so I was kind of a foreigner in my own country, if you like. My parents, well, okay, so we -- I immigrated to America with my parents 50 years ago in 1969. This was quite an occasion. Came on a boat and sailed into New York Harbor, right past Ellis Island, the Statue of Liberty and so on. We came to California, but that lasted only a short time. We went to North Hollywood. My father was a medical doctor, but he found that working in California was impossible at the beginning for him. He did not have the right credentials. So eventually we moved from California to Ohio. I went to high school in Northfield, Ohio, and then I went to college in Cleveland at Case Western Reserve. I got a degree, a bachelor's degree in physics there. Also, my mother was a social worker and both of my parents encouraged my early interests in science from the very beginning in every possible way, getting me to play with kits and doing experiments both in physics and chemistry and so on. It was all very wonderful. And my teachers, also I had a few excellent teachers from grade school on through high school. So, I've been inspired all my life by that, by an interest in the universe. But I also wanted to do something that would have an impact on the world today. So, my direction, my choice to go into applied physics explains that. I then went to Caltech because of its great reputation. And it was California, so I wanted to live in California. So, I was open to almost any area of applied physics at the beginning. It was a matter of being assigned as a research assistant or a, sorry, teaching assistant to a professor and that was Floyd Humphrey. So with Floyd Humphrey and Chuck Wilts, I met Chuck Wilts, who I ended up getting my Ph.D. with. So, he was a great influence on me in every way; in his style, in his approach to research, in his thoroughness, but also in his down to earth, modest attitude. While there, he arranged that I get an IBM predoctoral fellowship and seeing how you, Chris, were also one of his graduate students before me, I got a fellowship; so maybe I deserved it too. So, the samples that I studied were made by IBM. I studied the magnetic properties of ion-implanted garnets, magnetic and crystalline properties, and they were of interest to the bubble technology at the time, bubble memories. So I developed an early relationship with IBM. And after completing my Ph.D., I continued my interest in science with applications in mind as well, and IBM was a very natural place to go to. And again, you, Chris hired me, so, I'm ready to -- Let's go to the next interview.

Bajorek: So, yeah. A couple of questions. Was your dad able to practice medicine?

Speriosu: Yeah. Yes. So, he, yes, so it took him a couple of years to get all the right American credentials and so on, but then, yes. So, in America, he actually worked as a medical doctor from 1970 until 1994 or so, 24 years.

Bajorek: Very good.

Speriosu: Yeah.

Bajorek: What other schools besides Caltech had you considered or did you just--

Speriosu: Well, I mean, Caltech was my number one choice by far, partly because it was California and partly because of its reputation. I really wanted to go there -- so, once they accepted me, that was it.

Bajorek: I'm glad that worked out.

Speriosu: Yes.

Bajorek: Let me have Bernard Dieny summarize his background and how you ended up at IBM.

Dieny: Yes. I am very glad to participate in this interview and have also the opportunity to see old friends with whom I had great pleasure to collaborate. I am French and I was born in Paris in 1960 and I spent the first years of my life in Cameroon in Africa because my father was a missionary there. And just before the independence of Cameroon, we, my parents came back to France in 1965 and the family settled in, in a city in France called Le Puy-en-Velay, which is actually a city of middle size. And I spent 11 years in my youth in this city of Le Puy-en-Velay. That's where I went to middle school and high school. And I really enjoyed this time of my life here. Of course, I've been very influenced by my parents and during all of this time I was living with them, and back in France, my father was a pastor and my mother was also a social worker. But since we were a family with five children, quite quickly she stopped working to take care of us. I have a lot of wonderful memories of the way she looked after us. And, so I went to high school in Le Puy, and there I was also very influenced by a math teacher because I was not such a good student at the beginning of high school, but this teacher really helped me to progress in mathematics and also at the same time, I progressed in physics. He really encouraged me to work hard and I could feel that by working hard, I could really progress and move forward, and I found this very rewarding. And in the following of my career that's really an attitude that I really kept, to try to give the best of myself and work hard and to really get as good results as I can. So, after high school, I moved to -- I started my graduate studies and I moved for two years in Lyon, which is the third largest city in France. And then I went to Paris for three years where I attended a school called Ecole Normale Supérieure in Cachan, which is actually dedicated to the training of teachers, because at that time I wanted to become a teacher in physics or mathematics. Actually, during my youth, my plans for the future really evolved. At the beginning, I wanted to be a ranger because I really loved outdoor sports and I also loved mountaineering and rock climbing. And this is also something which pushed me to challenge myself all the time. Afterwards, when I started to be a good student, I shifted to -- I wanted to be a teacher. But when I was at the Ecole Normale Supérieure, my pofessor actually encouraged me to do a Ph.D. when I got the diploma. So, I went to Grenoble because it was also a nice place for mountaineering and hiking. So I went to Grenoble for the Ph.D., where I was supervised by Bernard Barbara, who I loved really to work with, he really gave me a very strong taste for research, and I had real pleasure during these three years to work on my Ph.D. topic which was related to random anisotropy systems. Random anisotropy systems are disordered magnets, amorphous magnets, in which there is a competition between randomly oriented anisotropy axes which tend to induce disorder in the magnetization and exchange interactions which tend to make the magnetization parallel everywhere. So, it's a competition between disorder and order, and these are frustrated magnets like spin glasses or spin ice, which are studied nowadays. So, this area of research was very interesting and helped me later on to understand the phenomenon of exchange anisotropy, the interface between ferromagnet and antiferromagnet where there is the same kind of frustration taking place along the interface due to competing interaction between positive and negative exchange interactions. But what I loved working with Bernard Barbara was also the way he tackled physical problems, because he was always combining experiments with numerical simulations and simple modeling. Numerical simulations are really a good way to guide the intuition because you can by simulation see how things evolve and you can look more carefully at some specific parameters the role of which you want to understand more closely. And also, the modeling really helped to analyze the experimental data. So, attacking a research problem with these three approaches in parallel is something that I kept on doing in the following of my career. So I completed my Ph.D. in 1985, then for two years I had to stop doing research, because in France at the time, we had the military service and since I did not want to do it, I had been a conscientious objector for a couple of years and I was working in a humanitarian association. After that I got hired as a researcher at CNRS, which is the main public research organization in France. And then as a young researcher, I wanted to have an experience abroad; I was very encouraged in that by my Ph.D. advisor Bernard Barbara because himself a few years earlier went to IBM Yorktown for a one year sabbatical and he was very positive about his own experience at IBM Yorktown. So, I had a chance in July 1998 to attend a conference in Le Creusot in Burgundy in France. It's a conference I remember very well where we drunk a lot of Burgundy wine. And that's where I met Virgil; there were several people from IBM attending the conference, including Alex Malozemoff from IBM Yorktown. Virgil, you were there. Stuart Parkin was there, too. And this is the conference where Albert Fert presented for the first time his result on Giant Magnetoresistance. Everybody was excited about this. This was really the highlight of the conference. Then when I talked with Virgil about my wish to spend a year abroad, this came in conjunction with this presentation on GMR. Virgil then proposed for me to work on GMR with him. And so back home I talked with my wife about this opportunity to go to California. She was very enthusiastic also about it. So, we moved forward and that's why later in October 1989, I arrived at IBM and started working in Virgil's team.

Bajorek: Very good, very good. So, Bruce, how did you end up at Caltech? And then how did you end up at IBM?

Gurney: Well, Chris, I was, I grew up in San Diego.

Bajorek: Ah, a native Californian? Were you born there?

Gurney: Californian. I was born in Oregon.

Bajorek: Okay.

Gurney: But moved very quickly to San Diego and really grew up there. My father was a EE electrical engineer from the Midwest and he always encouraged me to think whether it was little league or, you know, in whatever endeavor, Boy Scouts. And he was very much an enthusiast of the IEEE and was responsible for early GPS global positioning. He contributed there. My mother was a housewife and she had a curiosity about the world which she instilled in me, great curiosity. And this generated a lot of interest in me to read about science. And in middle school, I had a great biology teacher, Ed Burnham, who had a great enthusiasm for science, and he instilled that in me. And then, in grades 9 through 12, I had two excellent teachers Sally Remington and Ed Wright, who in the end arranged for me to meet with Sonny Chan, who was the Director of Student Houses at Caltech. I met with him and he encouraged me to apply to Caltech, which I was very interested in because of my interest in science. I had always wanted to be a physicist since age 10. I realized that it was physicists who, at least in my mind, thought about the fundamental nature of reality and that interested me then, it continues to interest me now. So starting at age 10, I wanted to be a physicist. Caltech was really the place to go for an excellent education and fortunately, after my interview with Sonny, I did get a chance to go to Caltech. You asked about alternatives. I considered Harvey Mudd and UC San Diego, which is, which I was very familiar with. And Caltech, I was a, I worked in the group of Gerry Neugebauer on infrared astronomy. He -- we were doing a lot of observations work, and at the same time, I assisted a graduate student in his observations. His name is Steve Beckwith and he became the director of the Hubble Observatory ultimately. After leaving Caltech, he became a professor at Cornell, and I lived with him at Cornell for a number of years. As I continued my work to assist the infrared astronomy group at Caltech, I built and fabricated Fabry–Pérot interferometers and spectrometers that made spectroscopic measurements of various filters for that group and realized that I had a real interest in experiment compared to theory. And ultimately, when looking around for where to go to graduate school, so I was a physics major at Caltech, I had a strong interest in astronomy. I thought I would continue to do infrared astronomy, but I had met with and been a co-actor in the Caltech musical with Richard Feynman. I went to him and asked him where I should go to graduate school. He said, "Oh, I think you should go to Cornell." And armed with a letter from him, I went and had a conversation with Ed Salpeter at Cornell and Ed-- After that conversation I realized that maybe infrared astronomy was not the direction I wanted to go. And so, I had a strong interest in, you know, materials and condensed matter physics. Went to work in the group of John Lee. Sorry, John Reppy, Bob Richardson and David Lee, they worked closely--

Bajorek: At Cornell:

Gurney: At Cornell.

Bajorek: Yeah.

Gurney: Worked closely with David Lee. They were the, at that time the three of them were the directors of the Micro Kelvin facility. That was considered low temperatures in those days. And I worked with Dave Lee on spin polarized hydrogen. So this is the first time that spin itself actually appears in my personal history. And I realized after a year or so that low temperature physics was not really for me. There was a starting assistant professor from Caltech named Wilson Ho, who was starting up a group to investigate molecules at surfaces, and so I joined his group. My thesis project was to be part of a project with two other graduate students, and my role was to design and build a spin polar-- Sorry. To design and build a time resolved electron energy loss spectrometer. Now EELS had been around for about 20 years and took about 20 minutes to take a spectrum. Our design was to improve the time resolution, and we improved the time resolution from 20 minutes to about five milliseconds. So we were able to begin to take energy movies of what was happening to molecules on surfaces, what sites they were sitting on, what molecules they were next to, whether they were leaning. And ultimately, that time resolved system was originally intended to look at the chemical reactions on surfaces, to look and to identify chemical intermediates during reactions, and that would require time scales on the order of microseconds. We were able to drive the speed down to milliseconds, and I pointed out that the limitation was really space charge effects and that this would mean that we would never get to the times that would be required to identify reactions during-- in real time. So, the TREELS device became a method for rapidly identifying phase transitions and following as a function of temperature and a function of concentration molecules on surfaces. And so we did for the first time see a number of phase transitions of molecules and groups of molecules on surfaces that allowed us to follow the physics of materials. I went to my very first conference at Princeton. And on the plane I met another researcher. He was at that time at NIST, National Bureau standards. His name was Bill Egelhoff and Bill told me that in his mind, the most important advances in surface science were going to be how metals grow on metals. And at the time I recall thinking that that was not of that much interest to me. Ultimately, that became the basis of much of the growth work that I did on GMR and that evolved into growth of magnetic media and the locating of defects on the surface where we could sputter individual grains of media to control size distributions and also to create patterned media. So, the two basic, you know, fundamental phenomena that would dominate the rest of my career, spin and the growth of metals on metals were part of my original education. And with the work done at Cornell on this low energy electron scattering to identify what was happening on surfaces, I realized that the one degree of freedom that we hadn't exploited was the electron spin. And at that time, spin polarized electron sources were being generated for the first time at NIST and I convinced, at least I thought I had convinced, IBM to hire me to build a spin polarized electron source to look at what happens to magnetic surfaces and interfaces during the fabrication of devices that they were then planning to use. At Cornell, I originally thought that I would go be a postdoc at IBM. It turns out that one of the legends of surface science, Thor Rhodin, was a professor and he had-- at Cornell-- and he had produced his most famous student, Joe Demuth, who had gone to IBM, Yorktown. And Demuth was able to arrange for me to have a postdoc at Yorktown. At the same time my advisor, Wilson Ho, contacted Shirley Chang at IBM Almaden Research Center. Shirley met with me because she was coming through Ithaca at the time, and she circulated my resume and eventually I was invited to interview with Ernesto Marinaro and Pantelis Alexopoulos at IBM. And ultimately, I was after some time, hired by you, Chris, and with the assignment to report to Alexopoulos. And so I began work at IBM, ultimately helping at the time, I was going to produce the spin polarized source. At that time, an IBM fellow was bequeathing some equipment to others and I wound up with a sputtering system that he was designing and starting to build. I completed that four - target sputter system and that's the sputtering system that ultimately was used to produce some iron-chromium multilayers and to see the interfaces of those GMR films to help understand the mechanism of GMR. The IBM Fellow was Kent Howard and who was helpful in helping to understand details of growth and interfaces of magnetic materials for a number of years. So that's how I came to IBM originally was through a connection that my adviser had with Shirley Chang. And then a number of other individuals became enthusiastic about using electron scattering to study metallic surfaces. And then Chris, you provided a way for me to be hired into what was then called the General Products Division ultimately.

Bajorek: It's a small world. You bring certain, all sorts of memories to my mind. And I didn't realize we'd have these conflicts of interest. Here I'm interviewing you decades later. And, it's so nice to know that we met when we did meet. And I'm so glad that I made some right decisions in my youth, right? To have hired you, Virgil, and to have hired you. And I remember writing the nomination for Kent Howard to become fellow before he was a fellow. So, it's such a small world, this is. And before we move on to Mustafa, I just wanted a couple of clarifications from you, Bernard. The small town in which you grew up, which part of France is it located?

Dieny: This is in Massif Central. Massif Central is a range of old mountains in the center of France, in a department called Haute-Loire. It's a middle-size city of about 50,000 inhabitants.

Bajorek: And when you joined CNRS, did you do it in Grenoble?

Dieny: Yes. I joined CNRS in Grenoble. It was at the Louis Neel Laboratory. I stayed at CNRS between 1987 and 1992. And when I was back from IBM, I actually changed from CNRS to CEA, which is another public research organization in France, but more technology oriented. I was still in Grenoble and later I founded this laboratory called Spintec. But I can explain more about this later.

Bajorek: We'll get back to that. Mustafa, how about yourself?

Pinarbasi: Okay. So, I was born in a small town called Koyulhisar in Turkey. The town is toward the center of Turkey, close to the Black Sea. And my father was a civil servant, my mother was a housewife, we had just enough to get by. We had fields and gardens and we worked in the gardens and fields. But education was to my father, to my parents was number one priority always. In fact, all my siblings, we learned how to read and write from my dad before they taught us in school. Every school year we would get, for example, clothes, new clothes like you do here at Christmas time and so on. For us, it was always the beginning of the school, I would get a new suit. So it was that important to him. I went through the middle school and there was no high school in my town when I was in middle school. I was always fascinated with the big cities. I wanted to explore and learn new things and it was my way out. When I got to high school, I was going to go to the big city. Not going to school wasn't an option. And my luck, they decided to open a high school the year that I was going to go to high school. I was wishing that that was not going to happen, but it happened. I told my dad that I don't want to go to high school here, I want to go to a better high school in a big city. I did not register in high school for about 10 days. And the principal saw my dad on Friday and at lunchtime, he said, "What is Mustafa doing?" And he said, "Well, he wants to go to a big city high school". And then the principal told him that he better come and register this afternoon because it's the last day of registration, otherwise he will lose a year. So, I had to go and register that afternoon. Monday comes around, I still cannot go to school. There's a hill. I went to the hill and watched the yard, the schoolyard. At about the third period my class comes out and they start playing volleyball, and that's when I decided I could not stay. I went there and played volleyball. The next class was the French. So, I attend French and when the break came-- of course, the French teacher gave me a hard time for not being there the week before. And the next period, and then they said, "The principal wants to see you." I said, "Ooh. Now he's going to-- He's going to be upset. He's upset." And along the way, a couple of the teachers were really smiling at me and were very happy. And then when I went to the principal's office, he told me I actually, I took an exam before, the year before, that I got a full scholarship to a boarding school, in no other place than Istanbul. So that was my ticket to go to the big city. I went to a boarding high school in Istanbul and then from there I went to college at Istanbul Technical University. That's where I got very interested in the materials science. And after graduating from the University, I got a full scholarship to study for a master's and Ph.D. in the United States. And so, the thing was that I learned French throughout school. I had no English. But the good thing was that my scholarship covered one year of learning English in the United States as well. So, when I came to United States, I knew a few words of English, and the two I remember were, "thank you" and "no smoking" that I learned from as the ships go by, you see the sign. Then I learned English at the Intensive English Institute at the University of Illinois at Urbana-Champaign for one year. Then I had a quarter when I went to Michigan Technological University and then I was accepted into the program that I wanted at the University of Illinois. So, after spending a quarter at Michigan Tech, I came back to the University of Illinois for my master's and Ph.D. For my Ph.D. work, my advisor was John Thornton and he is considered as the father of sputtering and the creator of the very famous Thornton Zone's Diagram. So, I studied thin film physics and electronic materials. That was his specialty as well. Specifically, I worked on the amorphous silicon thin films and what we were looking at was the hydrogen bonding to silicon in amorphous silicon. One of the things that we were looking was hydrogen bonding. When you don't have that, you have some of the silicon that is not bonded, then you have what's called a dangling silicon bond. And it creates defect states in the band gap that has a detrimental effect on the photoelectric and photovoltaic properties of the silicon and at that time, there were thin film transistors that were being made using amorphous silicon as well. And so that is what I have studied, really, how we can actually bond hydrogen, how you can make it stable and how you can actually get the thermal stability of the bonding. And so, you can actually eliminate these defects states from the band gap of the amorphous silicon films. Unfortunately, my advisor passed away before I graduated. In fact, the last conversation I had with him was that "Mustafa, we need to start and get your thesis defense ready. And start work writing up your thesis." And that was the last conversation I had with him. It was a great loss for me because he was my mentor and I have not met anybody who has really dedicated and devoted his life to science. There was only one thing in his life, that was science. He had no kids and had great respect for everybody who really did good work and contributed to science. And as an example, I remember one Friday late I was working in the lab doing experiments and I saw him coming in and he was walking on his toes. I said, "John, why are you walking on your toes?" And he said, "Mustafa, I don't want to disturb you." Here his graduate student, he doesn't want to disturb. This is how much he respected people who were actually doing good work and advancing science. But I was about almost a year and a half away from my graduation. We were collaborating at that time with Princeton University on the project along with Penn State. And so, he did actually talk to those professors, Sigurd Wagner from Princeton, Steve Fonash and Chris Wronski, actually there's an effect called effect called the Staebler - Wronski Effect. These are the professors who helped me finish up my thesis. They knew all the work that I was doing. I ended up having a professor called Mark Kushner from Electrical Engineering at the University of Illinois, who became my advisor. I graduated in 1989 and I interviewed at a number of places at IBM. One day I was going back from the interview and I traveled to Santa Cruz, I had time. Then when I went back to Illinois it was negative 20 degrees and everything was frozen. That's when I decided I was going to come to California. I was not going to go to Yorktown Heights. And then Bob Schwenker, whom you know very well.

Bajorek: Yes.

Pinarbasi: And he offered me a job in the development, thin film head development and I joined IBM in 1989.

Bajorek: '89?

Pinarbasi: 1989.

Bajorek: Very nice, very nice. Well, terrific. That's a -- I appreciate all of your summaries and interesting backgrounds. And three of the four are international backgrounds, right. It's-- And yours, Bruce, also. Well, on a scale of the world, right, Midwest, Oregon, California. Quite a few moves. Virgil, let's come back and pick up. So, why did the world care about GMR? What was GMR? Can you can you pick up from there and take us forward?

Speriosu: Well, actually, I want to take it back to what I did at the very beginning, sort of to an introduction to the GMR.

Bajorek: Okay.

Speriosu: It's a pre-- It's a prelude to the GMR. So, soon after joining IBM, I became interested in the phenomenon called exchange anisotropy or exchange biasing and it appealed to me because there were some physics type of questions that were unresolved. The magnitude of the effect was quite a bit lower than expected by very simple-minded models. So I thought this would be intriguing, an intriguing area to look into. But in terms of applications exchange biasing was interesting to recording heads as a means of suppressing domains in the AMR sensor. And this AMR sensor already was about to be introduced in a commercially marketed head. And you and Dave Thompson and others had something to do with that at the beginning. So, I was interested in this, and I approached Stuart Parkin, who had preceded me at IBM by a couple of years and he had constructed a custom sputtering machine that could grow multilayers of up to four different compositions, with monolayer accuracy and reproducibility. So I proposed this problem: let's look at the question of exchange anisotropy in permalloy – iron manganese. And we spent some time. He grew the structures. I measured them using FMR ferromagnetic resonance, looking at spin waves in the permalloy and trying to deduce something about the nature of the interface by the spin waves, excited inside the permalloy. And also from doing Torque Magnetometry on the system and trying to understand what's going on in the antiferromagnet. And one interesting feature there at the very-- from the very beginning was the strong temperature dependence of the exchange bias field. Exchange bias is a shift of the BH loop of the permalloy away from the origin, as if in another field applied to it. And the behavior of this shift indicated that there were almost uncoupled regions in the antiferromagnet that were acting independently, each one with its own thermal activation energy. There was a distribution of what is called blocking temperatures. So this leads to an interesting time dependence of the exchange bias field in the presence of opposing fields, such as demagnetizing field. And it leads to a reversal of the direction of the exchange bias over time. And later this became and it still is a question of interest in this whole industry. So, I talked about this because the structure that is of interest here today, the so-called spin valve, uses this antiferromagnet and ferromagnet couple. So, I was working on this question prior to the announcement of GMR. So, now I'm going to tell you why GMR is important. GMR, of course, was a discovery in 1988 by a French group, Professor Fert and a German group, Peter Grünberg, Professor Grünberg. And they independently did work on iron-chromium multilayers, which they had grown by molecular beam epitaxy and found, to most of the world, unexpected results. Perhaps they were not unexpected to people like John Slonczewski because he had already foreseen that perhaps 15 years earlier. But it was news to the world. So, the discovery of GMR, giant magnetoresistance, was very exciting. Prior to that, there was a lot of questions and interest in exploring magnetic metallic multilayers and expecting to discover new interfacial phenomena and looking for collective phenomena from a multilayer system, magnetic system. So, the entire community that I was a part of was kind of waiting for something very exciting to happen. And this is what happened, the GMR. There was work on multilayers going years, years in the past and they produced interesting results about anisotropy and various surface phenomena, but GMR was the most exciting one that got everybody quite interested in it. So why is GMR interesting? From a sensor point of view from a head manufacturer such as IBM, it was very interesting because you always need more signal and IBM was already using magnetoresistive sensors for heads and disk drives. So, since I was hired by IBM to look at problems related to IBM's technology, it was very appealing. But scientifically speaking at the time, this was iron-chrome and it was not clear whether the behavior, this antiparallel coupling that happened across the chromium was mediated by the anti-ferromagnetic chromium, because that's all that was known, that chromium was known to be anti-ferromagnetic in bulk; so perhaps this was just not such a big deal because the chromium already was anti-ferromagnetic. And if you put iron-chromium-iron, it's not a great surprise that you're going to see an anti-ferromagnetic transmission across it, at least that was my interpretation at the time. The GMR, however, was unexpected as far as I was concerned, and it was very large, up to 20 percent, let's say a factor of five larger than what had been seen before in bulk behavior. And … however, the very large fields, 20,000 Oe, roughly 1,000 times larger than is useful for a head, were quite daunting, quite a barrier to application. So, this was an exciting result. But I did not start working on it for some time because I didn't have the-- I thought, well, this MBE is and, it took, really it took Stuart Parkin to come back at IBM to reproduce results that were obtained by MBE in France and Germany. So very soon after this presentation, this announcement, in a few months, Stuart Parkin working alone-- not with me, I had nothing to do with that-- repeated and extended that. And importantly, it was by sputtering, it was not molecular beam epitaxy, and sputtering meant, of course, that the costs of production were considerably lower and were going to be lower. Now, this became a possible application in a product. MBE was just prohibitively expensive for recording. So, Stuart Parkin did that. He showed: yes, there is GMR in polycrystalline iron-chromium films. Very exciting. And beyond that, he showed a new behavior, the fact that the coupling, which appeared to be strongly anti-ferromagnetic across the chromium, in fact was oscillatory, and he was very excited about that. And also, he discovered that you could do a cobalt and also cobalt - ruthenium instead of chromium, --

Bajorek: If I could just--

Speriosu: Yeah.

Bajorek: Stop you for a second. For the audience in the future, can you clarify what oscillatory means?

Speriosu: Okay, oscillatory coupling, yes, thank you.

Bajorek: Yeah.

Speriosu: It means that as the thickness of the spacer layer varies, whether it's chromium or at that time ruthenium was known, just those two, that it changes sign.

Bajorek: That's the coupling between the magnetic films.

Speriosu: So the coupling of-- Right. The coupling of the two ferromagnetic layers across the spacer, which in one case was ruthenium-- not ruthenium, chromium initially and then ruthenium. It oscillated as the thickness of the spacer was varied.

Bajorek: Oscillated on being positive and negative.

Speriosu: And negative.

Bajorek: Positive--

Speriosu: And going through zero.

Bajorek: Yeah.

Speriosu: So that indicated a more complex phenomenon than simple mediation of a bulk-like behavior across an anti-ferromagnetic chrome. And just the evidence that ruthenium had also the effect was very strong. So, it certainly motivated a lot of new thinking about collective phenomena across these multilayers. Now, from my point of view, I was interested, of course, in seeing whether the GMR and the coupling were independent of each other. I was interested in doing experiments that would demonstrate the phenomenon at much lower fields. One kilooersted is way too much. Even 20 kilooersted is way too much-- way too much, of course. So, I was interested in exploring if it is possible to observe this kind of a magnetic (magnetoresistive) effect without the coupling? And the fact that Stuart demonstrated you can do it by sputtering was an inspiration to me.

Bajorek: Just a question. Were Stuart's results also requiring high fields to do the--

Speriosu: Yes. Yes, they did.

Bajorek: Okay.

Speriosu: They were in the kilooersted--

Bajorek: Consistent with the--

Speriosu: They were consistent but--

Bajorek: MBE-type results.

Speriosu: Since they went through zero, since the coupling oscillated, there were particular thicknesses of the spacer where the coupling would go through zero.

Bajorek: Yeah.

Speriosu: But to my thinking at that time, I was looking for basically no coupling. And I posed the question, well, how can you just separate these two phenomena? You have the coupling and you have the magnetoresistance. So, you put in, your two arbitrary ferromagnets and you put something in between. And now you vary the orientation of the magnetic moments. Will there be a magnetoresistance effect then in the absence of any coupling? That was the question that I posed. I'm sure a lot of other people asked the same question, but I was-- I was interested in that. So, I knew of prior work that happened in the 1970s, 1975 in tunneling junctions. Now a tunneling junction is two ferromagnetic layers: they were cobalt, an iron and they were separated by a germanium oxide layer. So, the current was passed perpendicular to the junction; whereas in all the previous work, I just talked about, the current is in the plane of the multilayer, it's in the plane. This is perpendicular to the plane. But it was done earlier. And so Julliere had discovered an effect in 1975 at low temperature. It was magnetoresistive and it was very indicative and suggestive that this is a broad phenomenon. And, and the reason why he was able to do that is because the iron and the chrome and the cobalt layers had different coercivities. So, when the ferromagnets have different coercivities, the application of a field causes one to switch before the other and you can change the relative orientation of the magnetizations as the field is scanned and they go from parallel to antiparallel and back to parallel. So, he showed, Julliere's results were very clear, very indicative. And also, I became aware of the work, theoretical work of John Slonczewski, who at that time explained the effect. He provided a theory of this phenomenon having to do with the extension of the wave function of electrons across into space or across a particular nonmagnetic spacer. And he called, John Slonczewski called this the magnetic valve effect. I don't think he called it a magnetic gate. I think he called it a magnetic valve, but I could be wrong on that.

Bajorek: I think so. I think you're right.

Speriosu: Okay. So I'm saying that because later I called our structures *spin valve* -- and that was inspired by the *magnetic valve*. I mean, it's not just a sexier name, I thought, but actually because it was metallic and it was in the plane, so that was a good reason to do that. Also, it was different. So, this was kind of a brewing idea, not only just to me. There was also Daniele Mauri at IBM at the time. Stuart was working on coupling. He was not working on this at the time. He was doing oscillatory coupling. He was very excited about that. And I had a-- I had a good friendship with Daniele Mauri. He was also interested exchange anisotropy on his own and we talked about things. So, I had this very good discussion, conversation with him one day, and it inspired me, and I said, "Okay, we have to try this." So, I went to Stuart and I said, "Let us build a structure that has two permalloy layers with a spacer. And let's pin one layer with iron manganese. And Stuart liked the idea and he agreed. And we decided to do two spacers, tantalum and copper, tantalum because it was available. It was studied and I didn't know what to expect on anything, so let's try that first; and copper. And it's interesting that, I mean we had a close enough collaboration that Stuart was going to be out of the country, and he … so he allowed my technician, Omar Need, to run his machine. So, Omar did this run, a sequence of samples with tantalum and a sequence of samples with copper, and we varied the thickness of the two spacers. And so, Omar Need gave me the samples and I measured the tantalum, I measured them both and tantalum behaved as I expected. Two separate loops uncoupled. But there was no magnetoresistive effect. It was just the normal AMR effect in the two separate layers. And then I looked at the copper spacers and the copper BH loop looked very strange because the two -- the two layers instead of being separated, were rather coupled across the copper. And so, both the magnetoresistive transfer curve, and the BH loop did not look like anything that I was expecting to see. So, I said, this is -- this was -- this has failed. This didn't work. There's something that didn't work here. And I told Stuart and I told Daniele Mauri. I told you, Bruce. I told my manager, you know, "Look, I tried this idea. It failed." So, I set it aside and did other things for a while. And then just nine months later -- I was not a manager then. Nine months later, I was made a manager and so I was given a group and both Bernard and Bruce and several other RSMs. But there, you know, it was Tadashi Yogi and Stephen Lambert had joined but Tadashi was a media person, and Stephen Lambert was a testing person, so I was more intellectually closely related to Bruce and to Bernard in our interests. And a number of technicians with Dennis, you know, Dennis Wilhoit, the engineer, and Omar Need. And so when I got this group, Bernard arrived and I asked him to work on GMR. And I suggested that he look at soft/hard materials with a spacer such as silver, because I thought copper wouldn't work. I tried it and it didn't work, so I did not suggest the spin valve. I never mentioned it to him. I never mentioned this to Bernard. He didn't know. But as soon as he got onto this, he got very exciting results immediately. Well, maybe not immediately, but to me, it seemed very quick that he got a GMR effect in soft/hard, multilayers. And then very quickly, within a couple of months, I was also out of town and so Bernard on his own, he thought of the spin valve again. So, he re-invented it and this time he tried it on his own and it worked. So, when I came, he showed me these breathtaking results. So, at this point, I'm just going to let him talk about it.

Dieny: Yeah.

Bajorek: You remember those days?

Dieny: Yeah, yeah. It was a very exciting time. I actually remember very well my stay at IBM working with Virgil's group and our colleagues there, yeah.

Bajorek: Just before we go, one little-- Who was your manager before you were made--

Speriosu: Yeah. So, Ian Sanders was my manager.

Bajorek: Ian Sanders.

Speriosu: Prior and what happened is that we both got promoted. He got promoted to second line--

Bajorek: Second.

Speriosu: And I became first line.

Bajorek: Okay. Okay. Thanks. I just--

Speriosu: Yeah.

Bajorek: Wanted to capture that.

Speriosu: But he was still my manager.

Bajorek: Good, good. Sorry, please.

Dieny: So, I arrived at IBM in October 1989 and I remember very well of my-- our arrival because it was actually a couple of weeks after the big earthquake, which struck the Bay Area in in September 1989. So, we had to postpone a little bit our arrival, because even the airport has been closed for a few days. And Virgil was there to welcome us at the airport with all our luggage. It was an impressive move, yeah. But I am very grateful that you were here. Because we moved with our three kids, so it was already quite a big move for us. But I started working at IBM one week later and I was very motivated by this topic of searching for GMR materials responding at low fields. But I was not at all a specialist of transport properties at that time because my Ph.D. has been on, as I said, on random anisotropy systems, so I did during my thesis only magnetic measurements and simulations, but no transport measurements. And later on, before coming to IBM, I started working on the magnetization process in magnetic multilayers, so I was aware of the phenomenon of antiferromagnetic coupling and I also read papers about exchange bias before coming to IBM. But all this area of electron transport was new to me. So when I arrived, I had to really read a lot of papers in order to get acquainted with the fields and in particular, papers from Albert Fert and Ian Campbell who had been working on spin-dependent scattering in metallic magnetic alloys since the 1970s, published a lot of papers between 1970 and 1990 on the topic of spin dependent scattering in magnetic alloys. So, I read all these papers. Of course, I studied the papers on the antiferromagnetic coupling which was observed by Peter Grünberg a few years before. And I studied, of course, also the papers on Giant Magnetoresistance. But I had also to be trained on the various systems and tools that I had to use, so I got training on the sputtering tool. I got training--

Bajorek: With Stuart's, Stuart Parkin's tool or the group’s tool?

Dieny: No, no. This was a different one. Yeah, a different tool which was in the clean room of Virgil’s labs and, actually, this tool was initially built to -- used to make, to study hard disk drive media, so it was using a planetary rotation of the substrate holder. And this was not very convenient for this study because we could only make one sample per pump down because of the planetary rotation. So basically, we could make one sample per day or maximum two samples per day and this was limiting actually our investigations, I will come back to this. But, so quickly, I started doing experimental work using this tool for deposition. But also, I got training on the VSM on the BH Looper, on the profilometer to measure the thickness of the layer, how to measure the magnetoresistance, all these kinds of things. And Virgil, as he said, encouraged me to start working on double coercivity systems. So, I tried several things like with iron, cobalt and silver as a spacer layer. And we got some very interesting results but it was a bit difficult to really identify the-- what the magnetizations were doing in these two layers because they were a little bit coupled and the switching at the coercive field was not steep, it was kind of a rounded transition, so the two hysteresis loops were somewhat overlapping. But we could see a kind of maximum of resistance at the overlap between the two hysteresis loops. But it was not very, very clear and we were always a bit uncertain about the results. And actually, the problem was with the sputtering tool because it was too slow to make only one sample per day or at maximum, two samples per day. Fortunately, there was an engineer in the group, Dennis Wilhoit, with whom we discussed this problem. And we decided to modify the sputtering tool to completely suppress the planetary rotation and introduce a stepping motor which allowed us to correctly control the position of the substrate holder. We also changed the shutters with electromagnetic shutters, so we could control the duration of the deposition of each layer. So, this took about a month to completely modify the machine. It was quite fast actually. And a student also helped with all the coding to control the machine

Speriosu: David Peterson?

Dieny: Yeah, David Peterson, yeah. And then thanks to this change, we could then produce eight sample parts per day, which was much more efficient. So the pace of progress increased correspondingly. But one of the limitations of the tool was that we had only four targets and for spin valve optimization, this was not much. When spin valves had been optimized at least 10 to 12 different elements were used, so to start with four targets was really kind of—

Bajorek: Limiting. Quite limiting, yeah.

Dieny: Indeed, kind of short for optimization. But nevertheless, with this new tool I could progress and get many more results. Initially I kept on working on double coercivity systems, and I remember getting nice results with a sandwich consisting of permalloy, so nickel-iron alloys, copper spacer and nickel-cobalt alloys. And nickel-iron and nickel-cobalt alloys have different coercivity and since they are relatively well-behaved materials with well-defined uniaxial anisotropy, and low coercivity, the hysteresis loops were a bit squarer. So here we could better see the hysteresis loops, the two separated hysteresis loops with a well-defined plateau of antiparallel alignment between the two coercivities. And when we measured the magnetoresistance, we clearly saw also a step in resistance corresponding to each transition corresponding to the coercive field that we were seeing with the BH Looper. So there was here a clear indication that we did not need the anti-ferromagnetic coupling to get the Giant Magnetoresistance and that Giant Magnetoresistance was only due to the relative change in the orientation of the magnetization of the two magnetic layers, provided you could control this change of orientation.

Bajorek: Could I just stop you a second?

Dieny: Yes.

Bajorek: Could this have been the world's first successful observation of the spin valve effect?

Dieny: This was not yet a spin valve. It was only a part of what we later called a spin valve, this exchange biased structure that will be described later. Here it was a double coercivity system. Various groups in the world worked on double coercivity systems and in particular some results were already obtained also at IEF, a laboratory in Paris, by Claire Dupas and his team. And a professor in Kyoto was also working on double coercivity systems but in multilayers using permalloy and copper: Professor Shinjo. Professor Shinjo was developing permalloy- copper-cobalt-copper multilayers in which he was observing also some sort of peak of magnetoresistance between the coercivity of the two layers. But again, it was not with well-defined plateaus and steps, well-defined transitions. Later, spin valves were the first systems to exhibit quite sharp transitions and in which the plateaus of parallel and antiparallel configurations were clear to identify. So, I continued working for a couple of months with these double coercivity systems, but the magnetoresistance was still relatively low. And we noticed when we were cycling these kinds of double coercivity systems several times there was a gradual demagnetization of the hard layer due to some sort of magnetostatic interactions between the domains in the layers and the magnetoresistance was kind of decreasing. So, before coming to IBM I had discussed with Virgil about exchange bias and in particular during a visit that I did at IBM in the fall of 1988. He told me about his work on exchange bias. And so I remembered this idea that you can get a loop shift with exchange bias and I thought by myself also, as Virgil said, that maybe we could use the exchange bias to pin the magnetization of one ferromagnetic layer and associate to it a non-exchange biased second layer to be able to get two separated hysteresis loops. So, I decided to introduce an anti-ferromagnetic target in the system, iron-manganese. And then I started growing these kinds of samples, like permalloy which is a nickel-iron alloy, copper spacer, nickel-iron alloy and iron-manganese, the permalloy layer in contact with iron-manganese being the exchange biased layers. And since I could make eight samples in the same pump down, I was systematically varying the thickness of one layer and for the first attempt I decided to vary the thickness of copper from 10 angstroms approximately to 40 angstroms. And then when I measured with the BH Looper, the loops of these various samples, I could see clearly the two hysteresis loops as soon as the copper thickness was large enough to decouple the two ferromagnetic layers. And this decoupling was observed typically above 22 angstroms of copper. And when I measured the resistance loops, I clearly saw the well-known shape that we now know as characteristic of the spin valve response. And so in this very first series of spin valve samples we could already observe the spin valve typical response with the two separated hysteresis loops. When I got these first results, Virgil was on a trip and he came back maybe two -- two days later. I showed him the results and immediately he became very excited about them as I was. And he showed me the results that he got the year before which did not work. But by looking at the evolution of the properties versus the copper thickness, we could immediately understand that, indeed, in these samples prepared a year earlier, the copper thickness was too thin.

Bajorek: It was below 22 angstroms?

Dieny: Yeah. So, the two layers were too coupled to really get the antiparallel magnetic alignment between the two layers. So, we both were very excited and also the whole team when we got these results and we showed them to Ian Sanders, the second line manager, who was also very interested. And so, we were very stimulated then to keep on working on this. And so later I made a systematic study where I varied, the thickness of each layer one by one and I could then optimize the structure, but with only four targets in the system. So we had one target for the magnetic layer, one copper target for the spacer, one for the antiferromagnet and most of the time we needed a buffer layer because permalloy does not grow well on the silicon oxide, so we needed some tantalum to have a good growth of the structure. So basically, we could only load one magnetic material at a time, so we investigated permalloy, then separately we investigated cobalt, we investigated nickel, and we found, for instance, when we changed the magnetic target from permalloy to cobalt, the doubling of the GMR amplitude of the spin valve was observed, from 4 percent to 9 percent. So, it was already showing that cobalt was providing much stronger spin dependent scattering than permalloy and that it would be interesting to use cobalt in a spin valve structure. We also looked at the influence of the nature of the nonmagnetic spacer. So we compared copper with silver with gold. We noticed also that the minimum thickness to get decoupling between the ferromagnetic material was quite dependent on the nature of the nonmagnetic spacer. And, for instance, with silver which tends to form islands during growth, we needed a much larger thickness of silver to get good decoupling between the magnetic layers. So, we did a lot of studies like this with different materials. But as I said, we could not have two different magnetic materials in the system, so we were a bit limited in the kind of experiments that we could do with this sputtering tool. But we also made a lot of measurements as a function of temperature, between 77 K and room temperature. And I remember using a Joule-Thompson refrigerator that Virgil provided to me for these kinds of measurements but it was a nightmare because the device was always clogging with ice formation and so it was very difficult to get a full curve from room temperature to liquid nitrogen. So sometimes I had to redo these kinds of experiments several times. But what came out of these experiments was a clear correlation between the rate of decrease of the magnetoresistance with temperature and the Curie temperature of the magnetic element. When using cobalt, which has the highest Curie temperature, the decrease of the magnetoresistance with temperature was much slower than when using permalloy or even more nickel, which has the lowest Curie temperature among these three materials. One thing we did also was the measurement of the angular variation of the magnetoresistance and this was the first measurement of this angular dependence of the GMR. And so, I used for this a set of two orthogonal Helmholtz coils that Virgil provided to me for this measurement. So, the idea is that the hard layer, the pinned layer was fixed in a given direction and the free layer magnetization could be rotated with the rotating magnetic field. And I was measuring the conductance of the structure as a function of the angle between the two magnetizations. And we clearly saw that there were two contributions in the signal; one contribution, having a cosine dependence coming from the Giant Magnetoresistance; and another one having a cosine squared dependence on the angle between the magnetization and current which was coming from the anisotropic MR. So, from all this knowledge that we acquired during this one and a half year, we could actually file a number of patents about how to properly bias a spin valve, how to combine anisotropic MR with GMR to actually maximize the output signal of the head. To use cobalt also in the spin valve structure because this was enhancing the GMR signals. So, I think we filed about five important patents. And also, with Bruce and with Peter Baumgart, on different aspects of spin valve structures. But we also published five or six papers during this one and a half year. So, for me, the output of this stay at IBM, has been really exceptional. And during this stay what I enjoyed very much was to see the interest of all the colleagues at IBM in what we were doing. And really, we could see the benefit for read heads of these spin valve structures. So, within this one and a half year, the engineers working on head development already started testing spin valves in read heads and for me, coming from basic research, it was extremely stimulating to see my work going so quickly to applications. And in the rest of my career, I kept on working in research fields at the boundary between basic research and applications because I found this very interesting during my stay at IBM. Otherwise, after these one and a half years, I had to go back to France because I had a permanent position in France at CNRS. CNRS accepted initially that I go to IBM for one year. They agreed to extend this sabbatical to one and a half years. I actually got an offer from IBM and Virgil introduced me to Heiner Sussner who was head of this whole R&D Center. And Heiner and Virgil proposed me a permanent position at IBM, but I did not want to lose my permanent position in France, so with the whole family, we finally decided to go back to France in March 1991.

Bajorek: Help me pin down a couple of things. I don't want to exaggerate anything nor minimize anything. I just want to make sure we take proper credit here. I jumped the gun when I suggested your double coercivity work was spin valve work. That wasn't right. But later you did do experimental structures that were very close to the spin valve, right, the ones you described? Were those the first measurements in a spin valve configuration to the best of our knowledge?

Dieny: With exchange bias, yes, it was.

Speriosu: I think there's a bit of confusion. Let me just explain something.

Bajorek: Yeah, yeah.

Speriosu: When we discovered this, we thought this should be called the spin valve effect. We said, "Forget about GMR. This is not giant. Why call it Giant Magnetoresistance? Let's call it the spin valve effect." So actually, it is the same effect. So, when you say it's a spin valve effect, in our minds the way we proposed it and we wrote a few papers like that, we tried to get the community to adopt this term, but the community eventually did not. So, so--

Gurney: If can jump in and just say--

Speriosu: Yeah.

Gurney: That the community eventually did name that device.

Speriosu: The spin valve.

Gurney: Not with the exchange bias, but just the two different coercivities. They called it a pseudo spin valve--

Bajorek: I see.

Gurney: Later on, after we introduced the term spin valve, in retrospect, in order to give that device without an anti-ferromagnet, without an exchange bias layer a name, they called it a pseudo spin valve.

Bajorek: Maybe I'm not expressing--

Gurney: So it relies on the same kind of physics.

Bajorek: Yeah.

Gurney: Having to do, as Virgil described, the relative orientation of one of the magnetizations of one of the magnetic layers with respect of the magnetization of the other layer and the transport of carriers between them. So that spin-valve effect is present in pseudo spin valves, --

Bajorek: Yeah, and--

Gurney: And thus, the name was given to include the phrase spin valve.

Bajorek: Spin valve.

Gurney: But it's-- but a pseudo spin valve is not a spin valve in the sense that technology uses it. We wanted to have the stability and the-- a number of other benefits that the exchange bias provides that--

Bajorek: Yeah and I appreciate very much the clarification. What I was after was, it seems to me that to put it for practical use in a head, right, you need it to be able to vary the angle between the two magnetizations a controllable way, right, predictable way. And I'm, what I'm trying to drive it is at what time did we see--

Speriosu: That was it.

Dieny: Yeah.

Bajorek: Come together the elements that later enabled, right--

Dieny: Yes.

Speriosu: Yeah.

Bajorek: What we call the GMR Head?

Speriosu: Well, that--

Bajorek: Because any--

Speriosu: That was the measurement of the angular dependence.

Bajorek: Yeah and-- and--

Speriosu: So, I--

Bajorek: And when I'm driving at, were you the first team who put-- who made that happen in the world?

Speriosu: Yes. Yeah, yeah.

Dieny: Yes. Everywhere.

Gurney: Yes.

Speriosu: Yes.

Bajorek: And at what point in time? I want to know the date, if possible, as accurately as possible. When did you observe the data? Where did you obtain the data where you could actually control the angular dependence?

Speriosu: January and February of 1990.

Dieny: Yeah. Yeah, yeah. That's it, yeah.

Speriosu: January, 1990.

Dieny: Yeah. I think it was around February 1990.

Bajorek: Okay. Sorry. That's what I was trying to--

Speriosu: Yeah.

Bajorek: I think it's-- It seems to me it's important to clarify because that was a very important milestone, right?

Speriosu: Yes.

Dieny: Mm-hmm. Yeah.

Bajorek: I appreciate the elegance of the dual coercivity, but I don't know how to use the dual coercivity if you get a head right by itself.

Speriosu: Yeah.

Gurney: There were a number of companies who were founded based on making sensors, magnetic field sensors using pseudo spin valves.

Bajorek: Yeah. Yeah, yeah.

Gurney: Which was-- which is a different application of the overall effect and--

Bajorek: But I was curious about the core thing, which is when do we see the data that was clear, you know, we put together, right, the elements that were essential, right, without which you could have never made a head, right?

Speriosu: Yeah. So, I want to-- Something I skipped over at the beginning of my remember-- my memories. I want to describe in what way the superiority, the advantage of this concept of having two soft layers and to begin with they are both soft. And now you could get a hold of one, and you hold that one and you can controllably rotate the other one. That's what the spin valve does in concept. And you do not require any coupling across the spacer. And you therefore can vary thicknesses both of the spacer and what we call the pinned layer and the free layer. Moreover, this structure allows you to measure GMR even when the coupling is parallel. Because previous work with antiparallel, antiferromagnetic coupled multilayers, the only way to get them to rotate in a field is if they are coupled antiparallel because they both respond. They have to go like this. But if you're holding one, then regardless of the coupling, you can measure the effect. So, one of the questions that we addressed was is the magnetoresistance oscillatory in itself as a natural effect? That was not-- that was unknown. It was not clear from the data, from …everybody talked about oscillatory magnetoresistance, oscillatory coupling. So, we were able, using the spin valve structure in 1992 or '93-- probably '92. I don't remember the exact year-- to observe oscillatory coupling across the copper and to measure GMR across that spacer, even for parallel coupling across the copper. So, it's a powerful structure. It's very-- it's versatile. That's its beauty.

Bajorek: Bruce. It would be nice now to have you fill --

Gurney: So, I--

Bajorek: Fill us in on your work with the group.

Gurney: Right. So, Virgil and Bernard have described how they were working on creating a structure, a device that would be useful in magnetic recording. My interest was in, and they pointed out that the magnitude of the effect seemed to be a bit smaller, maybe by almost an order of magnitude smaller than the Giant Magnetoresistance of multilayers. So my interest was in understanding the physics of and the mechanism of Giant Magnetoresistance itself. I described the sputtering tool that I inherited and completed from Kent Howard. What I did with that tool, that sputter tool, was to make samples in which I modified the interfaces of the, say, iron-chrome multilayers, whichever GMR material I was looking at and attempted to connect the scattering mechanism to the materials that were introduced at the interfaces. So Bernard mentioned some work of-- previously done by Campbell and Fert, describing and measuring the spin dependent scattering characteristics of different impurities in magnetic metals. And I introduced some of those same materials at interfaces to try and see whether the effect was an interface effect or not, and had some early success doing that. Parenthetically, I'm going to talk about a postdoc that we hired named Peter Baumgart. At the same time that I was working with these two individuals on GMR and working with trying to understand the mechanism of the scattering and Giant Magnetoresistance, I was also interested in another phenomenon which was clearly going to be the evolution of recording heads. And that is that devices were going to be made smaller and smaller and smaller because a lot of the value that the hard disk drive industry was providing customers was continuing to put more and more data on the same format size. So, for us, this meant improving areal density of data written and therefore data that needed to be read, so devices were going to be made smaller and smaller over time. And one of the ways of characterizing magnetic materials is with Brillouin Light Scattering. It turned out that Peter Grünberg was a BLS, Brillouin Light Scattering, expert and had been doing-- using this technique of scattering light off of magnetic materials and looking at the spin waves that are generated and the associated energy loss in light, that means a little frequency shift. He had been doing this for decades. And he had been and described in papers searching for this antiparallel coupling state and I just speculate that part of his interest in creating these structures which eventually had GMR, is that he was trying to create the magnetic state that he wanted for Brillouin Light Scattering. And the best way for him to measure that he had created the state that he wanted was through electricity transport and so this Giant Magnetoresistance may have just, in some sense, fallen in his lap because he was trying to create these magnetic states. So, I hired Peter Baumgart or encouraged Virgil to hire Peter Baumgart because he had a connection to Brillouin Light Scattering. I wanted to look at the scattering of light off of devices as they were made smaller and smaller because I knew that it would eventually come to a point where devices would be on the same size as typical wavelengths of magnetic excitations in these materials and that when that happened, the devices might work differently or cease to work. And so, part of Peter's mandate was to come to IBM and help me build a Brillouin Light Scattering system, which we did do and which survived the earthquake you mentioned, in large part because of an electronic vibration isolation system that we had installed with the BLS, which somehow protected the equipment through the violence of the earthquake. And I interested Peter in this question of what the mechanism of scattering was in GMR and he continued the work of scattering, of introducing scattering centers at interfaces and did some nice work to show that interfaces can and do matter. I think at the same time, Virgil mentioned that one of the huge advantages of the spin valve system was that by being able to as you described, grab one of the magnetic layers and be able to rotate the other magnetic layer arbitrarily meant that it was possible to measure the GMR effect as a function of the layer thicknesses very straightforwardly. We did do that and ultimately demonstrated that both bulk and interfacial scattering are present in GMR materials and in spin valve materials, and that you can modify the interfaces to improve the magnitude of the effect. By putting a small amount of cobalt, we were able to increase the effect dramatically and we called that nano layering, because these were nanometer size thick, layers and we were able to, both in the system that in the deposition system that Bernard used and in the deposition system that I had developed control layer thickness down to the equivalent of subatomic layer thicknesses. And this is one of the things that we wanted the rest of IBM to realize is that they first of all, shouldn't be surprised that you could do that with sputtering; and secondly, might need to invest in the right kind of equipment to be able to control sputtering on that level in order to produce the best effects. So, in those days, as I mentioned, I was mostly interested in improving the magnitude of the effect, while at the same time, others were developing the appropriate magnetic structure. And during that time, it was recognized that because this was all current electrodes and current flowing in the plane of the device that conductance, different from resistance, is a more appropriate physically relevant quantity. This was recognized by Bernard and presented in a paper that a group of us put together and showed that this is really the giant magneto conductance effect. So, it's GMC, not really GMR.

Dieny: That's for the current flowing in the plane of the structure.

Gurney: Yes, whenever you have the current flowing in the plane, which is the way that we were investigating all of these materials at the time. Current perpendicular to the plane did eventually come to both, both the GMR kind of device called CPPGMR and also to the magnetic tunnel structures that Julliere had produced first and that Slonczewski had described. I recall the naming of the spin valve a little bit different than Virgil does. But and that is that after the results that Bernard had had alluded to were known to us, a group of us got to get together at the Almaden Research Center cafeteria and sat down and said, "This structure needs to have a name so that we can, you know, refer to it easily and directly and everyone will know what we're talking about." And perhaps it was you, Virgil, who showed us all the Julliere paper and Slonczewski's paper about the Julliere paper. And Slonczewski had described this tunnel structure as a magnetic valve. And we knew that we did not have tunneling and as described it was this current transport of carriers at the Fermi level in plane in these devices. And so we wanted to-- we knew it wasn't a magnetic valve, but we wanted to give it a catchy name. And the concept that we had, spin polarized current flowing out of one of the ferromagnetic layers seemingly across the spacer to the second ferromagnetic layer made us focused very much on this idea of spin. And so, once spin valve instead of magnetic valve, spin valve was suggested. There was general agreement at this meeting.

Speriosu: It was instantaneous. Now I thought I proposed it.

Bajorek: It was at that-- It was at that meeting.

Speriosu: Yeah.

Gurney: Yeah. Yeah, so it was-- Yeah.

Speriosu: We were all there. We were all there at the same time.

Gurney: Right, right. So, it was--

Speriosu: Serhat Metin, too.

Bajorek: Serhat Metin was there.

Speriosu: He was also there.

Gurney: Yes.

Speriosu: Now so I thought I proposed it, but if you say actually you proposed it, I don't object to it. Maybe you did.

Gurney: Someone proposed it and we all agreed that was a good name.

Speriosu: Maybe he did.

Bajorek: You all latched on to it and agreed that--

Speriosu: It was instantaneous.

Bajorek: Interesting.

Gurney: Yes, everyone agreed that based on our understanding of what was going on at the time and this historical name that it was the right name to have and it had the kind of catchiness that we thought would stand the test of time.

Speriosu: A nice ring to it.

Gurney: And so far, it has, actually.

Bajorek: Very interesting.

Gurney: And so that brings us up to the point where having demonstrated this effect with an exchange bias layer and finding some way to build this structure in a four target system which Bernard had pioneered, I switched my device over to a-- excuse me, my sputter tool over to build similar kinds of devices and at the same time, you know, produced similar structures and similar-- with similar results. And so, we knew that we had something that had at the time that we named spin valve, we had a-- we were confident of a signal that was at least three times as large as anisotropic magnetoresistance. And so the obvious advantages in SNR meant that we would be able to continue the growth of areal density and the capacity of disk drives far beyond AMR. And what I recall at that meeting is we talked a little bit about what we would need to do in order to demonstrate to the company that this would be the right path to take. And certainly among the things that came up were thermal stability, magnetic stability and then stability to processing. Can we actually make this? One of the concerns we had at the time was that iron manganese, the choice for the antiferromagnet because it was well known, well studied and easy to sputter, was that having manganese in it, it was somewhat susceptible to corrosion. And I know that ultimately one of the things that was important to the product division was what is the antiferromagnet? How to process-- how to learn to process materials, how to process these devices so that they don't corrode? If you want to expose iron manganese to the air, just the water in the air would ultimately degrade the material. And so, one of the things that the announcement of our device spawned throughout the world was an interest in exchange bias, something that had not been, other than perhaps you, Virgil, had really not been studied for decades--

Speriosu: That's true.

Gurney: Since its introduction in the-- by, let's see, who are the originators?

Speriosu: It was 1956 and then it might.

Gurney: Yeah, around 1956-1957.

Speriosu: And then oxide, oxide--

Speriosu: Cobalt oxide on cobalt particles.

Speriosu: Were shifted from the origin. But I don't remember the name of the--

Dieny: Meiklejohn.

Gurney: Meiklejohn and Bean--

Speriosu: Meiklejohn, yeah, that's Meiklejohn. Meiklejohn. Yes.

Dieny: And Bean.

Speriosu: That's right. Exactly.

Gurney: And Bean. And little work had been done on that, on antiferromagnets as relates to exchange bias. Suddenly there was a reason to improve the antiferromagnet and to understand exchange bias and so groups throughout the world and we also spend a lot of time trying to look at alternatives to iron manganese. But from the point of view of optimizing thicknesses, spacers, nanolayer choices, we continue to use iron manganese. Ultimately, people would recognize that an antiferromagnet which is an oxide like nickel oxide, would be better. But nickel oxide turned out to have weak antiferromagnet properties when made thin. And so ultimately, things like iridium manganese were introduced, which had improved corrosion properties. So, the work--

Bajorek: Was that at the time contemporaneous to the work you guys were doing?

Pinarbasi: No.

Bajorek: Or was that later?

Pinarbasi: Much later.

Gurney: That turned out to be a bit later.

Pinarbasi: Much later.

Bajorek: Yeah. Because I think today's devices use iridium manganese, right?

Pinarbasi: Right. Correct.

Gurney: Yes.

Bajorek: So, like, all the state of the art TMR heads use iridium manganese. But I thought that came a little later, but you're saying--

Speriosu: But… and the awareness that the iridium manganese would be good.

Gurney: But the awareness that in terms of the things that we would need to demonstrate.

Bajorek: Okay, so you were already conscious of it.

Speriosu: Oh, yeah.

Gurney: Oh, yes, very much.

Bajorek: Okay.

Gurney: And other end, also, copper. Copper is a well-known corroding material. And so we found that carrier transport across copper from one magnetic layer to the other was advantageous in a lot of ways. But copper itself is also a highly corrodible material. And we were aware of that too. Our experience with tantalum and some other materials, so we were heavily investigating alternative spacers to see if we could find an alternative to change the actual metallurgy of the structure, of the device so that it would be more acceptable to put into products. And those are what I recall were the important things. The thermal stability, the corrosion susceptibility and the processing were the issues.

Speriosu: Also electromigration. Once we had sensors, patterned sensors, electromigration was an issue. Certainly, it was a concern to the community, the internal community at IBM. So, we did some experiments putting a lot of current, accelerated electromigration measurements. And I, actually I did those at the time.

Gurney: Yes.

Speriosu: And they were okay at the time. They passed.

Gurney: Yeah, so, once again, coming back to this theme of metal on metals and metal on metal interfaces, it was interesting and somewhat unexpected that the materials choices that we chose did not generate electromigration problems. In the IC world, putting copper and some other interconnects was known to be a very substantial issue even then and had to be overcome by in their case by proper alloying and processing. We turned out to be fortunate in that we did not have much of this problem. And so even though we were driving current through our devices, we were able to show that that the electromigration effect would be small and insignificant on the timeframes that we expected to be able to use this device in a product. I recall our target timeframe was that the device would last for a least 20 years. So that was our, again, part of our collective appreciation of what we would need to show the rest of IBM--

Bajorek: To get them to be excited about it.

Gurney: To get them to be excited, yes.

Bajorek: So I gather at this stage, you had to pick up this emphasis on showing that this could be a robust manufacturable practical device, so perhaps--

Gurney: That's right. On the face of it, the amplitude, the magnitude of the signal was so large that it seemed like a no brainer that the right thing to do was to use that, find a way to make it work. And so, we thought of it in terms of being able to drop this in instead of the AMR sensor. So we spent some considerable time thinking about the materials that were used in the AMR sensor and what processing, both in the fabrication of the layers and the subsequent photolithography of the device. How we could best mimic that and make use of the existing infrastructure to make it as easy as possible for our colleagues to accept this new device.

Bajorek: Now, during break we were talking a little bit about, this may be interesting to clarify, I remember Ian Sanders, your manager first, your first level and that second level. I tie-- I associate Ian with being very much a media-centric researcher. Right. He was very interested in the data storage layers, right, the film disks.

Gurney: Mm-hmm.

Bajorek: So How is it that this work is being done in a film disk group?

Speriosu: Well, it happened--

Bajorek: And how did you ever then attract the head people to this?

Speriosu: It happened, I guess it happened because of a combination of two things. One is IBM Research's tolerance of deviations from prescribed areas of research. There is a tolerance of that. If you like -- neglect, benign neglect. And on my part, you know, a certain personal predilection to-- this attracted me more, so I chose to pursue GMR. It was a fortuitous discovery by other people that allowed me to do something that interested me more than media. So Ian Sanders tolerated that and supported it as soon as he realized that this could work, you know. So he saw me fail. And then, you know, with Bernard, nine months later, he has, you know, the success. So I remember he said, "You know, this is persistence, right?" So, there was complete backing and enthusiasm from Ian, and but there was some resistance from the head area because we were different. But it was all in, you know, in a good, very, you know, it was a very reasonable amount of skepticism which had to be overcome. It had to be demonstrated. And I got a very early-- we got a very early supporter in Bob Fontana. You know, he saw this in the lab. You know, he was responsible for the lab where the BH Looper once was and I was showing him these transfer curves and he was very excited. So he was kind of a back door into the head department. And also Hans Zappe was, at the time as a second line manager for heads, he was also very receptive. But I think it needed the necessary amount of demonstration and convincing, you know, it was a surprising discovery. Nobody in the head area was-- thought this could be, and they were working on AMR sensors and there was no perceived need to improve that sensor. There was already a plan. What do we do with the AMR sensor? That's what they were working on. And there were plenty of issues the AMR sensor had that they were focusing on. So this kind of fell from the sky on the head area. But it didn't take long, you know, to interest people like Dave Heim and Bob Fontana as I said and Ching Tsang and some other people in the head department too, to decide to collaborate with us. So we continued to be in the media area nominally, but really all of the focus of my group was on the spin valve sensor and it was okay.

Bajorek: Now you had to, you probably also needed to, I remember we were always dependent on spin stand work. The proof of the pudding required flying a head, right, over a real disk.

Speriosu: Yes.

Gurney: For a long time.

Bajorek: With a real data sensing channel, right?

Speriosu: Yes.

Bajorek: How did you get that done? How did you rope--

Speriosu: Well, that took--

Bajorek: The spin stand people to do this?

Speriosu: That was in steps. So, the first step was to demonstrate it in on patterned-- I mean patterned but unshielded sensors. And so, this is now looking on a wafer and measuring the characteristics of a patterned sensor and showing some very interesting and unexpected by the head people behavior. One was the linearity to the applied field. Because the cosine theta dependence once you pin the pinned layer, then you have the-- So the pinned layer is at 90 degrees and this is at zero degrees, the free layer is at zero degrees. As it moves now the field is also in this direction, it becomes linear. It's just the mathematics fortuitously happens that way, so that, that all of a sudden made a very wide dynamic range. Too much signal and too much dynamic range compared to what people were used to doing. So they said, "Well, what do we do now? Do we have to thicken the media to use it or what?" So, one question that we grappled with, it was not a big deal, was we have to reduce the thickness of the free layer so that the dynamic range of the sensor would correspond to the amount of flux coming from the media at the time. It was not a big problem, but these conceptual questions had to be looked at, and they were looked at initially at patterned unshielded sensors and very quickly on shielded patterned sensors where, where Dave Heim and Mason Williams did calculations and then Ching Tsang did experiments. And all this was demonstrated before a flying head. It was by steps.

Gurney: As I recall, Ching found that the signal was so large that it saturated his electronics.

Speriosu: Yes, that was wonderful.

Bajorek: He blew the fuse-- a fuse on the preamp.

Gurney: I think what he did is he just flew further away.

Speriosu: Yeah.

Gurney: He just lifted the head further away so that he could get into a regime where the electronics could handle the signal even though that degraded the magnetic resolution a bit. And that was the first indication to I think to the people in the head area that there really was something here that was going to capture their imagination. Because it wasn't just a replacement for AMR, it was better than AMR.

Speriosu: Yeah.

Gurney: And so in our minds, it became a question of what do we need to show them? How much-- How much more better, if you like? Do they-- Do we need to demonstrate in order for them to want to include this in their road map?

Dieny: As a matter of fact, with anisotropic AMR the signal is dropping with the thickness of the layer; as you make the sense layer thinner and thinner, the AMR amplitude is dropping. Whereas with GMR, you can make the sense layer quite thin and still get a very high signal also. This was --

Speriosu: That was a very convincing graph.

Dieny: Yeah. We thought so, yeah.

Speriosu: And you know with this graph, AMR goes to zero and the GMR, it has a peak.

Bajorek: A peak.

Dieny: Mm-hmm.

Speriosu: So at the thickness that you want--

Gurney: You have it there.

Speriosu: Is a factor of five. So, yeah.

Bajorek: Very nice, very nice. And Virgil, I thought it might be good to go back and clarify. Was your group the only group working on the subject in the period we're talking about? Or was Stuart also?

Speriosu: Okay, so during this period of time, we had two, two groups at IBM. There was Stuart (Parkin) and his group with its mandate, internal mandate understanding that it would focus on towards the scientific aspects of GMR and oscillatory coupling and so on. And my group, where the emphasis was more towards a sensor, demonstrating the sensor, working with other members of IBM’s head area and manufacturing and demonstrating it and testing and verifying. So there were two groups. However, there was a lot of mutual overlap, both in terms of applications and the scientific area. We were quite interested in the scientific aspects of that. Maybe this would be a good point, a good moment to, you know, to invite Bruce to talk a little bit about that.

Gurney: So, I had previously mentioned that one of my keen interests was understanding the mechanism of GMR certainly from the point of view of wanting to increase the GMR amplitude, That was very important. And also part of our interest in demonstrating that GMR was going to be so much higher signal to noise than AMR. Understanding how to control the GMR effect as it was manifested in spin valves was an important aspect technologically. So among the things that we did was to, I think perhaps for the first time, use a spin valve structure that is a structure in which one of the layers was pinned with an antiferromagnet to, by varying thicknesses and comparing with models demonstrate that we could measure a fundamental property of some of the materials involved and in particular were able to demonstrate that we could measure the spin dependent, mean-free path of carriers in some materials like nickel-iron, cobalt, etc. And, as initially described by Nevill Mott in the 1920s, his sense was that it must be the case that in ferromagnetic materials carriers having one spin will have a significantly different mean-free path than carriers of the opposite spin and that spin up electrons would travel much further than spin down electrons, let's say. It's a consequence of quantum mechanics. So, in the thirties and forties, he looked for this effect and really was not able to see it with the materials that he and devices that he could make at that time. What we were able to do is to show that his idea was completely correct and that the spin up and spin down mean-free paths in these ferromagnetic materials was very different and measure what they were. From a technology point of view, that was important because it allowed us to understand what kind of materials we really wanted to have, and what it did was to spawn a whole series of inventions in which we modified the interfaces, the thicknesses and the materials chosen and also the interfaces, for example, of what the top layer and the back layer of the material are, all to improve and increase the GMR amplitude and produce devices as a result, which again have a large amplitude which we wanted to show overwhelmingly would allow us to make hard disk drives to very, very high areal capacities. So, the consequence of that has been that a number of people through the years have used spin valves as an investigative tool, as a device that allows them to control the magnetizations of layers and then measure a variety of different kinds of phenomena. And ultimately, this led to the topic of and the scientific investigation of what is a combination of electronics and spin called spintronics. And I would argue that the spin valve was the first spintronic device, and it may be so far the only device that's been introduced in the marketplace and made money. A variety of other spintronic devices have been proposed and investigated, including, perhaps Mustafa will be able to talk about magnetic random access memory, MRAM. And so the interest in the scientific underpinnings of the GMR effect was an integral part of improving the technology of GMR. And it's an interesting aspect of the whole story here that over time, over the time period that we're talking about discovering and inventing this device, our understanding was evolving. What's the right physical description? What do we want to measure? What's going to be relevant? How do we measure it? It's what made this project both extremely exciting and it also made it possible for people with all kinds of expertise, including magnetics, electrical signal processing, chemistry and all the people who do deposition and processing to make a contribution. And at a place like IBM, all these people were assembled together. This was a particularly exciting time for a lot of us because we were also given the freedom to pursue these kinds of scientific and technologically relevant investigations, all recognizing that ultimately what we wanted to have was a product that IBM would be able to use and use to extend its hard disk drive capabilities for an extended time.

Speriosu: If I could interject. Bruce, you mentioned the word invention. Maybe this is a good point, a good moment to talk a little bit about the invention of the spin valve. So I thought I did. And then Bernard also independently did. But when we tried to file a patent in the spring of 1990, we discovered that there was already an application, now a patent by Peter Grünberg, one of the co-winners of the Nobel Prize for the discovery of GMR. So in 1988, I guess at the time when he was making the announcement of the scientific work, he had already foreseen a lot of what was to happen and without doing any experiments, he proposed some basic structures and one of them was the spin valve. He didn't use that name. So we were disappointed that we were not quite as brilliant as we thought-- I thought we were, but still, we were the first ones to demonstrate it. And moreover, we were the only ones to work on this for five years in the industry. Maybe you wanted to say something about that.

Dieny: Yes. Indeed, in Grünberg's patent that he filed in 1980--eight

Speriosu: Eight.

Dieny: Eight, yeah, he clearly explained that we can make a sensor with GMR by controlling the relative orientation of the magnetization in two magnetic layers. And he proposes three ways to achieve this control of the relative orientation of magnetization. One is by using antiferromagnetic coupling, and this was done in the iron-chrome multilayers. The other one was with double coercivity systems, the so-called pseudo spin valve. And the other one was by using an exchange bias pinned layer associated with a free layer and this was the spin valve concept. But all this was said in about 10 lines in the patent and at that time, it was not clear which was actually the best approach among these three to make sensors. And then, in 1990 we experimentally built and observed the response of the spin valve structure. But it's interesting to see that in the following five years, between 1990 and 1995, if we look around the world, the three approaches were investigated by different groups. IBM was the only one working on spin valves till 1995. And I think the first paper I remember from outside IBM on spin valves was from Philips Eindhoven, a group led by Reinder Coehoorn. But several groups in Japan were working on the Giant Magnetoresistance for sensors and a professor from Kyoto, Professor Shinjo, discovered and studied GMR in permalloy, copper, cobalt-copper multilayers, which are double coercivity multilayers. So most of the Japanese group were working on these double coercivity multilayers. And other groups, also including Albert Fert's group in Paris, but also at the beginning, at Philips in the Netherlands, in Eindhoven, were working on the antiferromagnetically coupled multilayers and studying both the basic aspects of the oscillatory coupling, the frequency of the oscillations, the phase of the oscillations, and they were looking at the same time at the Giant Magnetoresistance. So, between 1990 and 1995, worldwide efforts were put on these three different kinds of systems. But only around 1995 everybody converged towards spin valves because the benefit of spin valves became more and more evident at that time. So all groups stopped working on double coercivity and oscillatory couplings and gradually shifted to spin valves, yeah. So, for the first five years IBM was the only team working on spin valves in the field of GMR.

Bajorek: It's interesting how visionary Grünberg had been, right, in terms of his foresight of what could happen here and then how much he stimulated it, right? A lot of work through those ideas. I remember independently of all of this, by the time I caught up with some of Stuart Parkin's work, we had commercialized what, I remember where SAF disks, these--

Gurney: AP -- Anti-ferromagnetically coupled disks, yeah.

Bajorek: Disks.

Gurney: Yeah. I'm an inventor on that patent.

Bajorek: So, you're inventor on that patent. And I was--

Gurney: Recognizing, if I could just jump in for a second.

Bajorek: Yeah, yeah.

Gurney: It was recognized that magnetic media would benefit from having a lower moment because of the impending issues with having magnetization in plane. And this was presented at IBM Almaden to a group of us who thought about it. And the next day, three of us, Eric Fullerton, Matt Carey and I, proposed exactly the same structure, which the group of Hal Rosen turned into a product in which the same kind of idea of using a magnetic layer, an antiferromagnet and another magnetic layer oriented oppositely to produce a lower net overall magnetic moment, was used in magnetic media for a couple of generations of magnetic media as I recall in the nineties.

Bajorek: Yeah, I was-- I was no longer at IBM then. I was at Komag, and when I heard of that development, I caught up with Stuart's publications where he had done all the detailed work on oscillatory coupling, right? And, so it reminded me that just coming back to it, I think you made earlier remarks that we had at least a couple of competing groups in IBM, right, working on these related subjects.

Speriosu: Indeed--

Bajorek: Your group and--

Speriosu: Parkin's.

Bajorek: Staurt Parkin's group.

Speriosu: Parkin's. But there was a natural-- It was very natural for the rest of the community at IBM, for development engineers, to want to test it on their own equipment. So actually, Daniele Mauri, Tsann Lin and Mustafa, they were all interested repeating and improving it with their own inventions. This was a natural characteristic of the environment we were in and in the freedom that all these people had to be able to just spontaneously adopt -- I cannot talk about transfer of technology -- some of it was a kind of diffusion, inspiration and hopefully a lot of collaboration, not a handing over of a recipe, it did not happen.

Bajorek: So Mustafa, how did you sneak under the tent here, into the GMR tent?

Pinarbasi: So I'll divide it into two areas of work. The one that I was already working on, which actually turned out to be the basis for the work that I was doing on spin valves; and then the individual or the work that was specifically geared for the spin valve as well. I'll just cover the first one first. When I joined IBM, actually into Bob Schwenker's group, who was a great manager to me. He was my manager throughout the whole development of the spin valve as well. There were two projects I was assigned to. One was the ion beam etching. This was etching for making the AMR sensor. This was part of the process flow for the product development. And the other one was the ion beam sputter deposition. This was purely an exploratory project. It had no relationship to what was going in development. He thought I was well-suited to do that work. The work that I have done at ion beam sputtering, basically which was later on introduced into manufacturing for both AMR deposition, and hard bias and lead deposition as well. I developed specific processes and developed techniques utilizing the heavy mass ions which could reduce the stress in the films significantly. It was a big deal for these layers and also enhanced the performance of the AMR films. This is a nickel-iron film and nickel-iron chromium, nickel-iron rhodium as a soft adjacent layer. And tantalum and the ruthenium, rhodium for the conductor leads and cobalt-platinum- chromium as well. I worked with these materials to develop a lot of good technologies and processes using ion beam sputtering. That was independently going on. I first became aware of the spin valves in the year 1990. I joined IBM in the summer of 1989, three months before Bernard's arrival. In 1990 I became aware of the spin valves and it was very exciting to me at that time. In the beginning of 1991, I already made the spin valve structures utilizing the iron-manganese again with the four-target system. This was the same material Bernard had used with his four-target system. I was working with Serhat Metin who was part of the original work. He was involved with both Stuart Parkin's group as well as Virgil's group. So, he was able to measure the samples for the GMR values and then he would tell me the data. So that's how it was done. Because in the development area where I was, there was no means of resetting these or making the measurements. So, I made the samples and he was very excited because the very first samples that I made showed very good performance. And the--

Bajorek: Just I want to interrupt you for a second. You all talk like you were at IBM, except three of you were in IBM Research. You were in the Product Division. What at that time, I think was called the General Products Division, right?

Pinarbasi: Correct.

Bajorek: So you were-- You had the double challenge of collaborating between your responsibilities as a product developer, right, and the Research team. And you're both in different divisions, which added extra barriers, right, to collaboration. But it's great that you were able to do that. I just want to clarify that you were not all in one division, right?

Pinarbasi: Correct. And I think in addition, the one that I think Virgil mentioned, the tolerance and then opportunity to go and do the type of things that one was interested in. IBM at that time was a great environment to do this type of stuff. I was lucky that I was actually given great freedom. In fact, I don't even remember when I was actually given a specific task to go and do it. It was always either I understood or I went and did it. And that was just an incredible environment in terms of really going and doing something that was interesting and exciting. But that changed a little bit. In fact, it became a bit more formal in the year 1991 after I did the initial spin valves. I think it was recognized by my manager, at that time Bob Schwenker, and also Mohamad Krounbi, who was also a manager in the Aries Development line. They thought that I could actually do work up in the Almaden Research Center as well. So, I was offered a dual position. Then I had the ion beam sputter deposition tool up in the Almaden Research Center and I started working there as well in 1991. Now I had the work that I was doing for the AMR and all the staff in the main site in the Product Division, and also, I was doing the exploratory work such a spin valves in the Almaden Research Center. I had a dual position that lasted for five-six years, nearly to the time that the spin valve was introduced as a product. That was the freedom that I had. I could work on both of them. So, I started working on the spin valve structures up in Almaden and at that time we already had the basic structure as already described. It was already being practiced. I could attend the meetings where Virgil and Stuart Parkin would be, we would take part as well, so I could see all the work that was being done. So, I could reproduce some of these results with the structures I was making. That went on for quite a while. And I think it was 1993 that I had made enough progress on a lot of the stuff that I had done that I sat down and talked with Virgil, and he was impressed and convinced him. He wrote, actually, we wrote a two-page report. Sent it to you, Chris, asking for a tool that had more than the four targets that was available to us at that time. And later on, actually, a couple of years later, we would get the funding to proceed with that work.

Bajorek: And that was a tool based on ion beam sputtering.

Pinarbasi: Ion beam sputtering, that's correct.

Bajorek: Yeah, yeah.

Pinarbasi: And so that was 1993. I was fully engaged at that time, but I think, that it's been mentioned a couple of times. But now, in the 1994-1995 timeframe, we were really looking at the specific areas that we had to improve upon. And I think the nickel-iron free layers, and the pinned layers and the nanostructures utilizing cobalt or cobalt-iron at the interface, these were well-practiced. And we were all using mostly iron-manganese as an AFM layer but we all knew the drawbacks of that one. It was just, we knew temporary work because of the corrosion issues and also because it had very low blocking temperature. Really, any type of head-to-disk interference, if there's any contact, it would lose the pinning. So that was just a temporary one. The other one at that time was nickel oxide, that was done, but it was talked about some results, but the exchange of that one was extremely poor and the needed blocking temperature distribution was not there. It was deposited with the conventional sputtering. So, what I thought was that iron-manganese was out and there was no other material really you could practice to replacing that one. I thought I could improve the nickel oxide, so I started working on the nickel oxide. If this is not being used, even getting the targets, making, finding a target manufacturer who is going to make a nickel oxide target, it takes a long time. So, it took some time. Finally, I got a nickel oxide target, developed processes and I was able to significantly enhance the performance of the nickel oxide exchange. But it still was not sufficient. So, I had to develop a new process from the nickel targets. I knew that utilizing reactive processing I could have a better control of the nickel-oxygen ratio and incorporation of oxygen, so I could probably improve the performance of the nickel oxide in that fashion. And this is a process that I developed. And indeed, I had a factor of five improvement compared to the initial work in terms of the exchange, and it was a significant improvement. Then I was excited, and we did some of these simple spin valves where we basically put the AFM layer with the pinned layer and then the copper layer. And what we have seen at room temperature was a great signal. it worked well except when it went through the processing it would lose the exchange. If you put it in the high temperature, like 200 degrees C, 150 degrees C, you could reset it. But this, of course, could not work in the drive, so that was not enough. So, in late '95 and '96 I started working on improving this aspect of the thermal stability, which Bruce mentioned, thermal stability, magnetic stability, and get improvement of the AFM material. At that time I started looking into what we called the APP spin valve or the synthetic antiferromagnet. So, I started working with that one. What I did is I combined all of these, reactive nickel oxide with the synthetic antiferromagnet which was used and patented by Stuart Parkin and David Heim. So, a combination of the two gave enough thermal stability. That looked very exciting. At this time in 1996, I remember it being of real interest to the Products Division as well. My manager Bob Schwenker and Mohamad Krounbi and Harry Gil and myself, we started having daily meetings, no, weekly meetings to really look at what we were doing, if we were keeping the progress and all that stuff was actually going on. At that time Bob Schwenker and in the early days Mohamad Krounbi, were in the Aries Development Lab. So, I could also talk with Mohamad Krounbi who worked on these things for quite a long time on the AMR sensor. He had a lot of insight as well. There were a lot discussions with him as well. So, finally, we had a structure that had shown good promise. And I believe it was early to mid-1997, we were basically ready to go into the last phase of it. At this time, I had really two responsibilities. I had to improve the AMR sensor and at the same time I had to introduce the work on the spin valve as well. So, the AMR sensor, the very last one that I did, which was MRX, was going into manufacturing. I was able to devote most of my time now on the spin valve structure. And about mid-1997, I went back and looked at the date. It was June 4th, 1997, we entered the last phase of the introduction of the spin valve, and, you know, you'll see that is how it really developed during the next four months. And at that time everybody was there, including Bob Scranton, and it was a huge meeting. He announced that we were going to demonstrate 10-- at that time it was 10 gigabits per square-inch. And he turned to me and he said, "Mustafa, you're going to lead the spin valve development project for this." And he said, "You get to name it as well." And then we immediately started the work and I remember the two batches that we started. It was EWR, Engineering Work Request, 790 and 791. I was not taking any chances. One batch was dedicated to a simple spin valve still, and the other one was the APP spin valve. And they both had the nano layer, cobalt layer and different thicknesses. The cobalt nanolayer impact was well-known at that time, but what was a problem was the softness of the free layer was a key issue. It had to be soft enough to rotate when it saw the field from the disk, so we could not use a very thick cobalt layer. It was 2 angstroms and 4 angstroms. Those were the two thicknesses. And we finished. We had only one chance. Later on, Bob announced that there was a COMDEX show that year in November, he wanted to have all this completed. The batches were finished in September and all the tests were done in October. Everything looked great. But at that time we knew that the APP spin valve was the one that was stable and the hard disk HDD tests were performed and we were the first ones to announce the GMR product in November, 1997 with the heads that were made from the batch 791.

Bajorek: And that was the product from a disk drive from Fujisawa. IBM Fujisawa?

Pinarbasi: It was called Deskstar 16 GP.

Bajorek: Yeah.

Pinarbasi: And I remember the Titan because initially I was told that it was going to be a technology demo, so they were going to make 50 drives and IBM was going to put it into Think Pads and send over to some important customers. Later on, I found out that the GMR went to the server as well and when it went into the server it enabled $1 billion dollars of revenue for IBM in that year. So, this is what I was told. It was really within a year and a half or two year timeframe, it went from just being a project to basically into the product. And at that time, one of the key things with the APP structure was that when people heard about it, 8 angstroms of ruthenium and it was immediately said, "This is-- this cannot go into manufacturing. It is totally unmanufacturable," because then the thickness of the free layer, the thinnest layer that we were dealing with was 120 angstroms. So, if you compare what was in manufacturing versus what this one was, they said it could not be done. But then these are the things that we were able to develop with the ion beam sputtering and we could control it at that level. In fact, you know, 8 angstroms was the target, and 7 and 9 angstroms were the limits. And we were able to repeat that one. Then going back a little bit more forward, all of the things that really made ion beam sputtering be special at that time, that really enabled us to make that introduction of the product actually was incorporated into conventional sputtering tools. Even today, what we are using, they all have very low-pressure deposition, an order of magnitude lower than what it was at that time. Also, they have very isolated targets versus the substrate. So, all of those good things that we thought were special for ion beam have been incorporated into the conventional sputtering that we use today for the magnetic tunnel junctions.

Bajorek: Amazing. Amazing. Well, I'm pleased to see, it's almost like a textbook case for a Harvard study, right, Harvard Business School study, how the technology moved, right. Because it wasn't just transferred, right?

Pinarbasi: No.

Bajorek: It sort of--

Speriosu: No, it's spontaneous-- No, it was a spontaneous adoption.

Bajorek: Spontaneous. And to avoid using a pun term, tunneled over, right from Research to the product and--

Pinarbasi: So, there is one more thing that I want to mention. That and this was at this time, nobody knew this specific part of the story. That finally, you approved the tool, so we purchased the tool. And now we had not only one chamber but actually two-- six target chambers. What we thought at that time was we could separate the nickel oxide deposition from the metal deposition, and then we could increase the throughput. And also, at that time what I was thinking, what we were thinking is that if you separated the oxygen process from the rest of it, it's an impurity right? Then we could enhance the performance of the spin valve structures. So, I was excited, and we got the new tool and set it up. When we ran the structures, the properties were not as good. In fact, they were very poor. We continued to do experiments. We could not repeat the data that we had from the single target chamber. And I was thinking, I remember that earlier, about a year and a half before, being the type of leader you were, you came to my office. We discussed for one hour. You asked all the different things and at the end you asked what I wanted. I asked for one thing at that time. It was a tool to do the work, which I got. I said, "Now I cannot make it work. So, what am I going to say?" I could not find out what the reason was. Then I said, okay, I put all the targets into the one chamber, as it was originally, I could repeat all the stuff that I had done. Now this was great. Now I knew that and it was working still. We had not done anything wrong. But I had to go and figure out what was done. And at that time, I had my manager know about this one. And he was not concerned, but I was very concerned. He knew I was going to find it. After a lot of work, I determined that, actually having oxygen had a very beneficial effect in the chamber. After a lot of work, I determined that if I actually have oxygen bleed into the interfaces before and after the copper deposition, that was the key. I developed the process, 30 seconds of oxygen exposure after finishing the cobalt deposition, before the copper deposition, and after the copper deposition another 30 seconds, significantly improved the performance. I would get 30, 40 percent higher GMR. I could get near zero or zero coupling field if I wanted. I could reduce the copper thickness below 20 to 25 angstroms; at that time 25 angstroms was the number for the requested coupling field. I could go to 19 angstroms and this was huge because that's signal, there is no shunt.

Bajorek: Right.

Pinarbasi: I was finally able to repeat this one. And at that time, with the information, I went to Bob Scranton, the person who was in charge, and presented the data to him, and he said, "Okay, this information does not go anywhere. It stays here. You do not discuss it with anybody." The issue was that although it was a great thing, it was not discoverable. If you go out and let somebody know, you cannot say that unless you get it and really go out and look at what they were doing. So, there was no publication on it. There was no patent application for it. And that continued until the year 2000. And the only time I had to let somebody know, it was Serhat Metin, who was a dear friend of mine--

Virgil Speriosu: Mine too.

Pinarbasi: Who actually initially worked with--

Speriosu: He worked very well with all of us.

Pinarbasi: With Virgil and Stuart, was the manufacturing engineer. So, I sat down and explained to him. So, we had to really utilize it by putting it into tool and then not let anybody know. He embedded it into a recipe so nobody could really figure out. And this is how it worked in manufacturing from 1997 to 2000. Finally, what happened was that there was an Intermag conference in Toronto. And I don't remember who it was, but they were giving a talk that if they do oxide and utilize oxygen to form the oxide cap, they were seeing improvements. And then I came back and Bob Scranton was there. I said, "Bob, this can't happen. I mean, people are going to figure it out. We should at least get some credit for it." And it is then when I actually put a patent application in. So, this was a secret that was in place for over five years.

Bajorek: Amazing. Amazing.

Dieny: Yeah. I think the presentation was made by Egelhoff.

Gurney: Mm-hmm. Yes, I think it was.

Pinarbasi: Yeah, he did-- He did--

Gurney: The same Egelhoff I mentioned. Interesting, He worked on--

Pinarbasi: Yeah. He worked on surfactants. Yeah, on a surfactant -- Yes.

Bajorek: Mm-hmm. Now so I gather then that you ended up with this head that had an AFM pinning layer, right? That pinned an SAF pair, right? A copper spacer and a free layer. That was a basic structure.

Pinarbasi: That was, right.

Bajorek: And that, the combination of AFM and SAF achieved--

Pinarbasi: Established the stability we needed, both thermal and magnetic stability. And furthermore, as Virgil mentioned the blocking temperature. This is-- this was a very important part of it, because the nickel oxide doesn't have high blocking temperature either. The concern that we had was the blocking temperature distribution. That basically part of the structure would lose pinning, and then, it would no longer act as an effective layer. That was another key thing with the APP spin valve, that the effect is somewhat removed from the free layer so that we could actually have a better signal with this AFM. And the other key part was that when we put this AFM layer and the APP pinning structure, we have a lot of metal layers. We have-- we lose a considerable amount of signal and--

Bajorek: Through shunting, right?

Pinarbasi: Shunting.

Bajorek: Shunting, yeah.

Pinarbasi: Shunting was huge. That's why everybody worked with the simple spin valves. And the advantage of the nickel oxide was it was an insulating layer so that we could actually get some benefit out of that one. And, I know that at that time everybody was working very hard to introduce the product. Everybody lacked the AFM layer. About a year later, a year and a half later, people came out with the first platinum manganese, and they introduced the simple spin valve structures because it had much better pinning. Later on, it became iridium manganese as well. But eventually, the rest of the industry came to what we had originally introduced, that was the APP spin valve structure. Even today, you know, we are using APP in structures for the magnetic tunnel junctions for the MRAM applications.

Bajorek: I also remember hearing that at that time, there was some hiccup in the introduction of the Fujisawa product and there was some last minute work to solve it. And I heard that maybe people had developed a way to reinitialize a head, if it--

Speriosu: Can I interrupt? Because you're asking this question. So, so we were still working with the iron manganese heads, and Moris Dovek was studying them in collaboration with us and he noticed this very, very pervasive problem. We actually knew that this, the resetting, the misorientation of the anti-ferromagnet was a feature of the very first structures we made. It was a problem. So, it was recognized very early on. By 1993, we all knew that this had to be solved. And if the antiferromagnet is misoriented due to a thermal event, either an HD interaction, a head-disk interaction or like a static discharge or anything that will raise the temperature, the pinned layer is in a high energy state and it wants to demagnetize. It wants to go in the plane at least partially. So, the antiferromagnet needs to be there to hold it in place. You're grabbing it. But if you bring the temperature high enough, then it softens the antiferromagnet and the pinned layer now becomes disoriented and the signal becomes unrecognizable. It looks nothing like what you want. It's much lower. It has the wrong shape and so on. So very early on we recognized in 1993 the need to ensure that the pinned layer stays properly oriented in the head. And so, a whole procedure was established by an annealing step, by heating the actual structure. I'm not sure if it was at the slider level, but at the wafer level. So, by the time heads were made, we had developed, Ching Tsang and so on, had developed a protocol for setting/aligning, the pinned layer orientation. And later in the same time frame, Moris Dovek was looking at these things and he noticed that pulses in the read current had a tremendous effect. The amplitude of the read current made quite an effect on the signal that he was observing. So, discussing the issue with me, I explained to him what was going on with the pinned layer, and he then proposed the resetting in situ at the head level. Once the head is already inside of the drive, if you like, you can send a heating pulse, which brings the temperature of everything past the blocking temperature of the antiferromagnet and then allow it to cool in the presence of a smaller but longer holding current and field. So that way he was able to demonstrate that you can overcome this instability. Each time there's an event, you measure the signal. If it's wrong, give it this and refreeze it. So that was patented, you know, and Bruce was a part of that as well. So that I understand is still being used because now--

Bajorek: Now I understand it shipped, right, in that first product.

Pinarbasi: In the first product this was actually implemented. It was in the first product. They wanted to make sure that if something were to go wrong, they had a way of fixing it. And this is the one that I remember that for a number of years, I explored this, that all the failed HDDs that came back, and they took the heads out and they analyzed them because they could tell what was wrong with it. And in no instance actually, this reset mechanism was activated. So, what we had done as a spin valve at that time had enough thermal stability and magnetic stability to survive the operations.

Bajorek: The belt was sufficient. It didn't need the --

Pinarbasi: It didn’t need --

Bajorek: The suspenders.

Pinarbasi: Suspenders, that's all right.

Bajorek: You had the suspenders but you didn't need to invoke them.

Pinarbasi: That's right.

Bajorek: That's great.

Pinarbasi: But without that one, I think, just to emphasize, without that one I think it would have been probably difficult to make the product announcement, because you always want to have something in the background, that if something were to go wrong, you could fix it. And having that one, of course, made it very easy.

Bajorek: Right.

Pinarbasi: If something were to go wrong, we can fix it.

Bajorek: Well, terrific. I think we covered, I think we are now at the point we wanted to get to, which is a shipment of the world's first GMR head, right?

Pinarbasi: That's correct.

Bajorek: In end of '97.

Pinarbasi: 1997.

Bajorek: Right?

Pinarbasi: COMDEX show, 19-- actually it was announced before the COMDEX show and then I think, and they made a large public introduction was in the COMDEX show.

Bajorek: And the rest is history in terms of the drive industry, right, because the whole industry then adopted, shortly thereafter, GMR and--

Pinarbasi: Correct.

Bajorek: Key innovations from the GMR structure were then adopted by the TMR, right.

Pinarbasi: Not only the key innovations in terms of structure, but also the eco system, making and being able to make these structures and developing these tools, it was not existent when we were doing all this stuff. That all had to be developed at that time.

Bajorek: Right.

Pinarbasi: This is the one that I was mentioning that, you know.

Bajorek: Right.

Pinarbasi: A lot of the stuff that we actually thought was very useful with ion beam sputtering, that was implemented in the next generation GMR tools. And by the way, all of those improvements are in the MRAM tools that we are using today for the foundries in the semiconductor industry, so--

Gurney: That's right. We were very fortunate that semiconductor tool manufacturers, tool manufacturers for the HDD industry all migrated towards this level of essentially of exquisite control over the deposition conditions and over the deposition rates of materials. And so, in 1997-1998 after the introduction of the first spin valves at IBM, what we did was to recognize-- was to project out into the future how far would GMR take us and what do we need to work on that would be the follow on technology? And we developed what we called the magnetic tunnel junction or MTJ Factory, which was all inside of Research, working closely with Stuart Parkin, actually, who was a part of this Factory. We would deposit materials and process them because this is a tunneling quantum mechanical phenomenon between layers that are very-- magnetic layers that are very close together whose tunneling current changes when the magnetizations change. This sounds like a familiar story, right? Magnetization changing. In order to make measurements on those kinds of materials, you can't just look at the entire film, you have to actually fabricate very small pillars in order to have a resistance that you can measure easily. We would as part of our Factory, then use all of these same materials and processing tools that had been developed for the spin valve and build layers to do this quantum mechanical tunneling and then process them to make very small pillars, measure the pillars, all learning how to make good tunnel ferromagnets and tunnel barriers for the MTJ world and a number of other companies also recognized the GMR would have it its heyday and have a sunset. And they also began at universities and some other companies began working on tunnel junctions at the same time. So IBM's contribution was to identify certain materials that would work particularly well and structures that would work particularly well and developed along with much of the rest of the HDD world and the academic world tunnel junctions that replaced GMR when GMR no longer was able to provide the signal to noise necessary, because current in the plane winds up being limited by the contact resistance of contacts that you could make to the small structures as they may get smaller and smaller, that contact resistance goes up and your overall signal to noise degrades. So going perpendicular to the plane was an important recognition--

Bajorek: To extend this technology.

Gurney: Magnetic tunnel junction to extend the fantastic growth in capacity of our disk drives, to continue to make areal density improvements, pack more and more information per square inch is necessary to drive current-- to build sensors in which you drive current perpendicular to the layers. Tunnel junctions have a much larger magnetoresistance effect than conventional GMR layers, and so tunneling became the standard follow on technology to the GMR technology that we're talking about and that has evolved into stand-alone pillars of tunnel junction materials where information can be stored, total-- referred to as magnetic tunnel junction, magnetic random access memory as a potential of, if you like, alternative to flash memory. It has a number of characteristics that make it attractive, one of which is speed. I think Mustafa can talk to many other aspects of how MRAM can be an interesting and viable storage technology. But I just want to emphasize that this all started back with Grünberg and Fert coming up with this Giant Magnetoresistance structure and then the whole team at IBM dedicating itself to understanding this phenomenon and learning how to manipulate it in ways that would-- and control it in ways that will be, you know, beneficial to--

Pinarbasi: Meaningful follow-on technologies.

Gurney: And it turns out many follow-on technologies and so some of the concepts that were introduced in those early heady days continue to be very relevant to devices and information storage today. And in the future.

Bajorek: Well, congratulations to you, to all four of you, as well as those who worked with you because--

Speriosu: Yes.

Bajorek: This is I'm sure-- you mentioned many names that contributed to this.

Pinarbasi: If I may add for what Bruce mentioned for the MRAM. Really, all of that stuff that we have done that I have worked on, we all worked on through those years and later on in the magnetic tunnel junction, we are utilizing pretty much what we have done at that time. Today, I am CTO of an MRAM company, the Spin Memory, and basically, we ride on all of this stuff that we have done in those years to extend and really utilizing experience to further the-- our understanding of these subjects.

Gurney: And you can correct me, but also very much the same tools.

Pinarbasi: And very much the same tools. And it has not changed. But they became better. We added more features to them. We would do deposition at room temperature. Now, maybe we actually have higher temperature and lower temperature depositions but fundamentally it is the same concept. In fact, what was derived from those days as the low pressure deposition and isolation of the plasma from substrate and target, these were all implemented in the tools following-on to the work that we have done with ion beam sputtering and that is still being used today. Those are the tools that really make the PMTJ, perpendicular tunnel junction, today that is being used in the semiconductor industry by all the foundries to make MRAM structures.

Bajorek: Well, your comments then bring me to the point where I think it would be fun to transition to what have you all done since those early days?

Speriosu: Well, I guess I can start it now.

Bajorek: In GMR. Yeah, so--

Speriosu: I only-- I started, I already started this session by saying that I retired three years later and I actually, I spent several years getting medical treatment. And you know, you don't see that. I look, you know, I am in excellent shape.

Bajorek: You look terrific.

Speriosu: Thank you. But I spent years not looking terrific and not feeling terrific and wondering if I'm going to live. So, I had a very hard time for a number of years, health-wise. And then when I came out of that, my health improved in various ways. I reincarnated as a photographer. I'm now a photographer. I'm an art photographer, and I've been doing that passionately all my life. I had this interest in visual arts kind of in parallel with my scientific interests. So--

Gurney: If I can drop in and say that was very much to our benefit during the GMR days.

Speriosu: <laughs>

Gurney: We spent a lot of time thinking about, and Virgil in particular, spent a lot of time thinking about how to take what we had-- were learning and be able to present it to other people. And so a number of very illustrative and deeply insightful figures. presentations were generated that allowed us to--

Bajorek: Communicate.

Gurney: Sell this story to the rest of our company and also to sell this story to the rest of the world at conferences. And that's very much because of the emphasis that you put on being able to give good presentations and create really insightful figures.

Speriosu: Thank you so much.

Bajorek: Well, I'm glad we were able to distract you from photography long enough to join us here for this interview.

Speriosu: Well, I thank you for bringing me back, Chris, thank you.

Bajorek: It's great. How about you, Bernard?

Dieny: As I said, I left IBM in March 1991 coming back to France and to Grenoble. But I kept on working on spin valves in Grenoble because I brought some samples back from IBM, so a set of samples on which I could do further characterization in Grenoble, that I could not do in Almaden, in particular, low temperature measurements. So, I measured the magnetoresistance and the evolution of the exchange bias field down to low temperature. I conducted different studies which helped me to understand the physics and model also the Giant Magnetoresistance and the resistance of spin valves. Two years earlier, two theorists, Camley and Barnas had published a paper on the theory of Giant Magnetoresistance in iron chromium multilayers based on the Boltzmann equation. And I used this model and I extended it to any spin valve structure. This allowed us to calculate the resistance and magnetoresistance from the microscopic transport parameters, which are the spin-dependent mean-free paths and spin-dependent coefficients of reflection and transmission across the interfaces. Of course, there are many such microscopic parameters to be introduced as input parameters because for each layer there are two mean-free paths: spin up and spin down mean-free paths, and at each interface four coefficients: two of reflection and two of transmission. But gradually, by fitting sets of data from experiments like Bruce has been doing which allow to determine this spin up and spin down mean-free paths, we could collect a sort of database of these microscopic parameters of transport. And I developed software which allowed from the knowledge of these microscopic parameters to calculate the resistance and magnetoresistance of any spin valve structure. This software has been used, for instance, by Headway Technologies in Kochan Ju’s group to optimize their spin valve structures and it really helped them to accelerate the optimization of these structures. So, I have worked on this for a few years and we did various studies on understanding the relative role of interfacial and bulk scattering. The software that I developed really helped to quantify the relative role of bulk versus interfacial scattering. Then, in 1996, I came back to California in San Diego and I worked for one year at the Center for Magnetic Recording Research. I worked with Ami Berkowitz there and it was the early days of tunnel magnetoresistance because the first observation of tunnel magnetoresistance at room temperature took place in 1995, just one year before. So, like when I arrived at IBM one year after the discovery of GMR, I arrived at CMRR one year after the discovery of room temperature tunneling magnetoresistance. So, with Ami Berkowitz we jumped into this field and we tried to develop magnetic tunnel junctions using different kinds of oxide barriers. And I remember, we worked of course with alumina barriers. We also tried magnesium oxide barriers but at that time the quality of the oxide was not good, so we obtained magnetoresistance in the range of only 20 percent. Later, Yuasa (Shinji) and Parkin got much higher tunnel magnetoresistance with magnesium oxide barriers. But we also tried oxides like hafnium oxide and at the time we obtained TMR of about 20% with hafnium oxide. So, it was an interesting time with this experimental work aimed at developing magnetic tunnel junctions exhibiting tunneling magnetoresistance at room temperature. But again, I came back to Grenoble after this one-year sabbatical. Then in Grenoble, in 2001 I founded a new laboratory that we named Spintec, which stands for Spintronic and technology of components. The purpose was to accelerate in France and in Europe the transition from basic research in spin electronics to applications and in particular in this field of Magnetic Random-Access Memory, MRAM, which was rapidly growing at that time. So, the lab actually grew over time and now counts about 90 people, among them about 32 researchers and around 50 Ph.D. students and postdocs. So, we are entirely focused on spin electronics, studying various aspects of spin electronics. GMR has been the discovery which launched this whole field of spin electronics, but then the field really expanded in a lot of different directions. And today it is really a great pleasure to see new applications entering in the microelectronics industry with the MRAM and MRAM attaining volume production at foundries like Samsung, TSMC and others. So, this field is really ramping up and it's good for everybody working in this area to know that beyond magnetic storage there are these applications in microelectronics which are going to expand in the future.

Bajorek: How largest know the current effort? I know you mentioned various group sizes and number of graduate students. But total the laboratory right now. What's the population?

Dieny: It's between 90 and 100 people, actually. It's fluctuating depending on the number of students, but 32 researchers, around 10 administrative and technical staff and 50, 60 Ph.D. students and postdocs.

Bajorek: And the students can work at the lab as part of their graduate work at the university? Or you, they could get a degree from--

Dieny: They work in the lab during their Ph.D.

Bajorek: Okay.

Dieny: And then they get their Ph.D. from the local University.

Bajorek: From the lab.

Dieny: Yeah.

Bajorek: So you're able to issue, award a degree?

Dieny: Not ourselves, that's because our lab is affiliated to three organizations in France. It's affiliated to CNRS, to CEA and to Grenoble Alpes University. This is why we can host students and they get their Ph.D. degree from Grenoble Alpes University.

Bajorek: Yeah. It's a very nice arrangement, isn't it?

Dieny: Yes, yes, it is.

Bajorek: Very convenient arrangement.

Dieny: And the lab actually launched four start-up companies in the past 12 years related to various aspect of spin electronics.

Bajorek: So again--

Dieny: So it's good, yeah.

Bajorek: Bruce, how about you? What have you done since these exciting days?

Gurney: Chris, before I get to that, I have a couple of things. One is related to Cornell when I was leaving. A Professor John Wilkins used to come by my advisor's lab, Wilson Ho's lab, on a regular basis and poke around the way theorists are curious at what experimentalists do. And he found out that I was going to be leaving finally and going to IBM. So, he came by the night before I left and said, "Well, you know, the world of magnetism is dead as far as academia is concerned. Your job is to go to IBM and revive it." And I think that the spin valve GMR story represents a revival in magnetic research everywhere. I've seen it through my work with the IEEE, the number of participants and conferences has, it did go through a dip, and now it's come back up considerably. Magnetism is an exciting, vibrant field again. And so, my hat's to John Wilkins for--

Bajorek: For asking you, challenging way to do that?

Gurney: Asking me to do that. The second thing I have is some advice for people starting out their career. It's trite but it's true and I've tried to live up to this throughout my career and I think the gentlemen that are with me represent how that has worked for me. First of all, it's important to have a good project but it's just important to work with good people. Secondly, you know, recognize and trust your opportunities. And most importantly, do good science. That's my advice.

Bajorek: Good advice. Good advice. What are you currently doing?

Gurney: So what happened after the things that we've discussed is that Virgil wound up leaving IBM and I became the manager of that group and was the manager during the rollout of the first spin valve product. As I said, we continued to work on magnetic tunnel junction work. Later IBM around 2002 sold its disk drive division to Hitachi. I went with those people to Hitachi, where we worked on improving tunnel junctions, CPP GMR. And in my own case, work focused back to my origins again to this metal on metal deposition question, back to magnetic media where we would define nucleation sites either by patterning or with nano particles to create media with grain size distributions that were particularly narrow, which would be useful in magnetic recording and also to create patterned media. Subsequently, I've left HGST, which was owned by Western Digital, and I'm a freelancer now.

Bajorek: Good, good. Well, the ultimate right patterned disk is if you could make the grains all equisized and perfectly placed, right.

Bajorek: We knew how to do that across the entire size of the disk using nanoparticles.

Bajorek: That's-- that would make the ultimate disk, right. And Mustafa, you hinted that you're in a spintronics world. Did you go from IBM to this MRAM world directly or did you-- I somehow vaguely hearing you spend a little bit of punishment in the solar industry.

Pinarbasi: You came and visited me in the company. That's correct.

Bajorek: <laughs>

Pinarbasi: Actually, the IBM division was sold, I went with Hitachi. I was a distinguished engineer at IBM in the year 2000 and I was an advanced technology manager and working on the CPP and TMR head processing when I was at Hitachi. I left Hitachi in 2007 and joined the solar company. I was a VP first, and then I became CTO. We developed solar products and the flexible solar products and certified them. But the financials were obviously not favorable at that time. So, I left solar company beginning of 2013 and joined an MRAM company and I've been there since. I'm the CTO and the senior VP of Magnetics Technology. And basically, the company was very small when I joined and utilizing all the knowledge and experience that I had in the hard disk drive industry, I built a development fab where we can actually receive the silicon CMOS semiconductor wafer and we completely finish everything else in the fab and we make four megabit arrays and fully characterize those. The company I work at is the MRAM company with the key goal of not only the making the non-volatile memory for flash replacement, but our goal is to develop MRAM for SRAM replacement. So, we have to improve the speed, we have to improve the endurance and we have to improve the signal. We have very specific technologies that we are working on to make that happen. Eventually our goal is to really enter it into certain areas where we can use specific devices for DRAM as well as the standalone and as buffer structures as well.

Bajorek: Excellent.

Gurney: And I think the outlook for storage is extremely good. The exponential increase in the demand for stored information is continuing. And so, I think the big challenge for storage is really speed. And just as you were suggesting, Mustafa, being able to switch storage and being able to load and store storage at microprocessor speeds will allow for a completely new architecture of computing, so-called storage-level computing, and that's going to make what we're able to do with information quite a bit different than just being able to read it in serially, but to take whole 2D arrays of data and process it should be very much up Virgil's line, since that's going to involve things, including photography. So, I think for a lot of us, things were coming around to--

Bajorek: Full circle.

Gurney: Full circle in the storage world.

Pinarbasi: If I may add that we mentioned spintronics. Actually, when you look at it, GMR was the first spintronic device and product and the-- and there are many generations that actually happened since then. And the, in terms of the future, the way that I look at this one, when you look at it in the past, magnetics and semiconductors were two different worlds. There was no connection. And now with the MRAM, they really combined. And when you look at the semiconductor field, it's silicon, it is perfect world, right? All the devices are really based on the charge of the electron. What we do with the spintronics is we bring another dimension, that is the spin of the electrons. I believe the future devices are going to have to utilize another freedom in the properties of the materials, that is the spin of the electrons, and the GMR being the first one that we've been part of in the early and mid-nineties. And it's going to continue. Then there are still a lot of new inventions, new ideas from different materials to something called spin-orbit torque. I know that Bernard is working very hard on that one. And topological oxides and there is just incredible amount of excitement--

Bajorek: And opportunity.

Pinarbasi: Just from my part and opportunity in the field that we are very familiar with.

Dieny: Yeah. What is interesting today is that as you said, Mustafa, the microelectronics community and the magnetism community start to work together. And this was not easy at all initially, because we had a big cultural gap between these two communities.

Pinarbasi: If I may add Bernard actually has taken a lead on that front.

Speriosu: Yeah. Bernard, you know, he just played a major role in that.

Pinarbasi: He actually organized meetings with the semiconductor industry and magnetics industry.

Bajorek: Oh, yeah?

Pinarbasi: He's been doing this for a number of years in December, and so he's taken the lead role to combine and bring these two worlds together.

Dieny: Thank you.

Pinarbasi: So we all, we all thank you for that, Bernard.

Dieny: You're very welcome. Yeah, thank you.

Gurney: Yeah. For many, many years, the semiconductor industry treated cobalt, our-- and iron, our mainstay materials--

Bajorek: As poison.

Gurney: As contaminants.

Bajorek: Yes.

Gurney: And to be avoided at all costs.

Gurney: And so the amount of-- the activation barrier to get over that fear has been tremendous.

Bajorek: It's very big.

Gurney: And You've played a huge role in making that happen.

Dieny: As a matter of fact, when we showed to microelectronics people our magnetic tunnel junction stacks with 20 different layers, each of them a sub-nanometer thick, for instance ruthenium 0.8 nanometer thick, they always say, "This is impossible to manufacture."

Pinarbasi: Yeah.

Dieny: And in the microelectronics industry, the perception was even worse, with twenty different materials and this kind of thickness to control. So, we needed to explain that this can work in industrial applications as it has already been working for magnetoresistive heads for hard disk drives.

Pinarbasi: And show them that it works. We can do this.

Dieny: And show that it works in MRAM. But then we need a very good uniformity on the wafers, a very good yield.

Gurney: So thanks to the people in this room for demonstrating that originally.

Dieny: Yeah, yeah.

Gurney: That in fact, that level of control was achievable with equipment as crude as it was that we had at the time and which has been improved considerably because of the work of you all.

Dieny: And the nice thing about the introduction of MRAM on the market is that now this technology is accepted by the microelectronics industry. And since there are a lot of developments in the pipelines on the basic research side, all these developments will gradually lead to new products. So that's why I'm fairly optimistic about the future of spin electronics.

Pinarbasi: I still see Bernard, I see him at all the conferences together in the MRAM world, so.

Dieny: Yeah, yeah.

Pinarbasi: It continues.

Bajorek: I propose that we end on that positive and optimistic note. I congratulate you again for the wonderful accomplishments that you achieved. And I'm honored for the opportunity of being able to sit in front of you and do this interview. Thank you very much.

Speriosu: Thank you, Chris.

Dieny: Thank you, Chris.

Gurney: Thank you, too.

Pinarbasi: Thank you, Chris.

END OF THE INTERVIEW