The Evolution of Thin Film Magnetic Media and Its Contribution to the Recent Growth in Information Technology

My Personal Experiences
In Founding Komag Inc.

By Tu Chen

This book is dedicated to

My wife Pi-Fang (Nancy) and my sons, Glenn and John

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Foreword

Tu Chen was born in Taiwan in 1935 during the Japanese occupation of the island. He came to United States in 1961 to attend the University of Minnesota, where he obtained a doctorate in materials science & engineering. He started his career at IBM in Endicott, NY, followed by several years at Northrop, the defense contractor, in Los Angeles, CA. In 1971, he made the crucial decision to join the famed Xerox Palo Alto Research Center (PARC) in Palo Alto, CA to work on the development of various magnetic recording materials. Those were exciting times at Xerox PARC, where key components of today's computing technology were being developed. Key innovations such as the graphical user interface (GUI), object oriented programming, the Ethernet, and laser printers were all being developed at PARC. These were all parts of the famous effort at Xerox to develop the next generation of computers for the "office of the future" which was to become the basis of the personal computer revolution that came later. The important role which PARC played in the early development of the personal computer (PC) and associated technology is well-documented. The story of Steve Jobs's fateful visit to PARC in 1979 and how this lead to the eventual development of the Macintosh Computer at Apple Inc. is the stuff of Silicon Valley legend. The Ethernet technology that was the basis of 3Com Corporation, and the software development that led to Adobe Systems Inc., both had their beginnings at Xerox PARC as well.

Less well-known is the role that Tu Chen played at PARC in the development of magnetic thin film media and disks. Magnetic thin films are a key component of all hard disk drives being manufactured today. Development of small and inexpensive hard disk drives that store ever larger amount of data is one of the key contributing factors to the success of personal computers and it is the key engine that now drives the success of the Internet. It is what allows vast amounts of information to be available to everyone for practically no cost to the user.

This book is Tu Chen's recounting of the early development of thin film media, and it is also the story of how hard disk drive technology evolved to become so ubiquitous today. By founding Komag Inc. in 1983, he was able to commercialize hard disk media based on sputtered magnetic thin film technology. Disks were sold to many emerging hard disk drive companies that were getting their start in the early 1980s. Hard disk drives and their ability to inexpensively store huge amounts of data, and making it rapidly available to the user were the key contributions that drove the computer technology revolution.

Like many key innovators in Silicon Valley, Tu Chen is first and foremost a superb scientist. However, it is his skill in applying this talent to the creation of a product and commercializing it that sets him apart from most scientists. Those that have encountered Tu Chen even casually have quickly appreciated his intense competitiveness and sometimes fierce temperament. Nothing that Tu Chen ever undertook was done half-heartedly. Everything was done with intense passion and energy, and obsessive attention to detail. In addition, willingness to take risks and face immense obstacles is another trait that he possesses. These are also the traits that characterize successful entrepreneurs and they are prerequisites for achieving success in Silicon Valley, or for that matter, in any major venture. Most new technology is very difficult to develop. Commercializing it is even harder. This process however, is repeated many times in Silicon Valley's start-up companies. Some of them have achieved immense success, while many were destined to fail and quickly disappear. Those that plan to undertake such an adventure for the first time would be well-advised to learn from an experienced veteran. Tu Chen's story is well worth reading, to understand how he overcame so many obstacles in life and formed a successful start-up company. In the process, he made a significant impact on the advancement of magnetic recording technology and on the computer and information technology revolution that is taking place now.

Tom Yamashita
August 2013

Preface

In our current fast-paced lifestyle, it may be useful to take a moment to reflect on just how much our daily lives have changed in recent years with respect to how we access and interact with various forms of information. The computers that we have at work and at home, along with all the mobile devices, from smart cell phones to the new iPads, are now all interconnected and give us access to a vast storehouse of information through the Internet. All manner of information can be searched through the Internet using a service such as Google, and made instantly available for viewing. All matters of commerce, such as shopping, selling and banking, are now conducted on-line on the web. The Internet has lived up to its initial promise of transforming our lives and it is continuing to evolve still. Social networks such as Facebook have also sprung up recently so that we can create communities of friends and acquaintances that are connected all the time and share all manner of information in real time. It is obvious that these technologies are changing our lives in a profound way. Even our economic, social and political landscapes are being affected. The connectivity and access to information is radically transforming the world, and the old order is being replaced by something new and different. We will not know the full effect of this revolution that is taking place before our own eyes for many more years.

Thanks to this new technology, information has become truly much more democratic. A much greater fraction of the people on Earth now have access to information that was not available because of cost or due to political machinations of tyrants and dictators. I believe that one of the key factors underlying this revolution is that information has become very inexpensive to store and access. The old dictatorships that managed to control its populations through the control of information are now hard pressed to contain them. The floodgates have been opened up and it will be difficult to stand in the way of the information flow that is now pouring out. The key components of this revolution are quite visible and obvious. It all started with the PC revolution via which powerful computing capability was made available to the masses. Networking of these PCs to each other may have been the next big thing. Then the World Wide Web made the network more pervasive and available. Easy access to all manner of information became possible through software innovations such as HTML and the web browser. Yahoo and Google search engines made access to information much easier and faster. Access to these capabilities now through ever-smarter cell phones and pad-based devices are driving the next revolution in connectivity and access to information. Vast amounts of money are being invested in these new technologies, and creating huge new businesses in return. It is interesting to note that one of the most valuable businesses in the world today is Apple Inc. at a market capitalization of $400 billion, vying for top position with Exxon-Mobil. Apple hit a valuation of over $600 billion in October 2012, and if Apple was a country, its valuation would have ranked 19th by GDP, higher than Switzerland or

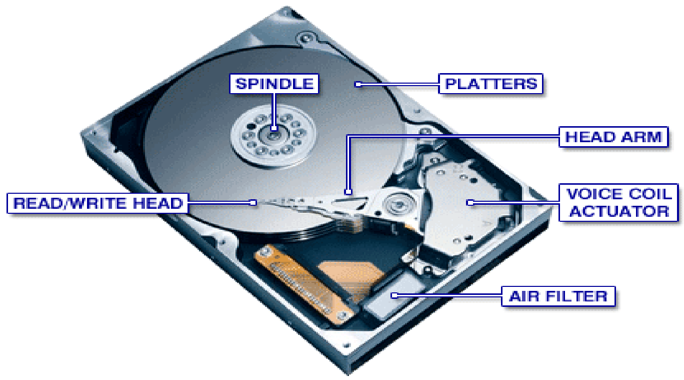
Saudi Arabia (Reference P-1). The fact that Facebook achieved the astronomical valuation of $100 billion at its initial public offering signifies the important fundamental change that is taking place in our commerce today. It is truly a revolution that is taking place in the way the world operates. It is clear that the financial world understands the value and the promise that this new technology brings to the table.

Some very large number of bytes		
Megabytes	=	10^6 bytes
Gigabytes	=	10^9 bytes
Terabytes	=	10^{12} bytes
Petabytes	=	10^{15} bytes
Exabytes	=	10^{18} bytes
Zettabytes	=	10^{21} bytes
Yottabytes	=	10^{24} bytes

Less obvious is the technology that underpins this current revolution in information technology. This technology is **data storage**, much of it based on magnetic recording technology. All the information that we use has to be stored somewhere and made available nearly instantly. Also, a considerable amount of such information is being provided practically for free. Google for example, is said to have now many exabyte of storage (10^{18} bytes or million terabytes), and a million servers to access it. While their search results, maps, YouTube, e-mail and many other services are being provided for free, they still generate close to $50B (50 billion dollars) in annual sales through advertisements and other services which they charge money for, and they are highly profitable. This scenario where massive amounts of data are being stored is repeated many times over by businesses and governments. The rate at which data are being stored is also increasing. It is rumored that Google alone adds petabytes (10^{15} bytes or 1,000 terabytes) of data every day. Our own government agency, the NSA (National Security Agency) is said to be building a data center in Utah to store national security surveillance data with yottabytes (10^{24} bytes) of data storage (Reference P-2). This figure is clearly an extrapolation and an outrageous number, as this would require a trillion 1 terabyte drives, and there is no physical way that such number of drives can be even built or put together. More realistically however, data storage requirement for this facility could be in the hundreds of exabytes eventually. It would still be a very large number, and the fact that outrageous figures such as yottabytes are even talked about now speaks to the explosion in data that are now being generated, and that people and organizations are attempting to store and organize.

Our financial transactions are also made much more convenient and faster due to fast access to data that banks and credit companies store. This database must also be vast. One can use Visa or MasterCard credit cards nearly anywhere that we can travel to nowadays, and our credit is checked nearly instantly. Much of these types of data will be stored on hard disk drives. Unless the data storage was inexpensive and efficient, none of this would be possible. We see with our own eyes the impact that hard disk drive technology has had in the last 30 years or so that we had the PC, as we have upgraded them to newer models every few years. Today (August 2013), a 1 terabyte hard disk drive can be bought for well under $100 from several manufacturers, and is widely available even in warehouse stores such as Costco. Compared to the 1970s, when I started my work in the field of data storage, this is beyond anything that I could have imagined back then. Even in the early days of the PC, in the early 1980s, a 10 megabyte hard disk drive cost $2,000 and it was much bigger than today's disk drives. Also, one usually had to buy them pre-installed in the PC by the PC manufacturer. I marvel at how far the technology for magnetic recording has come. Comparing the cost of magnetic data storage per megabyte for an HDD from the early 1970s to today, the cost of storage has decreased by **seven orders of magnitude**! This is, a ten million times reduction in cost over that time period. I believe that there is no other technology-based product that has had such a drastic reduction in user cost as the hard disk drive. I truly believe that underpinning of the aforementioned information technology revolution is the reduction made in the cost of storage, which is driven by advances in hard disk drive technology.

A hard disk drive (HDD) consists of a motor, media, recording heads and associated electronics to process the data. Today's hard disk drive is an extremely sophisticated and precision-engineered device that belies its low price. It is really a technology *tour de force* when it comes to the variety of the various sciences and technologies upon which it draws. The three HDD manufacturers that remain today--Seagate, Western Digital and Toshiba--produce around 700 million drives per year, with combined annual revenue of around $40 billion. For 2011, the total data capacity produced with these drives was around 400 exabytes. They employ hundreds of thousands of employees all over the world. The technologists that design, develop and advance the capabilities of these disk drives are amongst the most talented in numerous fields of science, including physics, materials science, electrical engineering, signal processing, software, chemistry, tribology and mechanical engineering, just to name a few.



Key components in a modern disk drive. Motor is hidden inside and below the spindle. (Figure P-1)

At the heart of the HDD is the disk or the medium on which the data are recorded. A disk drive may contain one or more disks and each surface that is used will have a very small head that is used to write and read the data to and from the disk. A "3½-inch" drive pictured above has a 3½-inch (95mm) diameter disk inside (platter in above picture). A typical drive can have up to 5 platters inside a 1-inch high disk drive box. The current highest capacity 3½-inch disk holds approximately 1 terabytes of data on two sides of the disk surface, corresponding to a recording density around 650 Gb/in^2 (gigabits per square inch - there are 8 bits to a byte). This "areal density" or "recording density" is a figure of merit for the performance of the media and the drive itself. The development on which I have spent the majority of my career is on the disk or medium itself. Strictly speaking, the term "medium" refers to the magnetic material that is coated on the surface of the disk, which is made of either aluminum or glass. But the terms "medium" and "disk" are often used interchangeably to refer to the same thing. It is only part of the hard disk drive, but it is one of the most important since it is what holds the data. The story I present in this book is about how this hard disk medium came into being, and how it evolved to its current form.

A few comments about the solid state drive (SSD) are worth mentioning, since it is being touted as the technology that will eventually replace the venerable HDD. Although the SSDs are now the storage of choice for USB memory sticks and cards, smart phones, iPads and high-end portables computers, large scale-high capacity storage will continue to use HDDs for a long time to come since HDDs are still much cheaper than SSDs. For example, a 120 gigabyte SSD drive costs below $100 now, but one can obtain 1 terabyte HDD for the same or lower price. SSDs based on semiconductor memory will remain quite expensive for large capacity drive applications, and there is still no infrastructure available for dramatically increasing SSD volume. With a typical chip fab line now costing $6 to $7 billion dollars, it is estimated that over one hundred such fab lines will be needed to just match the current HDD production capacity, at a cost of around $700B (Reference P-3). Not even the largest semiconductor company has that sort of capital to spend in the near term time-frame. In fact, to build even one fab line has become so prohibitively expensive that now only a handful of companies in the world can continue to play in this high stakes game. Even though typical users will continue to migrate to SSD devices on their mobile and notebook devices, the vast majority of storage will continue to use HDD and there is no alternative that we can see that will replace it. Even HDD manufacturers will be hard pressed to keep up with the increasing storage demand, as they too must continue to invest heavily in manufacturing capacity. However, the capital cost for capacity increase for HDD is at least 10 times smaller than that for the SSD fab. Nevertheless, the stakes are also very high and expensive, so that size matters. As noted earlier, as of now there are only three manufacturers left in the world that make HDDs.

There have been many improvements in areas of magnetic recording technology that encompass the hard disk drive. There have been improvements made to the recording/reading head, the channel, signal processing, drive design, and of course the media to record the data. However, I believe that it was the introduction of sputtered metallic thin film media for hard disk drives in the early 1980s that played one of the most important roles in starting the cost reduction process. You may think that I am biased in this opinion, since I have spent the majority of my career working on the development of magnetic media, and I started a company called Komag Inc., which was a very successful start-up that began manufacturing of sputtered thin film magnetic media. The story told in this book is about the development of sputtered thin film media. In large part, it is my own personal story about what happened at Komag Inc., but it also pertains to what happened in the entire HDD industry. I hope to convince readers of the importance of the development of thin film media to the unprecedented growth in capability of hard disk drives. Even today, media characteristics continue to play a key role in the performance advances in the hard disk drive.

The story of thin film magnetic media is also the story of how the progress in increasing recording density of a hard disk drive was accomplished. The marriage of inductive film heads with magnetoresistive films enabled development of dual element heads, with an inductive element optimized for writing and a magnetoresistive (MR) element optimized for reading. Adoption of these dual element heads dramatically accelerated the improvement of recording density (Reference P-4). The evolution to even more sensitive recording heads such as the giant magnetoresistive recording (GMR) heads and finally, the tunneling magnetoresistive heads (TMR) also played a large role in increasing the areal density. Albert Fert and Peter Grünberg were awarded the Nobel Prize in Physics in 2008 for discovering the GMR effect, which was, in part, recognition of the importance of their discovery to the field of magnetic recording. Greatly enhanced sensitivity of the read sensors allowed the magnetic bit size to be reduced in size; hence more bits could be packed into a smaller area on the disk. Smaller bit size required that media become less noisy achieved by making the individual crystalline grains of magnetic films smaller and better isolated from each other. As the grain size was reduced, however, the thermal stability of the individual grains holding the magnetization was reduced. Eventually, we started to reach the basic limit of the material in maintaining magnetization, called the "superparamagnetic limit," and this issue had to be resolved in order to continue the increase in recording density. Such issues caused the industry to migrate from longitudinal recording to perpendicular recording in recent years. These sometimes esoteric concepts used in magnetic recording technology will be explained in more detail as my story unfolds in the rest of the book. All these innovations on media to meet the requirements of increasing the areal density are as critical if not more critical than the innovations in the recording/reading heads, channels as well as in the signal processing technologies that are used in HDDs.

(Reference P-1)
Google Finance Search Results, October 2012

(Reference P-2)
http://www.wired.com/threatlevel/2012/03/ff_nsadatacenter/all/1
The NSA Is Building the Country's Biggest Spy Center (Watch What You Say)
James Bamford, Wired On Line
3/15/2012

(Reference P-3)
http://www.forbes.com/sites/ericsavitz/2012/04/12/seagate-ceo-luczo-on-drives-zettabytes-flash-and-his-tattoo/5/
Seagate CEO Luczo on Drives, Zettabytes, Flash and His Tattoo
Eric Svitz, Forbes Magazine
4/12/2012

(Reference P-4)
http://www.computerhistory.org/groups/storagesig/media/docs/Magnetoresistive_Heads.pdf
MAGNETORESISTIVE (MR) HEADS AND THE EARLIEST MR HEAD-BASED DISK DRIVES: SAWMILL AND CORSAIR
Christopher H. Bajorek, Storage Special Interest Group
Computer History Museum, Mountain View, CA

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Tom assisted me at Xerox PARC when he was sent over from Stanford University as an undergraduate in 1979 to gain experience in laboratory science. He was one of many Stanford students that Xerox PARC supported and interacted with during the 1970s and 1980s. He is a very intelligent scholar and a hard working researcher who I came to appreciate while working with him then. In early 1984, he joined me at Komag when we had just got started, and went on to work with me to solve many of the problems described in this book. He was my key right hand man at Komag for designing and developing materials and processes for media and many of the important discoveries mentioned in the book. Therefore, I need to credit him for his contributions. He eventually became the CTO of Komag in 2006 and continued to work for Western Digital until 2012 after their purchase of Komag in 2007. Another person who I am indebted to in preparing this book is Chris Bajorek who became CTO of Komag in 1996 and became a good friend. He has a tremendous amount of knowledge about how the HDD industry developed from his distinguished career at IBM before joining Komag. Chris reviewed the factual content of the book and provided valuable input. Finally, I owe a debt of gratitude to all the people that I worked together with to create Komag Inc., and make it successful. There are too many to list here, as they had so much to contribute to the success of Komag and to thin film media development.

Also a special word of thanks goes to following people who I owe for contributing to the content in this book:

Denis Mee for persistent encouragement to create this book.

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Finally and most importantly, I like to thank and give due credit to my partners that had lot to do with the success of Komag Inc. Without them, Komag would not have gotten started, let alone succeed. The key contribution that Jim Shir made in solving the tribology problems in the media will be described in more detail in Chapter 6. I should also mention the sacrifices. He had to resign from IBM to join Komag only 15 days or so before qualifying for the IBM pension for his 15 years of service, because the venture capital investors required him to sign up for the start-up. He risked everything on making Komag successful. Jim organized the R&D group and served as the CTO of Komag and worked so hard that he destroyed his health. I still feel deeply indebted to him for his efforts. Scott Chen also joined Komag with some health concerns, and we would not have had the success that we had without his ability to understand the drive customers' needs and put in the quality system at Komag. He and his group put in all the testing equipment and defined stringent performance requirements to make the Komag disk the most reliable and sought after product in the business. Also to Steve Johnson who brought business experience in running the company. We could not have grown as we did without his leadership in organizing the company, and bringing in the right people to manage the overall business for growth. It was the sort of experience that none of us had coming out of R&D and engineering backgrounds.

There are many other people who contributed their expertise and knowledge to the success of Komag Inc., from the beginning, with just a handful of people - to a major player in the thin film magnetic media business. I owe many thanks to the members of board who had served Komag while I was there, particularly Irwin Federman who was my personal mentor since the beginning of the company, as well as all the people that contributed so much in making Komag Inc. so successful.

Tu Chen
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Chapter 1

How I Got Started

My venture into the field of magnetism and magnetic materials began in 1957 when I was writing a thesis to fulfill the requirements for the Bachelor of Science degree in metallurgical engineering at the National Cheng-Kung University (國立成功大學) in Taiwan, Republic of China. While working on the thesis, I became aware of the role that magnetism plays in metallurgy. The subject of my thesis was enrichment of iron content and reduction of sulfur inclusions in a pyrite cinder for the purpose of converting the cinder from an undesirable waste material to a useful raw material for making steel. The pyrite cinder is the remaining cinder of iron sulfide ore, after the ore is burned to extract the sulfur for making sulfuric acid. Pyrite or FeS_2 is more commonly called "Fool's Gold." The remaining cinder consists of a large quantity of non-magnetic iron oxide or Hematite (Fe_2O_3), silica and residual sulfur. However, the iron content in the cinder is not sufficiently high enough to be economically useful. Therefore, the cinder is an undesirable waste material and an environmental nuisance. In my research I designed a process to eliminate the sulfur and at the same time convert the non-magnetic iron oxide to a ferromagnetic oxide, which is called magnetite (Fe_3O_4). The product is then subjected to a magnetic separation process to enrich the iron content to a level that is useful for making cast iron in a blast furnace. In developing this process, I designed a magnetic separator device that took into account the magnetic strength of the magnetite particles so that the separation process could be optimized. Many years later, I would learn that someone in Taiwan filed a patent on the process that I had developed, and that industry started to make use of the process. I did not receive any credit for having come up with the process in the first place.

While working on the thesis I became fascinated by the physics of magnetism and magnetic materials. This was the start of my lifelong interest in magnetic materials and it became the primary focus of my career. I cannot say that materials science of magnetic materials was something that I had intended to study from the very beginning. It was actually by default. I had taken a somewhat circuitous route to the university as I had failed the high school entrance exam on the first try and I took about a year off from school and worked for a doctor at his clinic as an assistant. I finally entered high school, but I left shortly after becoming angry at my Chinese language teacher for some insults that I received from him. I finally went back to high school to complete it but it had taken a little longer than normally. After high school I took the entrance examination for Cheng-Kung University but I barely passed due to my low scores on Chinese and English languages. I was always very strong in math and science, but quite weak in other areas such as history and languages.

During the time I worked for the doctor, I had some aspirations of pursuing medicine, but my grades were never good enough. Because of the low scores in the entrance examination to the university, my choice of departments to join was limited. I picked the metallurgy and mining department because space was available. This field was not the most popular choice for most students. Initially I thought that it would be quite interesting to become a mining engineer and I joined a field trip during the summer break of my sophomore year to a gold mine to see what it would be like. When we went down into the mine I became terribly claustrophobic and realized that mining engineering was definitely not for me. Metallurgical engineering was what I had to go into by default. At Cheng-Kung University I was very fortunate to be advised by a visiting professor from the University of West Virginia, Professor Fairbanks, who taught physical metallurgy. He came to the university under a Fulbright Scholarship to help with education at the school. He was the one that encouraged me to apply to U.S. universities to further my education and seek better opportunities. He provided guidance as to which universities to apply for. His wife even taught me English for a while.

After graduating from the university I had to serve in the Army ROTC in Taiwan to fulfill the military requirement, which lasted from August 1958 to February of 1960. I was assigned to be an artillery officer at Quemoy Island and this suited me quite well. Artillery involved math and physics, which were my strong suits. Perhaps as a result of this experience, I have often found myself using artillery analogies when explaining research and development strategies to people later in life. After returning to civilian life, I found that I could not find any meaningful work. Part of the reason was that I did not belong to the Kuomintang Party (also called Chinese Nationalist Party) and this was a prerequisite for finding work in government or in key industries. My family belonged to the original Taiwanese that had been living there long before the Mainland Chinese arrived in large numbers in 1949 to flee from the communist takeover of mainland China. The Mainland Chinese under the Kuomintang took over Taiwan and ruled the island under Marshall Law until the late 1980s. In fact, the Kuomintang itself owned many industries and businesses starting with expropriation of all Japanese owned industries after the Japanese surrender, and they also confiscated land and properties owned by many Taiwanese that were suspected of collaborating with the Japanese or opposed the Kuomintang. The Kuomintang Party is still considered to be the richest political party in the world with its true net worth and what they own still shrouded in secrecy (Reference 1-1). The original Taiwanese who outnumbered the Mainland Chinese by at least four to one were disenfranchised and marginalized, and many who opposed the Kuomintang were severely persecuted. In any case, I had no future in Taiwan and I needed to look for opportunities elsewhere. I took the advice of Professor Fairbanks and started to apply to graduate programs in metallurgy and materials science in U.S. universities. Professor Fairbanks's strongest recommendations were for the University of California in Berkeley, CA (UC Berkeley) and the University of Minnesota, which according to him had excellent programs in mechanical metallurgy and in the emerging field of materials science. While waiting for replies from the U.S. universities, I was able to find a job as a math and physics teacher at a girl's high school (today called National Lan-Yan Girls' Senior High School 國立蘭陽女子高級中學) in my home town of I-Lan (or Yilan) located in northeast Taiwan through a recommendation from the doctor that I worked for during my high school years. I worked at the school for about a year and a half. The pay was quite miserable--an equivalent of around $20/month. In addition to applying to UC Berkeley and the University of Minnesota, I also applied to the University of Toronto and to Missouri-Rolla (now called the Missouri University of Science and Technology) as backup choices. I was accepted to all of them with scholarships as it turned out, but the choice was really between UC Berkeley and the University of Minnesota. I decided that I needed to visit the two schools before deciding which offer to accept.

While I was teaching at the high school for girls, I met Nancy (Pi-Fang), my future wife. She was a student at the school. After she graduated, I happened to meet her again, and we started to date. Her family was from Mainland China and had come to Taiwan after the war. She faced considerable opposition from her family in even dating me. Nevertheless, we dated for about a year. When it became clear that I would be going to the U.S. to pursue a graduate degree, we decided to get married without her parents' blessing. When I left for the U.S., Nancy had to remain in Taiwan for a while, but I did not know for how long. She was already pregnant with my first son, Glenn. I borrowed $300 from a friend for airfare to the U.S., and my father gave me $100 for spending money. The flight took me from Taipei to Tokyo and then to San Francisco. During the two days of stopover in Tokyo I went to visit my schoolmates from college. I spent $10 in Tokyo and I had $90 left. I arrived in San Francisco and met another friend that was studying at UC Berkeley who put me up at his place and showed me around the campus.

The scholarship offer from UC Berkeley was from Professor Earl R. Parker who was already very famous for his work on dislocation theory and experimental work on dislocations. But this field of mechanical metallurgy was not quite where my heart was. Upon visiting the metallurgical sciences lab where I was to work I was quite disappointed. The facility looked more like an army barrack than a laboratory. I did not even bother to wait to see Professor Parker, and decided to put my bet on the University of Minnesota. But how to get there was a problem since I had very little money. My friend advised me to use the Greyhound Bus because it would be much cheaper than flying. The reality was that I did not have enough money to fly. It still cost $54 for the Greyhound Bus fare from the Oakland depot to Minneapolis, and the trip would take over 2 days. He also gave me the address of his friend in Minneapolis and told me that his friend would help me to settle there. For food my friend showed me how to make a peanut butter sandwich and I picked up a loaf of bread and a jar of peanut butter to sustain me for the trip. The only other food I bought was a pair of pears. I also bought one pack of cigarettes. During the ride the passenger sitting next to me was a Korean woman who had just arrived in U.S. for her second trip to get religious training in Christianity. Seeing that the only thing I was eating was a peanut butter sandwich she took pity of me and recommended that I eat in a restaurant at a stop in Omaha, Nebraska. I was told that it would be inexpensive but the meal cost me $2.70. I suppose that it was just a matter of perspective, but this was still a lot of money for me. The meal was not even that good. It was some non-descript meat and mashed potatoes dish with some beans. I also lost my pen and had to buy a new one on the way. I was left with only $27 when I arrived in the Minneapolis bus terminal. My friend in UC Berkeley did not inform his acquaintance at the University of Minnesota about my arrival, and no one was waiting for me at the terminal. This was not so unusual, because in those days telephone calls were quite expensive and most students did not use them. There were many Taiwanese students that went to the U.S. to study those days. We all depended on each other for help through a network of acquaintances and mutual friends. I just needed to look him up and my friend in UC Berkeley assumed that I would get help from his friend. The problem was to figure out how to get to his address at the University from the bus depot. I did not know how and my English was still too poor to even ask anyone properly. For a while I wondered aimlessly around the bus station trying to figure out what to do. Then I noticed that another Asian passenger was also walking around the bus depot, also looking lost. I introduced myself to him. It turned out that he was from Japan. He worked for Mitsubishi Corporation and he planned to attend the University of Minnesota to get a Master's degree in business administration. I could speak Japanese, as it was the language that I had to use at school during the long Japanese occupation of Taiwan. We started to talk to see if we could help each other. He also had an address at the University for a Japanese professor that he was supposed to see. While we were talking, an amazing thing happened. A Caucasian fellow with blond hair introduced himself to both of us and asked us whether we needed help. I remembered that he was on the same bus from Oakland, and he apparently took notice of me during the two-day ride on the bus. I asked him whether he could help us figure out how to get on a bus to the campus addresses that we had. His answer was that it probably would be too difficult to take a bus and offered to help us get to the campus address using a cab since he had several hours to kill before his next bus that was to take him to Montana. He found a waiting cab right away and we were headed to the campus. In the cab, he told us that he emigrated from Holland to the U.S. when he was 14 years old and he now lived in Montana.

As we rode the cab I was intensely watching the meter, wondering whether I had enough money to pay for the cab fare. I remember that I was sweating out every click of the meter as we were headed to our destinations. We arrived at the Japanese professor's residence first, and the Dutch fellow made sure that it was the right address and even helped to carry the Japanese fellow's luggage to the door. Luckily for him the professor was at home. Next stop was my destination. When we got there, a man answered the door and told us that the guy that I was looking for had moved out to Chicago. My friend in UC Berkeley did not know this. It was a big blow. But lucky for me, the guy that answered the door was another Taiwanese and he was quite happy to welcome me and help me out. Seeing that I would be taken care of the Dutch fellow said that he would be now heading back to the bus depot. I walked back to the cab with him thanking him all the way. I then tried to pay for the cab fare, but the man told me not to bother and that he will pay for it. The meter was already at around $10 and I probably would not have had enough money to pay for the round trip anyways. He then told me that when he arrived in the U.S. for the first time he was in a similar situation and some Good Samaritan took care of him and helped him out. He said that this was his chance to repay the debt to the Good Samaritan by helping us out this time. I will never forget this experience. I thought to myself, what a wonderful country this is to get help like this from a complete stranger.

The Taiwanese guy that took me in and helped me through the initial starting steps at the University of Minnesota was Lai Ching-Teh (賴金德 or Lai Kin-Toku by Japanese pronunciation). He was studying civil engineering at the University and we became good friends. He was quite fond of fishing, as I would become later. He had just caught some fish at a local lake when I showed up at his door and he cooked up a meal for me. I ate three bowls of rice with his fish that night. It was the best meal I had in a while. So, the University of Minnesota is where I was to spend the next few years of my life. A sequence of some very good luck and fortunate circumstances had led me to the University of Minnesota. It felt like this was my destiny. This was in 1961.

In order to start receiving stipends from the University one had to first register at the University. It turned out that in order to register you had to first pay a fee of $140. I only had a few dollars left but I was able to borrow the money for the registration from another friend. The stipend was $240/month. This was for a “half-time” appointment which meant that you obtained this amount during the academic year when you were taking courses. However, in the summer time, for about 3 to 4 months, we received a full stipend of $480/month. In the U.S. this was not very much money, even back then, but it was a princely sum for me. I was only making $20/month as a high school math and physics teacher in Taiwan. With the exchange rate of around NT$40 to US$ at that time, the stipend was a lot of money by Taiwan standards. With the stipend money it was typical for 5 graduate students to rent a 4-room apartment, which cost around $29 per person per month. We shared the food cost and cooked together. Food cost was about $30/month. With this university stipend, I quickly paid off my $300 airfare loan and $140 registration loan. Through my graduate career, I sent over $2,000 to Taiwan to help pay for schooling for my younger brothers and sisters in Taiwan. I had 3 brothers and 5 sisters, all younger than me. I had an older sister but she passed away early in life. So I was the oldest and I had a lot of responsibility in looking after my siblings. Of course, I also had to send money to support

Nancy and my newborn son Glenn. The pay was certainly very good for me but I was flat broke most of the time. The living arrangement in Minnesota was still quite good as I had my own room to live and study. This was a far better situation than anything I ever had in Taiwan with our large family crowded into a small house.

When I was accepted into the University of Minnesota, my main choice was to work with Professor John M. Sivertsen, but I also had an option to work for Professor Richard Swalin. Professor Swalin was already a well-known professor in the emerging field of semiconductor materials science, and he is famous for his textbook, Thermodynamics of Solids. He went on to become the dean of the school of engineering at the University of Minnesota. Professor John M. Sivertsen or Jack Sivertsen, the name that he usually went by, was a recent addition to the department with strong backgrounds in metallurgy and solid-state physics, with specialization in magnetism and the electronic properties of metals. He was working on magnetic materials amongst other topics and I was more attracted to his area because of my background and interest in magnetism from the National Cheng-Kung University. After talking to Jack Sivertsen, it was clear that I should be working for him, and I was very fortunate that I did. Jack Sivertsen had in-depth knowledge of magnetism and magnetic materials, and he was considered to be a leading authority in the field at that time.

In the early 1960s there was no field or department called "Materials Science" in the university as it is called today. In those days, research activities which we associate with materials science today were conducted in the metallurgical engineering department or in the solid state physics department. The metallurgical engineering department in the University of Minnesota was one of a handful of institutions in the U.S. that had materials science research programs dedicated to electronic device applications. Starting in 1958, the University of Minnesota invested heavily into the department with a new heat treatment laboratory, microscope rooms and darkroom facilities for processing photographic images and X-ray diffraction equipment. An electron microscope was soon added. It was on the cutting edge of emerging materials science research with very good facilities being built up. I decided to work under Jack Sivertsen, and magnetism and magnetic materials were to become the subject matter for my M.S. and Ph.D. degree theses. Looking back now, I believe that I made the right choice. I was able to utilize much of what I learned in school to contribute to the exciting evolution and growth of information technology.

Jack Sivertsen also helped in many other ways besides being my mentor and academic advisor. At the time I arrived in the U.S. in 1961, it was not possible to bring your spouse or family into the U.S. as a student, even if you had the money. There was a strict immigration quota for people of Chinese origin and it was not possible to bring Nancy and Glenn. In 1962, under the Kennedy administration, the law was changed to increase the quota for people from Asia, but it was restricted to people with a job, not for students on a scholarship. With that, I needed to finish my Master's degree as quickly as possible and find a job so that I could bring my family to the U.S. My Ph.D. degree would have to wait. But my academic and research work had impressed Jack Sivertsen sufficiently that he urged me to remain at school and continue working toward the Ph.D. To that end, Jack Sivertsen helped me by giving me a position as a research associate, which is considered to be a university employee rather than a student. I had not even obtained my Master's degree at that time. This was so that I could apply for the visa for Nancy and Glenn. I sent in the application in 1962 and it was granted in August of 1963. Nancy and Glenn finally arrived in the U.S. in September of that year. We had been apart for two years, and it was the first time I saw Glenn. He was only a year and half old, but already speaking Taiwanese quite well. This was quite remarkable to me. We were able to live as a family for the first time. My second son, John was born in 1965 in Minnesota.

As a research associate, Jack Sivertsen had to pay me a full salary of $480/month. This helped with my dire finances and also allowed me to apply for the visa. This put considerable strain on his research funds, but he managed to keep me in this status until my family arrived. Once this was accomplished, he put me back to a student status so that I could continue with my studies toward the Ph.D. At a later time, in 1966, this maneuver caused some issues for me with the U.S. Immigration Service because they started to question it. But Professor Sivertsen came to bat for me again and set things straight with the U.S. Immigration Service. I am greatly indebted to Jack Sivertsen not only for being my mentor and advisor, but also for helping me to bring my family to the U.S. I was able to pursue the American Dream together with my family. My stay in Minnesota was to last six years, four of which were with my family.

For my Ph.D. research, the work involved the study of short-range order in Cu-Pd alloys. I investigated how the atomic structure and clustering affect the band structure of the material (Reference 1-2). Cu-Pd is paramagnetic, which is different from the Fe-Au system that I worked on for my Master's thesis. I used x-ray diffraction as a tool to determine the effect of thermal quenching and aging treatments on the atomic structure of various compositions of the alloy system, and used magnetic and resistivity measurements to determine the change in their electronic band structure. I completed the thesis in 1967, for a Ph.D. in Metallurgical Engineering. The project was a continuation of the work that had started with Bob Sundahl who studied Au-Fe alloys for the effect of short range ordering in atomic structure on magnetic properties, also under Professor Sivertsen (Reference 1-3). I participated in this project with Bob Sundahl when I first joined the group, and it became the basis for the deep understanding that I gained on magnetism, magnetic properties and their relationship to material characteristics such as atomic and crystalline structure. I also gained tremendous knowledge of various magnetic and materials characterization and measurement techniques. Bob Sundahl was my mentor and helped me to develop my research skills. He also became a very good friend, and we continue our friendship to this day. Bob went on to Bell Labs after his graduation and then had a distinguished career at Intel as a researcher for many years until his retirement.

(Reference 1-1)
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Chapter 2

My Career after obtaining my Ph.D. (1967 to 1971)

After finishing my Ph.D. in 1967, I began interviewing for a job. I first interviewed with Union Carbide in Cleveland. They were working on carbon composite materials with metal reinforcement. I was offered a job but the job did not appeal very much to me. My 2nd interview was with the Bell Telephone Laboratories in New Jersey and in Allentown, Pennsylvania; and with Western Electric Laboratory, which was a division of Bell Telephone located in Princeton, New Jersey. I wanted a job with Bell Labs, but did not get an offer. Instead, I got an offer from Western Electric which I did not accept. My third interview was with IBM in Endicott, New York. They wanted me to work on a project to develop hybrid circuits, which used ceramic substrates and a sputtering process to form metal interconnects and thin film resistors made out of nichrome (NiCr). They really wanted me because of my work on the electronic transport properties of metallic Au-Fe and Cu-Pd alloys, and they thought that I would be a very good fit. The job looked interesting so I accepted the offer and started in 1967. As I started work on the hybrid circuit project, I quickly became aware that Endicott was competing with two other IBM sites for the design and Endicott was in 3rd place behind the Yorktown Heights laboratory, which was in the lead, and East Fishkill, which was second. This was not told to me during the interview and the situation was not particularly motivating right from the start of my job. However, there were other projects that came my way. One was to develop a superplastic aluminum alloy to use on the IBM Selectric typewriter type ball. The type ball then in use was made from cast aluminum-zinc (AlZn) alloy, and someone had thought that if a superplastic version of AlZn alloy could be used, the part could be made cheaper by using an extruding method. This work also involved heavy use of Endicott's analytical facilities, including a brand new transmission electron microscope (TEM). I was such an active user of these new analytical tools that they quickly put me in charge of the TEM and X-ray equipment. Developing the superplastic AlZn alloy involved the use of phase diagrams, annealing experiments and microstructural characterization. These types of projects were, at best, side projects that were not that important to IBM and the work was not very fulfilling. I was more interested in doing fundamental work. From the point of view of gaining experience, it was not all wasted, however. I learned quite a bit about the sputtering process and the use of sophisticated analytical tools such as the TEM.

It did not take me long to start thinking about changing jobs after being there for only a short time. Another factor was experiencing my first winter in New York. Minnesota was very cold in the wintertime, but I managed for many years without ever having to use snow chains on my car tires during the winter, for example. The thing that made the difference for New York was that it was hillier, and the conditions of the roads were quite poor. There were a few instances where I could not get home because the road would freeze over and I could not make it up the hill to get to my apartment. On one occasion after a freeze, I bought chains and installed them on the tires by myself. Unfortunately, I must have installed them improperly as I had to do this in darkness. After driving around with this chain for a while, I had completely ruined my tires and that was the last straw for me. New York winters were just more difficult to live with compared to Minnesota winters. After the chain incident, I decided that I needed to move out and I asked my manager whether it was possible to transfer to another IBM site. There were three possible choices: East Fishkill, Yorktown Heights and San Jose. I interviewed for the East Fishkill job, which involved work on germanium. Back then, germanium was still competing with silicon for supremacy in ICs. I could have taken this position but it was still in New York and I was not an expert on semiconductor technology. San Jose did not have any openings at first so that they would not interview me until fairly late. By the time they came around to interview me, it was too late. I already had made up my mind to leave IBM. The last thing that made the decision to move easier for me was that my apartment complex caught on fire and we had to move out. This was a good time to leave New York for good as I was sitting on an offer from Northrop Corporation in California. My job with IBM had lasted only 8 months.

In 1963, the University of Minnesota metallurgy department recruited a new assistant professor, Y.P. Gupta, from MIT. He did not stay for very long and he went on to become a lab director at Northrop Research and Technology Center in Hawthorne, California, a few months before I graduated. Northrop was involved in trying to develop Permalloy wire for memory devices, and Y.P. Gupta was looking for someone to work on the project. He knew about my work at the University of Minnesota and he must have thought well of it. He had tried to recruit me before I took the job at IBM Endicott. I had turned him down then. Once I became unhappy with the situation at IBM, I called Y.P. Gupta and the position was still available. The attraction of the Northrop job was that the work would be more fundamental in nature and I would be able to fully develop my scientific knowledge and work on innovative materials. I hoped to use my knowledge in magnetic materials to full use. And lastly, the job was in Southern California, which had much better weather.

Northrop is known today as Northrop Grumman Corporation and it is one of the largest defense contractors in the U.S. They are famous for having developed the B-2 Stealth Bomber. What Northrop wanted me to work on was a plated Permalloy wire for use as a data storage material on military defense systems. The concern back then was to have a storage device that was radiation resistant. The issue was that these devices had to survive a nuclear blast and semiconductor memory was susceptible to radiation damage from such blasts. This idea of Permalloy wire for memory apparently came from a vice-president who headed the

Research Center and it was his pet project. As such it had funding and visibility. Instead of a wire, the work quickly migrated to tape and thin films. At Northrop, I got the opportunity to learn various thin film deposition technologies and the physics of thin films. During this period, we were in the middle of the Cold War and we were able to obtain defense contracts for extended periods of time on magnetic materials research. I also worked on other projects, such as on radar absorbing paint for stealth capability, holographic storage materials, cold cathodes for use as a flat panel display on military aircraft and on zinc selenide (ZnSe), which is an interesting high band gap semiconductor material with improved radiation immunity. I also learned how to grow single crystals, which became useful for me later. The work on Permalloy films for data storage was the most important work for me. Working for Northrop was very fortunate since it was on the basis of my experience there that I eventually got the job at Xerox PARC (Palo Alto Research Center).

While I was working at Northrop Research Center, I lived with my family in the beautiful Palos Verdes area in Los Angeles. The house was near the top of the hill, and I was living the American Dream. In the neighborhood where we lived, there were many other engineers like me working for aerospace or technology startup companies located in the southern part of Los Angeles. One of those neighbors was Andy Lin, who was a program manager for new product development at Scientific Data System Corporation (SDS). One day when Andy and I were talking about our work, he realized that my technical expertise in magnetic thin film could be quite useful to his work. His team was developing the next generation advanced computer system, which included data storage devices for SDS. He tried to entice me to join SDS, but I was enjoying my research at Northrop and I declined his offer. I told him that I preferred to work in a research environment rather than in a product development environment, which was the SDS job. I wanted to work on advanced materials science research and publish papers while I was still young so that I could establish myself in the field. I also did not enjoy the job that I had briefly at IBM and I did not want to go back to a similar situation.

About a year later, in October of 1970, Andy told me that SDS, which was sold to Xerox in 1969 for about $900 million, intended to establish a new research center in Palo Alto, California to be called PARC (Palo Alto Research Center). This new research center would support SDS in developing new computer products. Xerox, having made considerable amounts of money in office copiers, had their sights set on the “office of the future” which would involve the use of computers. SDS was founded by Max Palevsky and it became quite successful in selling moderately priced computer systems that were used at large corporations and also at government agencies such as NASA and the military. They were early adopters of silicon transistor ICs and carved out a surprisingly large market niche for computers that was dominated at that time by so-called “Snow White” (IBM) and the “7 dwarfs” (NCR, Burroughs, Control Data Corporation, General Electric, Honeywell, RCA and UNIVAC).

Xerox bought SDS from Max Palevsky with 10 million Xerox shares and it was one of the biggest purchases by any company at that time. Max Palevsky became enormously wealthy (and famous) by this deal and his name will re-surface again later in my story. Xerox was going to use SDS and PARC to go head-to-head with IBM for the new business opportunity of office computing. Andy encouraged me to join PARC so I could help them develop magnetic data storage devices for SDS's future computer systems. He told me that the job at PARC would be to do fundamental research. I thought that this opportunity was quite interesting now because it was to be in a research laboratory environment, and I was also beginning to worry about long-term job security in the aerospace industry. Around that time, President Nixon had just announced reduction in funding for B-1 bomber development. This had a big impact on the aerospace industry as a whole. With job security in mind and a chance of working in a research environment to do what I like to do most, namely on magnetic materials, I decided to take Andy's advice and went to interview at PARC in October of 1970, for a job in PARC's General Science Laboratory Group (GSL).

At the interview, I learned that PARC had just been founded and that they had moved recently into the new building. They were located in the foothills of Palo Alto behind Stanford University. The entire area was part of Stanford Research Park, and it was already occupied by Hewlett- Packard as the anchor tenant. GE and Lockheed also had research facilities there, along with Varian. It was a beautiful location and strategically located near Stanford. Xerox believed that the combination of excellent R&D established at PARC, SDS market presence in scientific and engineering communities, and Xerox's strong presence in the office business environment through their copier business, would make them a strong player in the emerging field of office automation. They also told me that Xerox's diversification into the computer business was based on the feeling that the core copier business had limited long-term growth potential and the emerging digital information processing business would be their new opportunity. Xerox believed that this "Office of the Future" would be all digital and that everything would be electronically connected. This vision of the future would use much less paper, and hence less copying. It was a very serious and strategic undertaking for Xerox. Therefore the interviewers told me that they needed a person with my skill to work on Permalloy thin film magnetic media for mass data storage devices which then could have been used by SDS.

I also learned that PARC was organized into four independent laboratories, the Computer Science Laboratory (CSL), the System Science Laboratory (SSL), the Optical Science Laboratory (OSL) and the General Science Laboratory (GSL). The background of Xerox's management strategy and formation of PARC at the time is described in detail in the book Fumbling The Future: How Xerox Invented, then Ignored, the First Personal Computer by Douglas K. Smith and Robert C. Alexander in 1988 (Reference 2-1) and Dealers of Lighting: Xerox PARC and the Dawn of the Computer Age by Michael A. Hiltzik in 1999

(Reference 2-2). Soon after the interview, I got an offer from the acting director of GSL, Dr. Gerald Lucovsky. I accepted the offer without any hesitation because I was sold on the great Xerox vision. I told them that I would like to join them in February after I finished transferring my work to other people at Northrop and after taking a long overdue vacation back to Taiwan.

Once Dr. Lucovsky agreed to my start date, I resigned from Northrop and began the work of transferring my research work to other researchers in Northrop. This was completed by the end of December. Thereafter I took a long six-week vacation to return to Taiwan with my wife and two sons. This was the first time I had returned to Taiwan since leaving for graduate school in 1961. Ten years had gone by. I never had a chance to go back for a visit because I was too poor during my graduate school years, and became too busy with work and developing my career after graduation. I also wanted to settle down and have some peace of mind before going back to Taiwan. The job transition to PARC was the perfect time to return to Taiwan.

During those six weeks in Taiwan, it was very enjoyable to see all my relatives and friends. I met my parents, brothers and sisters, grandmother and many relatives that I had not seen in 10 years. I also went to visit my university where I did my undergraduate studies. It was very nostalgic to see the laboratory where I did my thesis project. Of course many things had changed since I had left, but some of the equipment that I used was still there. I went to see one of my professors, and talked to him about my career and the work that I was doing. I mentioned about my excitement for the PARC job, and the opportunity to develop new magnetic media for computer data storage devices. My professor remembered about my interest in the field of magnetic materials and he gave me enthusiastic encouragement to do good work at PARC. He even predicted that I would be successful in making new discoveries in my research work. In those six weeks while I was in Taiwan I was not only exited to see my relatives and old friends again, but also eagerly anticipating the start of my new career at PARC with all the possibilities for doing good work there.

(Reference 2-1)
Fumbling the Future: How Xerox Invented, then Ignored, the First Personal Computer
Douglas K. Smith and Robert C. Alexander
W. Morrow, 1988

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Michael A. Hiltzik
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Chapter 3

Joining Xerox PARC

Beginning my Research Career at PARC

After returning from the trip to Taiwan, I was very excited to join the visionary team at PARC and launch my new career there. However, on the first day that I reported to work at PARC in mid-February of 1971, I was shocked and dismayed to be told by the acting director of GSL that the Permalloy data research project had been cancelled. Fortunately however, he told me that I still had my job and that I was to spend some time to help organize the materials research laboratory and identify new research topics that would be useful to PARC. It was quite a surprise and a shock to me that in less than four months since my interview with PARC, the strategy and direction of research efforts at PARC had changed so drastically. I was disappointed and secretly wondered whether Xerox PARC may be similar to the aerospace companies, which constantly change their research directions and projects just for the purpose of securing funding from the government. I felt some regret in jumping from one uncertainty to another. Worse yet, no one explained to me the actual reason for the sudden change in strategy. It was sobering to realize that my position did not warrant management explaining their decisions to me. My area in the GSL was at the bottom of the totem pole in their organizational hierarchy. Of course, I had just started there also.

It was actually many years later that I finally learned what had happened with PARC's strategy. In reading the book, Fumbling the Future, I finally realized that PARC management was spending considerable effort in redefining their vision for the Xerox computer business around the time that I joined PARC. The new strategy was very different from the original Xerox plan when they had bought SDS. PARC's new vision was based on creating an entirely new and revolutionary interacting computer technology, which turned out to be the right vision after all.

When people talked about the "office of the future" back in 1971, things were not very clear as to what that really meant. Computing was certainly a big part of the equation, but computers were large and very expensive machines back then. One idea for distributing this computer power to everyone was to "time share" the processor time among many users through a centralized system and software accessed through "dumb" terminals. SDS had this sort of concept as well, and many large computing companies were working on this model of distributed computing. From the macro-economic view point, investments taking place in the factories through automation equipment were generating tremendous productivity gains during this time period. In terms of dollars invested per blue-collar worker, the gain in productivity was very large. When people thought about this for the large employers of white-collar workers, it was not so clear what sort of comparable investment would boost the productivity in the office. What PARC set out to do was to address this issue. The first question was what sort of budget a typical company might set aside per white-collar employee in a bid to boost productivity in the office. The figure was around $10,000 per person. Within this budget, PARC asked what such office would look like. They had the vision of providing a small computer to each employee, which would be connected to all the others and working interactively. Software would do what one normally does in the office, such as generating memos and analyzing data, creating charts and communicating them to everyone. In addition, there would be printers that would be shared by everyone for printing hard copies when needed, but the office of the future would be largely electronic. So within this $10,000 budget, one would need a computer, software, distributed printing and most importantly, storage for data and software. This was a radical departure from what most people envisioned. First of all, it was hard to imagine being able to do all of this on a $10,000 budget. Computers still cost a million dollars then, and storage devices sold for around $100,000 or more. But this was the new PARC vision for the office of the future. In hindsight, PARC was way ahead of its time. Their new vision became the cornerstone of what led to the rapid growth of the personal computer industry in the last three decades.

Looking back now, the feeling of uncertainty that I felt in the beginning of my employment at PARC was due to my inexperience and lack of understanding of the computer industry and computer technology. I was quite ignorant about computers at that time, but I chose PARC for the purpose of applying my knowledge of magnetic materials technology for data storage. Now I feel quite fortunate that I made the right choice in going to PARC. Their vision of the future became a reality and I was able to work on part of that grand vision from the ground up. I was still young and full of energy. My whole career in magnetic materials and their application to data storage was all out in front of me and I was in a perfect place and time in history. Of course at that time, I did not know this.

Search for a New Project at PARC

Work on acousto-optic crystal to support a laser scanner for the laser printer project (1971-1972)

A few weeks after I joined PARC, I started to settle down and focus on figuring out what I could do to be useful there. My first job was to organize the Materials Research Laboratory. PARC was in a leased temporary building with many empty spaces, but there was no laboratory equipment. Therefore, I undertook the task of building a general research laboratory equipped with commonly used instruments for materials research. At the same time, I spent time talking to people in the GSL and OSL to identify projects that I could work on. The first project I identified was to create an advanced acousto-optical crystal which had a figure of merit higher than the state-of-the-art material, which was lead molybdate (Pb_2MoO_3) and which was used in laser printer applications. The laser printer was under an intense development effort led by Gary K. Starkweather at PARC's OSL group at that time. My understanding was that the laser printer was a very innovative concept to be used by computers to print out a hard copy of the digital information as text or as graphic images. In this concept PARC was again way ahead of its time and they were right on.

The printing mechanism of the laser printer is based on the same principle as the xerographic printing process, except that the light exposure on the surface of the photo receptor, in order to create the electrostatic charge for attracting the toner particles, was made with a single laser beam instead of a large area exposure as used in conventional xerography. The single laser beam is modulated by a stream of digitalized information from the output of a computer to expose a discrete area on the surface of the photoreceptor. To print a whole page of the paper, the modulated laser beam has to be raster scanned across the whole surface of the photoreceptor.

The initial design of the raster scan mechanism was a mechanically driven 32-facet polygon mirror for steering the laser beam. However, the earlier version of this mirror was not adequately stable to generate a straight line. Therefore, a decision was made to create a backup device to replace the mechanical scanner with an acousto-optical crystal scanner to steer the beam. Consequently, a search for a high acousto-optic figure of merit crystal was launched in parallel with the work on improving stability of the mechanical scanner. To undertake the search for a new crystal material with a higher figure of merit than that of lead molybdate, I read many publications on acousto-optics to learn about the field. As a result, we were able to produce a single crystal of bismuth molybdate, $Bi_2(MoO_4)_3$, which had figure of merit 1.5 times higher than that of lead molybdate.

In cooperation with the physicist Dr. David Biegelsen in our department, the research effort took us over a year of hard work including development of a method of growing the first single crystal of bismuth molybdate in the world. However, even though the research was a success, it was never used in the laser scanner application because by the time the acousto-optical crystal was produced, the team that worked on the mechanical version of the scanner was able to solve the stability problem, and the company decided to adopt the mechanical scanner in the laser printer. At any rate, the research provided me with a chance to learn a new subject and gave me the confidence to be useful at PARC. Even though the device was not used in the revolutionary printer product, we were able to produce many publications on the work on bismuth molybdate. It turns out that a new phase in the bismuth molybdate phase diagram that I discovered from the single crystal work became an important material as a catalyst in the petroleum cracking process. I found out about this later from a scientist at the DuPont Corporation who called me up to ask for my publication and instructions on how to grow the crystal.

While we were working on the acousto-optical scanning material, we never forgot our first priority to do research on the future of computer data storage technology. Therefore, we spent time to search the literature for various data storage technologies and investigated the potential for low cost data storage based on a magnetic medium. By 1972 we were able to identify a couple of directions to take on data storage media.

Magneto-Optic Memory Research.

In 1972, there were three possible choices for magnetic recording. One was the rotating disk storage invented by IBM using iron oxide, which was already in use commercially. The second one was magneto-optics and the third was magnetic bubble memory. I investigated these fields and decided that improving on the storage density of hard disk media would not be an easy task. At the same time, the physics of magneto-optics and bubble memories were "glamorous" topics those days. They were both difficult, but they were clearly identified as the emerging technologies that should have a storage application. Many researchers were involved in looking at magneto-optics and bubble memories, and I was also drawn into it. It was fashionable enough for a young researcher like me to spend time on it and try to make a contribution to the field.

Magneto-optic (MO) and bubble memories were proposed in the mid-to-late 1960s. The MnBi magneto-optic memory film was proposed by D. Chen et al from Honeywell (Reference 3-1) and the magnetic bubble memory was invented by Andrew H. Bobeck at Bell Labs (Reference 3-2). These technologies were quite innovative and very sophisticated. They appeared to be more exciting for researchers to be involved in it than on twisted wire or plated film based on Permalloy thin film technology, or hard disk drive storage. Therefore, like many researchers, I was attracted by these new emerging technologies and decided to investigate them starting from reading the published literature and by attending conferences while I was working on the acousto-optical device in 1971. An MO memory system uses a rotating disk similar to hard disk storage. However, unlike a conventional magnetic recording disk system, which uses a magnetic medium and a magnetic head for writing and reading data, the MO drive uses a special class of magnetic material that exhibits strong magneto-optical properties, and a laser for recording and reading the data.

In MO recording, the writing process consists of heating the medium with a laser spot in the presence of a magnetic field, which is supplied by an electromagnet. To read the data, the same laser beam is used but at a lower power. A material exhibiting what is known as the Kerr effect is used, whereby the polarization of the laser light changes with the magnetized state of the medium. This effect is used to detect the bit that has been recorded. Even though the MO medium is also a magnetic film, its direction of easy magnetization is perpendicular to the film plane. In the writing mode, bits of information are stored by magnetizing small regions of the media by heating the region to be recorded with the laser beam and the locally applied magnetic field. Depending on the direction of the applied field (up or down relative to the film plane), the direction of magnetization of a written bit can be made to either point up or down relative to the film plane. During the reading process, the magnetized bits will cause rotation of polarization of the reading laser light. The amount of the rotation of the polarization by the magnetized element depends on the magneto-optic activity of the material. Greater rotation of the polarization by the MO media nets higher contrast and signal to noise ratio in the readout signal. The amount of this MO Kerr rotation is the figure of merit of the MO material.

By the late 1960s and early 1970s, there was extensive research work done and many papers had been published on development of an MO medium and drive. It was projected at that time, based on experimental and theoretical work, that a Manganese-Bismuth (Mn-Bi) alloy film-based MO system could reach 100 Mbit/in^2 recording density. It was also speculated that if one were to use a shorter wavelength laser, the areal density could be pushed beyond 1Gb/in^2. Comparing the number for MO to the state of the art hard disk drive technology using iron oxide particle media (Fe_2O_3), the MO technology naturally looked very attractive and also more glamorous. Oxide particle media were only capable of recording densities around 1 Mbit/in^2. It should also be noted that the laser was only invented in 1960, and semiconductor lasers were invented in 1962. Laser research was also very active everywhere and PARC had its own research on the development of semiconductor lasers for use in printers. Combining lasers with rotating magnetic recording appeared very elegant and attractive. Like many researchers, we were also convinced and quickly jumped into MO research in 1972.

As for bubble memory, I decided to stay away from it even though it was scientifically interesting and very fashionable to work on it at the time. I had concluded that it would have been necessary to grow a near perfect single crystal of Gadolinium-Garnet material for the data storage layer, and this I perceived would have been very difficult. All crystalline materials routinely contain imperfections in the crystal lattice, called dislocations. Thermodynamics stipulates that the inclusion of dislocations is energetically favored; therefore it will be very difficult to eliminate them. I thought that such lattice defects would be a big problem for bubble memories. Furthermore, the cost per megabit of the storage device based on bubble memory would not be able to compete with a hard disk drive or MO drive. Perhaps the real target for bubble memory was for a system memory to compete with DRAM. In any case, I had a metallurgical and materials science background and I had different viewpoints compared to those of researchers having a physics background, for example, when it came to mass storage. I thought a lot about the material itself and how it could be made and how practical it would be to actually make a device that would be manufacturable. The bubble memory field appeared to be dominated by physicist types. There were other considerations as well. One discouraging factor was that there were already too many people working on bubble memories at that time, and they were way ahead of us. Considering the limited resource that we had, we decided to focus our effort on the MO memory device. My decision to stay away from the bubble memory proved to be a correct one as bubble memory became a bust and never succeeded commercially.

When PARC decided to create a lower cost office computer in late 1972, we were able to propose to management that we could focus on developing the MO media for the low cost memory device the computer could use. The fit was perfect. I asked for and obtained the funding for all the equipment needed to fabricate and test the media, and immediately went to work on the project. Following the earlier research by others, we started to do the work on MnBi thin films for MO media. MnBi thin film media were made by alternating thermally evaporated layers of Bi and Mn, followed by thermal annealing at about 300^{o}C in-situ in the same vacuum system. We were able to quickly reproduce the results by others, but realized that in order for MnBi to be useful, we needed to solve the media instability under repeated thermal-magnetic writing cycles. The material degraded as one used it. The other problem was the irregularity of laser written bits around the periphery of the bit. The irregularity of the written bit is caused by the strong demagnetizing field and the structural imperfections in the material, such as grain boundaries in the vicinity of the written bit. These two problems were the main concern for using MnBi thin film for MO storage media. We were not the only ones seeing the problems. Other research groups were busy trying to figure out how to solve these two problems.

One of the solutions proposed for the media instability was to add alloying elements into MnBi films to try to make MnBi more stable. However, the results were not promising. These were ad-hoc trial and error types of approaches. Therefore, we decided to spend time investigating the nature of MnBi itself. Following the time-tested approach in materials science, we started to look at the phase diagram of the MnBi system and noticed right away that the phase diagram was incomplete at the high temperature portion of the diagram. For example, if there is a phase decomposition of the compound at higher temperature, the media could be decomposing under repeated thermal cycling. In order to better understand the phase diagram of MnBi, we decided to grow a single crystal MnBi so that we could study the detailed behavior of the compound and its phase diagram. I found a used Czochralski crystal puller from the Xerox Rochester Lab, and started to try growing a single crystal of MnBi. I had some practice in growing single crystal germanium at Northrop, so that knowledge and experience came in handy.

We were able to successfully grow single crystals of MnBi, and this was quite exciting as this was the first time this was ever achieved. With the single crystals on hand, we were able to investigate thoroughly the effect of temperature on the compound. The results showed that the high-temperature phase of MnBi is in fact a separate compound with the chemical formula $Mn_{1.08}Bi$, when grown from a Bi-rich solution. It was found that the transformation of MnBi at 355°C upon heating is associated with a peritectic decomposition of MnBi into $Mn_{1.08}Bi$ + liquid, and the transformation at 340°C upon cooling of the high-temperature phase is the result of decomposition of $Mn_{1.08}Bi$ to MnBi + Mn. Stated more simply, the material would undergo decomposition with heating and cooling, and one could never stabilize this material. Working with single crystals of MnBi and trying to understand this material based on determination of its phase diagram made it absolutely certain that MnBi could not be a stable material to use for an MO application. Therefore, it would have been a waste of time to pursue the work on MnBi media any further. From the results of the work, we published several papers on the subject involving the phase transitions and the magnetic properties of the compound. Consequently, I was invited to talk about this work at the 1974 Intermag Conference in Toronto, Canada (Reference 3-3). After that presentation, I believe that the research on MnBi thin films for MO media applications stopped completely across the entire research community. It is a rare situation where one piece of work completely wipes out so many research activities all at once. I killed MnBi.

Even after we found the fatal problem with the MnBi compound in 1973, we did not give up on the search for other viable MO storage media. We still believed that MO drives could fulfill the requirement for low cost storage for PARC's vision of a future computer system. Therefore, we continued the search for a new material to replace MnBi. We found that some past work on Manganese-Antimony (MnSb) looked interesting. We quickly made thin film samples of MnSb material and began to take measurements. As reported by others earlier, MnSb has an MO figure of merit that is less than one third of the one for MnBi. To investigate the reason for the low figure of merit, we grew single crystals of the MnSb compound to study the behavior of more perfect material. After careful studies of the magnetic and magneto-optic properties of the single crystal, we found that the magnetic anisotropy constant of the compound was almost one order of magnitude lower than that of MnBi. Therefore, the material could not completely support the magnetic moment that is perpendicular to the film plane even though the crystalline "C" axis of the hexagonal closed packed MnSb crystal is oriented normal to the film plane. Consequently the magneto-optic figure of merit would be low for this material. After these setbacks, we completely gave up working on crystalline MO materials except to spend some time to publish the work in academic journals. All this work took me to the middle of 1975. So after 3 years of hard work, both MnBi and MnSb were busts. I was no closer to having the storage material that PARC's vision called for based on MO recording technology.

The failure to find a viable media material did not discourage us from pursuing new MO memory materials. In 1975, I went back to review all the published works again to search for potential alternative MO media. Fortunately, we found that in early 1973, a group of researchers from IBM Thomas J. Watson Research Center published several papers reporting on a new discovery of a series of amorphous rare-earth-transition-metal (RE-TM) thin films having a high MO figure of merit (Reference 3-4). The amorphous thin films were reported to be useful not only to replace the Gadolinium-Garnet single crystal for bubble memory but they also had potential as MO media. The amorphous films not only had a strong magnetic anisotropy normal to the film plane, they also they had a high MO figure of merit. Once we learned about the potential of the material, we immediately decided to direct our research to the amorphous RE-TM material.

The amorphous RE-TM thin film media were fabricated by a sputtering process in a high vacuum chamber. The rare-earth element and the transition metal were sputtered simultaneously to create the amorphous alloy film. Since the process for making RE-TM films is quite different from the process for making MnBi films, we had to acquire a new set of equipment for our laboratory work. Unfortunately, there were no clear recipes on how to properly co-sputter the alloy film. Additionally, the rare-earth element is very easily oxidized, and at that time one could not buy any pre-alloyed sputtering targets. No one knew how to make them. Therefore, we had to learn everything from scratch. At first we acquired a very small sputtering system to study the composition dependence of the alloy and the vacuum conditions required to make a viable MO film. For the sputtering target, we used different amount of Tb and Co chips placed evenly on top of a solid Fe plate to change the composition of the resulting film. To study the effect of vacuum conditions on the film, we developed a fixture to remove the residual oxygen in the sputtering environment through a self-getter method created by the stray rare-earth atoms that were deposited away from the substrate during sputtering. With all these modifications of the sputtering system, we were able to study a series of Terbium-Iron (Tb-Fe) alloy and Terbium-Cobalt-Iron (Tb-Fe-Co) alloy compositions. We learned that the sputtered films had a microstructure consisting of nano-sized grains separated by oxide of the alloy. The oxide was composed of Tb and the residual oxygen in the system and it was segregated out of the grain into the grain boundaries during film formation. With formation of the isolated magnetic grains at the boundaries, it ensured that there would be no exchange coupling between the grains. Consequently the written bit had a smooth magnetic profile at the edges.

Furthermore, we learned that the magnetic moments of the Tb and Fe atoms were oriented opposite to each other in the film and acted as an antiferromagnet. Consequently, one could find a composition at which the moments of the two sub-lattice atomic species compensated each other completely. This composition is called the "compensation composition" of the alloy system. At this compensated composition, the alloy would not have any net external magnetic moment, and hence it would not have any demagnetizing field to cause jaggedness around the periphery of a written bit, which was one of the problems that killed the MnBi film. Consequently, the bit profile of the compensated composition of an amorphous TbFe-based alloy would be perfectly smooth and hence the read out signal would have minimal bit noise. On the other hand, even if the external moment was compensated, the internal moments of the ferromagnetic atoms still exist and they still would cause polarization rotation of a reading laser beam, therefore the laser could detect the state of magnetization of a recorded bit in a film for the data storage application. Now, armed with this knowledge, we were able to create a formula for a series of new alloy compositions that would generate the optimum MO effect in a medium. Also from that knowledge we were able to design a larger sputtering system for producing viable prototype disk media with sizes up to 14 inches in diameter for drive level evaluations and demonstration.

The first alloy composition we chose for the investigation was the Terbium-Iron-Cobalt (Tb-Fe-Co) alloy. Many 2x2 inch samples were made to study the MO response of the alloy films. The laser writing and MO response study of the samples were made by a static method on equipment having an X-Y translation station to move the sample. By comparing the results of the measurements on the amorphous Tb-Fe films with those of the MnBi films, it was quite encouraging to find out that in terms of repeated writing cycles, the amorphous media were much more stable than MnBi media and the figure of merit was about the same for both types of films. Furthermore, we verified that the profile of the laser written bit was extremely smooth around the periphery of the bit without any jaggedness.

After we installed the large prototype sputtering system in 1977, we decided to continue looking for a better combination of alloys to improve the MO figure of merit. We figured out that the addition of Cobalt in the Tb-Fe could increase the Curie temperature and hence increase the MO figure of merit; therefore we started to investigate this ternary alloy soon after we acquired the equipment. Also, in order to optimize the MO effect for the media, a proper optical interference structure on the media had to be developed. Basically, an anti-reflective coating to enhance the optical signal of the media was needed. The large prototype system was designed with several features so it would allow us to create many alternative multi- layer optical interference structures in situ. Consequently, in 1979 we were able to arrive at a formula for optimum alloy composition and interference structure to create many 14-inch diameter disks that had various optical interference structures, which were theoretically estimated to have the capability of storing more than 1 gigabyte of information. The substrate consisted of 14-inch tempered glass, which was originally purchased by ADL from Corning Glass for plated media development.

Unfortunately, the researchers in the OSL were not able to create a spin stand system to directly verify the recording performance of the disks that we had made. We felt that without the spin stand measurements, we couldn't progress any further on the project. We made the decision that except for some time that was spent on publications; we would put all MO activity on the back burner and turn our attention to hard disk magnetic media research. The hard disk media research had started in parallel with the MO media development in 1973.

The MO activity was re-ignited in the early summer of 1981, when Professor David Treves from the Weizmann Institute of Science in Israel came to visit PARC, on a sabbatical leave from the Institute to work on the MO program. David Treves was already a world-renowned research physicist and an expert on equipment and measurement, particularly in optical devices. He did research on many magnetic materials early in his career, and he was a pioneer in the field of magnetic reversal processes and magneto-optic memory in the 1960s at Ampex Corporation and at IBM. The announcement of his arrival re-ignited my enthusiasm on the MO project and we were waiting anxiously for his arrival. However, the very first meeting that I had with David Treves in my office ended badly. We got into a big argument about the viability of the MO memory, and he walked out of my office in a huff in less than 30 minutes of discussion. It was not an auspicious beginning.

The argument started because after I showed him the static MO figure of merit data and the impressively beautiful 14-inch diameter disk that we had made before, he told me dismissively that it would never work as an MO medium. I asked him how he could make such a statement without even measuring my disk and his answer was that his past experience on other MO media had shown high media noise and the signal to noise ratio was too low for it to be useful. That sort of generalization really made me quite angry because he really had no experience in measuring the type of TbFe alloy film that I made. I told him that the amorphous TbFe thin film had less noise than that of MO media made from a crystalline thin film such as MnBi or a Co-Pt alloy. Therefore, in principle, an amorphous alloy thin film does not have grain noise that can develop from the variation of individual particles of crystalline material. Also, by showing him microscope photos of many laser written bits in a static mode, I told him that the compensated composition of the amorphous TbFe film does not have a demagnetizing field to cause bit edge noise as he had experienced before on his films. However, he did not accept my reasoning. Therefore, I asked him rather impolitely at that point that if he didn't believe the MO media that we had developed had any chance to be useful, why he even bothered to come to PARC in the first place. With that, he got very angry and he stormed right out of my office. Obviously, we found out very quickly that both of us had quite a temper and strong opinions.

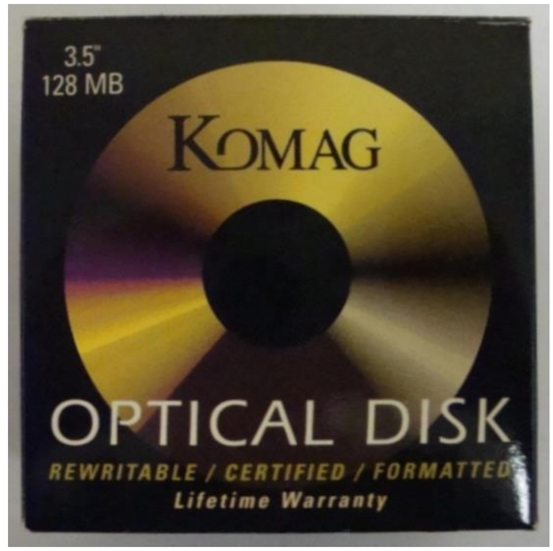
After the initial encounter with David Treves, I thought the spin stand project would be dead before it even got started, and I was preparing to give up on the project. However, few days later Dr. Robert Sprague the program manager from OSL came to my office to tell me that he was able to convince David to build a spin stand system for MO media. The first system would be designed for a 3-inch diameter disk rather than a 14-inch disk. Once we were convinced that David was going to build the spin stand system after all, we went ahead quickly to modify the sputtering system to produce 3-inch format MO disks for him to measure.

A couple of months later David was able to create the spin stand system and measure the MO performance of several 3-inch diameter disks, which were made with various formulations. Once he started to take the measurements, he became completely convinced that the media we produced had the potential to become a viable commercial product. With that experience, he became more enthusiastic than me on the MO project and he was willing to come back to PARC in 1982 and 1983 to help the OSL team design and build a 14-inch spin station to test the disks we had made in 1979. In early 1983, David came back to finish building the spin station and the OSL team tested some of the disks. The results demonstrated that the signal-to-noise ratio of the media was sufficiently high for the data storage application, and it was concluded that based on theoretical calculations, the 14-inch disk could hold more than 1 gigabyte of data. We believed this was the first demonstration of a multi-gigabyte capacity memory disk (of any type) ever produced in the world. We did not publish or promote this result at the time, and very few people knew about the excellent results that we had achieved on the MO technology. Thinking back now, I was so happy that many years of hard work on the MO project finally resulted in accomplishing a major technical feat, but it also netted a life-long friend in David Treves, even though we had a very rough start. We continued to have heated arguments about many things, but we remained very close. Later on, David joined me at Komag, the company I co-founded with a few other people in 1983 to make magnetic thin film media for a hard disk drive, which I will describe in much more detail later.

MO disk on 14-inch diameter glass substrate, made in 1979 and tested in 1983 at Xerox PARC. The media is theoretically capable of holding more than 1-gigabyte of data. My friends at Xerox PARC framed the disk as a farewell gift to me when I left Xerox PARC in 1983.
(Figure 3-1)

In 1987, after Komag went to IPO, I decided to ask David Treves to join Komag to develop a pilot production process for making commercially viable MO media. He joined Komag in 1988 and spent several years developing MO media using TbFe-based amorphous thin films. The task of developing a commercial MO product was much more difficult than developing magnetic thin film media for a hard disk drive, though, because a viable MO drive for data storage did not exist at that time. It was also technically more complex and difficult to make the media. It took several years for him to develop the media after the industrial standard for MO drive was generated. Finally, in 1994, Komag was able to commercialize the MO media and generated several million dollars of business. Unfortunately, the MO business never grew because the advent of low cost, write-once and rewritable CDs and DVDs made MO a much more expensive proposition. Here was a case where the work was technically superb and the product functioned in every way it was imagined, but the market acceptance was not there. The whole thing was a bust. The only company that had any level of success with MO was Sony. They had a version of Walkman that used a compact MO disk for audio recording. This disk also used the TbFeCo film for recording. This product was sold mainly in Japan, and never had much of a market acceptance anywhere else.

Komag's commercial MO disk developed by David Treves's team in 1990. A 3.5 inch cartridge capable of storing 128 Mbytes of data.
(Figure 3-2)



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Chapter 4

Low Cost Disk Memory (LCDM) Project at PARC

In 1973, when I discovered that the MnBi material was not good enough for MO storage media applications, I started to look for alternative MO materials but also decided to spend some time re-evaluating the material choices for hard disk media as well. While I was doing this re-evaluation work, a group of managers from the Advanced Development Laboratory (ADL) of PARC in Los Angeles (that was formerly a division of Xerox SDS) showed up at PARC in Palo Alto to discuss with us how to develop thin film magnetic recording media. This was to be used in a new hard disk drive they were developing as part of a low-cost computer that PARC had decided to create. This was the start of the Low Cost Disk Memory or LCDM project at PARC. Their idea was to re-engineer their existing commercial 24-inch diameter hard disk by replacing the old "head per track" technology with a brand new "Winchester-type" drive technology using a 12-inch diameter disk. The term "Winchester" was the code name for the IBM 3340 drive, which was introduced in 1973. The plan was to use three 12-inch plated Cobalt-Phosphorus (Co-P) thin film media and existing ferrite heads to provide 300 MB of storage. Following the conventional way of calculation, the budget for the storage was set for 15-20% of the price of the computer, which meant that the drive should be built for $2,000 or less. The reason for selecting a 12-inch diameter disk was to reduce the thickness of the disk by half, from 1/4-inch to 1/8-inch, so as to be able to use a smaller motor for the drive and still maintain good stiffness for the disk. This was part of the PARC office of the future strategy for providing a $10,000 computer system for each person. With a budget for storage at around $2,000, this price target was about hundred times lower compared to the IBM 3340 drive which had a list price of $87,600 for the 140 MB configuration – with four 35 MB 3348 Data Modules, each containing two disks, for a total of 8 disks in this configuration. So not only did the price of the drive have to be 100 times lower, it had to be done with about 6 times higher areal density compared to the IBM 3340 drive. Therefore, the project had very aggressive design goals and it required the GSL division at PARC to get involved.

The 300 MB storage capacity goal for the LCDM was considered quite large at the time. The reason for the large size was that PARC and SDS management anticipated that the new Alto computer that was in development at the time would not only store digitized text, but also serve as buffer storage for graphic images to a laser printer, which was also in development. Since digital images generate very large data files, it was deemed necessary to have 300 MB storage capacity. I thought at that time that the strategy of connecting the digital information to a laser printer was quite innovative and it was also a challenging subject for my research career at PARC. I was very excited to be part of this LCDM team and I accepted the responsibility of developing the media for the drive. Immediately after I joined the team, I started to do a literature search on the subject and learned the basic science and technology of hard disk drives from Dr. Gordon Hughes of the Advanced Development Laboratory of SDS/PARC. In the following section, I would like to go through some of the background information that I learned during that time and describe the hard disk drive "landscape" during that time period.

Some historical background of hard disk memory files.

The hard disk "drive files" that existed in the market in that period were either removable disk files, having a single disk cartridge such as an IBM 2310 or disk pack with multiple disks such as IBM 1311; or a "memory file" such as the 100 MB IBM 3330-1 "Merlin" file, which was introduced in 1971. Introduction of IBM 3340 drive in early 1973 is considered to be one of the most significant events in the history of hard disk drive development. This drive contains the basic design principle that all subsequent hard disk drives followed. The 3340 drive had a capacity of 35 MB or 70 MB depending on the Data Module, which contained either two or four 14-inch disks, with oxide coating for media. Data Modules (disk cartridges) were called the IBM 3348, and contained within them a head-arm assembly, carriage and spindle with bearing. Each surface had two heads that accessed the data. One of the key departures from the past was that it used a very small and lightly loaded head, which landed on the disk. Having a fixed disk and head assembly allowed much tighter tolerances to be maintained and led to significant improvement in areal density and performance. Use of a small ferrite head and smaller head-arm assembly and carriage reduced the cost so that these components could be part of the data cartridge or module.

The drives were not, strictly speaking, sealed as in today's hard disk drives, but the module had a lid which opened inside the disk drive to allow the carriage to connect to the actuators, and to also operate inside the controlled environment of the drive. Apparently the IBM team had considered and wanted a fixed drive without the removability feature with the data module, but there was considerable resistance to eliminating the removable disk pack by the sales and marketing team because previous generations were removable and it was considered an essential feature. A subsequent model, the IBM 3350, came out with a completely sealed and fixed drive, but the basic mechanism was the same as in the IBM 3340 design. Because the slider landed on the disk, the media for the first time was formulated with hard alumina particles inside an oxide coating so that it could withstand repeated landing of the head. It also used PTPF based (fluorinated polymer) lubricant on the disk for the first time. The term Winchester came about because the original design called for two 30

MB spindles and .30-30 was the popular cartridge size for the Winchester rifle. Winchester became synonymous with the disk drives that followed the IBM 3340 design concept.

View of IBM 3340 drive, with IBM 3348 Data Module pictured on top. System was announced together with IBM 370 model 115 processor in 2, 4 or 8 unit configuration, with 280 MB total capacity with 35 MB IBM 3348 Data Module (from IBM Archives). Courtesy of IBM Corporation © IBM

(Figure 4-1)

IBM 3348 Data Module for the IBM 3340 drive, containing either two or four 14-inch disks with capacities of 35 or 70 MB. The module had a spindle with bearings, head carriage, and a head-arm assembly with two low mass flying head per surface (Reference 4-1).
(Figure 4-2)

Image courtesy of Computer History Museum

Prior to the IBM 3340 drive, the head and carriage to move them on and off the disk surface were very large and bulky. The heads were loaded and unloaded off the disk. Such mechanisms added significantly to the cost, and they also increased data access times. The IBM 3340 drive with very low mass heads that eliminated the load-unload mechanism, was quite revolutionary and significantly improved data access speeds. Yet at the same time, much more burden got placed on the media by the fact that the head was landing on the disk. This single design feature had a very profound effect on the future design and requirements put on the media as we shall see.

IBM in the 1970s completely dominated the computing world and also the storage business. The technology they brought to market became the standard that everyone else usually had to follow. Many businesses depended heavily on the IBM “compatible” equipment and components market. Disk packs were a good example, where many companies produced disk packs to work in IBM disk drives. IBM was also the key technology driver as well and their 3340 model had a very big impact, even though in terms of total amount of sales it was not their most successful model. However, subsequent models, the 3350, 3370 and 3380 were huge moneymakers for IBM. It has been said that the IBM 3380 model was the most profitable product in IBM history. It is also said that only one other line of manufactured equipment was more profitable than the IBM 3380 drives, and that was the slot machine. The importance of the Winchester design, on which all of these models are based, was reiterated by Al Shugart, a founder of Seagate, when he stated in a 2000 interview that "the low mass lightly loaded head or, as some people call it, the Winchester head." was one of the four most significant events in the history of mass storage (Reference 4-2).

The four he listed in order were as follows:

1. Invention of the slider bearing;
2. The track-following servo;
3. The “low mass lightly loaded head or, as some people call it, the Winchester head”;
4. Magneto-resistive (MR) heads

Of course, Al Shugart did not mention media anywhere on his list and this omission is quite typical of many drive people throughout the industry. The critical part that media played in the Winchester drive concept and how this ultimately contributed to the miniaturization of the drive and dramatic increases in data capacity is often overlooked even by key figures such as Al Shugart. My intention is to hopefully correct this oversight in this book.

The advantage in disk drive performance and cost conferred by the Winchester design technology allowed IBM to migrate to a fixed disk file by 1976. By then, batch sequential processing was being replaced by online interactive data access, which called for faster access times to data. Fixed disk file systems allowed significant improvements in areal density by eliminating the tight tolerances that were required in having to match heads to different media and the mechanical variations that came with removability. This can be seen by the fact that from the IBM 3330 drive with removable disk pack to the IBM 3380 with fixed disks, the areal density went from 0.775 Mb/in^2 to 12.2 Mb/in^2. Of course, there was a tremendous improvement in head and servo systems which made this possible as well. Areal density progression of disk drives using oxide media is tabulated on the next page.

In 1973, at the start of the LCDM project, the IBM 3340 drive was the benchmark performance set point. It was, as stated previously, the start of a revolution in disk drive design that made it possible to get to where we are today. The history of the disk drive goes back much further, to 1956 when RAMAC 305 was first introduced by IBM. This long history is well-documented in many books and articles. The list of the industry "first" disk file in the last five decades, which was copied from a Disk/Trend Report (Reference 4-3), is presented partially in the table on the next page.

The history of magnetic recording, including considerable details on disk drive development at IBM, is well documented in an excellent book by Denis Mee in Magnetic Recording: The First 100 Years by IEEE Press (Reference 4-4). Somewhat more scholarly details of the history of magnetic storage were published in 1981 by L.D. Stevens (Reference 4-5) and by Harker et al (Reference 4-6), are also useful reading for getting the flavor of the state of affairs for magnetic recording during the 1970s.

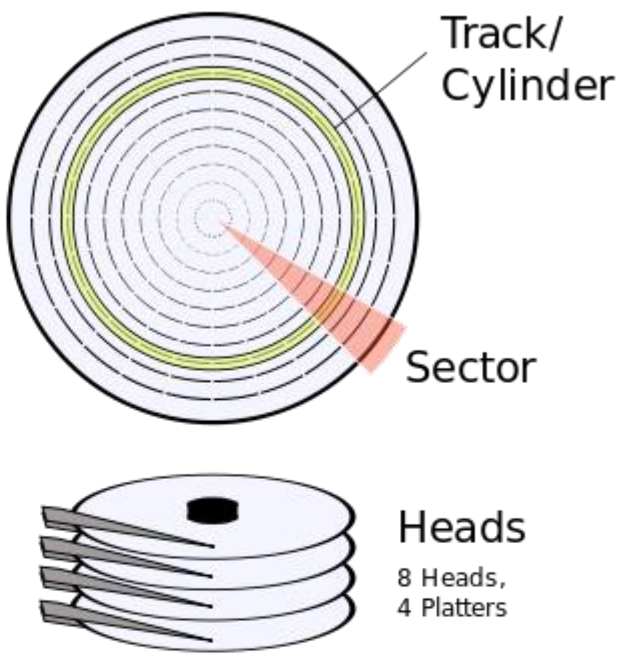
Drives	Type	Year	Areal Density	Track Density	Linear Density	Capacity	# Disks	Disk Size	Comments
			Mb/in^2	tracks/inch	bits/inch				
IBM 3330	Disk Pack	1971	0.78	192	4040	100	11	14 inch	
IBM 3340	fixed *	1973	1.69	300	5636	35/75	2 or 4	14 inch	
IBM 3350	fixed	1976	3.07	478	6425	317.5	8	14 inch	
IBM 3310	fixed	1979	3.8	450	8530	64.5	6	8 inch	First fixed 8 inch
IBM 3370	fixed	1979	7.7	635	12134	571	7	14 inch	8 turn TF head
Seagate ST506	fixed	1980	1.96	255	7690	5	2	5-1/4	
IBM 3380	fixed	1981	12.2	801	15240	1260	9	14 inch	8 turn TF head
IBM 3380K	fixed	1987	35.9	2089	17200	3781	9	14 inch	31 turn TF head, all digital servo
IBM 3390-2	fixed	1989	62.6	2242	27940	3784	9	10.8 inch	31 turn film heads

* 3340 Data module had fixed head inside the module

Year	Name	Capacity	Disk Size / Quantity	Comments
1956	IBM 350 RAMAC	5 MB	24 inch diameter/50 platters	
1961	Bryant Computer 4240	90 MB	39 inch disks / 24 platters	
1962	IBM 1301 Adv. Disk File	28 MB	24 inch disks / 25 or 50 disks	
1963	IBM 1311 Low Cost File	2.68 MB	14 inch disks	First removable Disk Pack
1965	IBM 2310 Ramkit	1.024 MB		single disk cartridge
1966	IBM 2314 Storage Facility	29.17 MB	11 disk removable pack	first ferrite core heads
1971	IBM 3330-1 Merlin	100 MB	11 disk removable pack	First track following servo system
1973	IBM 3340 Winchester	35/70 MB	3348 removable data module	low mass heads, lubricated disk, sealed assembly
1976	IBM 3350 Madrid	317.5 MB		Fixed disk
1979	IBM 3370 New File Project	571 MB	14 inch disks / 7 disks	first thin film head
1980	IBM 3380 Direct Access Storage Device	2.52 GB		

Basic working principle of a hard disk drive

The Winchester disk drive, first pioneered by IBM with the 3340 drive and subsequent fixed disk drives, 3350, 3370, and 3380 models, all used a 14-inch diameter aluminum disk coated with ferrite particles. Exceptions were the 3310 model introduced in 1979, which used a fixed 8-inch disk, and the 3390 model introduced in 1989, which used 10.8-inch diameter disks. Data capacity per surface of these media is simply a function of the number of tracks on the surface times number of bits per each track. Figures of merit for these two numbers are tracks per inch (TPI) and bits per inch (BPI) along the track, and areal density, which is (TPI) X (BPI) expressed in bits/in^2. Progression in TPI, BPI and areal density for many of the IBM drives are shown in the previous table. Schematically, the physical organization of the data on the disks is shown below, showing the tracks and various sectors of data that are organized on the surface. Sets of tracks that exist on top of each other on multiple disk surfaces are often called "cylinders" and data might be organized on these cylinders and accessed together.



Data organization in a disk drive (Figure 4-3)
From Wikipedia Commons

As mentioned earlier, the target figure of merit for the LCDM project was the IBM 3340 drive as a starting point, and the numbers are re-listed below for easier viewing.

	IBM 3340	LCDM Goal	Improvement
Linear Density	636 bits/inch	14,650 bits/inch	~ 2.6X
Track Density	300 tracks/inch	780 tracks/inch	~ 2.6X
Areal Density	1.69 Mb/in^2	11.3 Mb/in^2	6.7X

IBM 3340 Capacity/disk - 17.5 MB on 14-inch disk (2 sides)
LCDM Goal - 100MB on 12-inch disk (2 sides)

The LCDM project expected 300 MB on 3 platters (on a smaller 12-inch diameter disk), therefore one needed to get to 100 MB per disk. This is 5.7 times the capacity of the IBM 3340 oxide disk; therefore the areal density capability of the media had to scale accordingly. If the gain is to be shared equally from the TPI and BPI, then hypothetically each has to improve by $\sqrt{6.7}$ or about 2.6 times for both TPI and BPI as indicated in yellow in the table above. One does not necessarily have to split the share equally between the two, however. One might put more burdens on one or the other, depending upon the technology choices that one might have. In any case, at the start of 1973 when this choice was being looked at, obtaining 6.7 times more capacity from the existing technology was very difficult to conceive. A disk drive is a *system* and areal density increase can come from a number of areas. In terms of track density, much of this depended on being able to define smaller tracks by the head, and the ability to stay on track by more sophisticated track-following servo systems. Linear density depends much more on the ability of the media to support narrower transitions between the north and south poles of the bits. As shown schematically on the next page, in the so-called "longitudinal recording" mode that was being used at the time, the transitions between the recorded bits consisted of poles of the magnetized region that changed sign. The same inductive process that wrote the transitions was used to read back the data. The amount of signal that came back through the inductive head was a function of the size of the bit (transition), how sharp the transitions were, and the speed at which the transition (field) went by the head. Much depended on the head itself, as to how capable the head was in writing sharp transitions, and the signal-to-noise ratio of the overall system in being able to detect the signal.

While the drive and head contributed to the areal density increase, there were plenty of issues with the media as well. As linear density is increased, the transitions crowd into each other and the opposing field of the poles tends to cancel each other out, to demagnetize each other due to the field being created by the bits. This situation is illustrated below in Figure 4-4 on how the recorded transitions would look like for the direction of magnetization along the track.

Between the opposing magnetized regions, there will be demagnetizing field and it is inherent in the recording process. In order to pack more transitions into a smaller and smaller volume of material, one needs higher coercivity (expressed as Hc) in order to prevent demagnetization fields reducing the magnetization of the bits. As the bits or transitions are packed together more closely, the situation will become worse. Hc is very much of a materials parameter and strongly dependent on the choice of magnetic material one uses. In simple terms, much stronger magnetic material will have higher Hc.

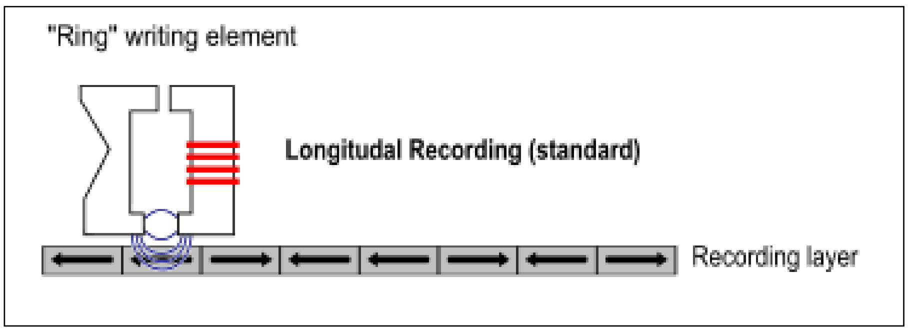


Figure 4-4 Longitudinal Recording

By a quirk of nature, the list of materials which are ferromagnetic, and therefore able to be magnetized, is rather short. Of the elemental materials of importance, we have only Fe, Co and Ni to really work with. Fe has been used in the form of oxides Fe_2O_3 (strictly speaking, a ferrimagnet), and various cobalt alloys are the main choice. The magnetic moment of the material is another important factor as it relates to the amount of signal that will be available. We start the discussion on Fe_2O_3 or ferrite. Before the acceptance of thin film magnetic media by the disk drive industry in the early 1980s, the magnetic layer for the storage medium was made with ferrite particles dispersed in an epoxy binder, which was coated over the aluminum substrate surface. The ferrite particles were acicular, meaning that they were roughly shaped like needles or rice-like grains. Because of this acicular shape, the ferrite particles had their magnetic north and south poles pointed along the long axis of the particle. The industry terminology for disks made with ferrite particles is “oxide media” or “oxide disk”. The process of making the oxide media consists of first mixing the ferrite particles with an epoxy binder material. As mentioned earlier, starting on IBM 3340 media, small amounts of submicron-sized alumina particles were added into the mix. These alumina particles are hard, and they served the purpose of making the disk surface harder and more durable. The mixture of ferrite, alumina and epoxy were ball milled to provide uniformly dispersed slurry, which was then coated on the surface of the polished aluminum surface by a spin coating method. The interesting story in IBM folklore is that they had been using alumina balls to ball mill the oxide particles all along, and they had thought that alumina debris that got into the oxide particles were causing defects. When they worked to reduce the alumina "contamination", the mechanical durability of the media became much worse, and it was realized that alumina particles were needed for durability. It became much more important in the Winchester design, and hence they started to deliberately add alumina, but in a much more controlled fashion. For the IBM 3340 drive product, IBM started to introduce the magnetic field during the spin coating and curing so as to orient the oxide particles. The field was applied to the disk with the strong field directed parallel to the circumferential direction of the disk. This tended to orient the acicular grains of the ferrite particles with their long axis along the circumferential direction of the disk. The purpose of this process was to create media with magnetic anisotropy (or directionality in the magnetic property of the media) such that the media would be easier to magnetize along the circumferential direction of the disk. Likewise, along the radial direction of the disk, the particles would be much more difficult to magnetize (Figure 4-5). This anisotropic magnetic feature provided some advantages in the magnetic recording process. More detailed discussion of "magnetic anisotropy" and how this characteristic is exploited in magnetic recording will be made in the following chapters. The spin-coated and magnetized disk is then baked to cure the epoxy. Following the curing process, the disk surface is "burnished" using an abrasive tape to smooth out the surface and remove excess material and asperities. "Burnishing" is simply another term for literally sandpapering the surface of the disk. However, the method was effective in providing very smooth and flat surfaces, which allowed a read/write head to fly closely over the surface of the disk. After the tape burnishing process, the disk was cleaned to remove all the loose particles and debris from burnishing.

From the very early days of disk drive technology, the recording head was designed to "fly" over the disk surface by the clever use of an aerodynamic air-bearing built into the underside of the recording head. The IBM 1301, introduced in 1961, was the first to use flying heads. Until the IBM 3340 drive, the heads used in the drive were very large and were unloaded off the disk when the disk stopped spinning. This involved complicated and bulky mechanisms to load and unload the heads and there were often many heads involved as there were multiple platters and surfaces in the drive. For the IBM 3330, drives used a load/unload mechanism with a ceramic slider, which was around 1cm on its side and had a load suspension force of 350 grams. The head flying height was 50 μ-inches (1.25 μm). With the IBM 3340, when the head was made much smaller using a single piece of ferrite or "monolithic" head, the size was reduced to 5.6 mm x 4 mm x 1.93mm (thick), and the load on the head was at 20 grams. There was also a dramatic reduction in the flying height to 18 μ-inches (0.46 μm). This was a major revolution in disk drive design.

PARTICULATE MEDIA 3.13

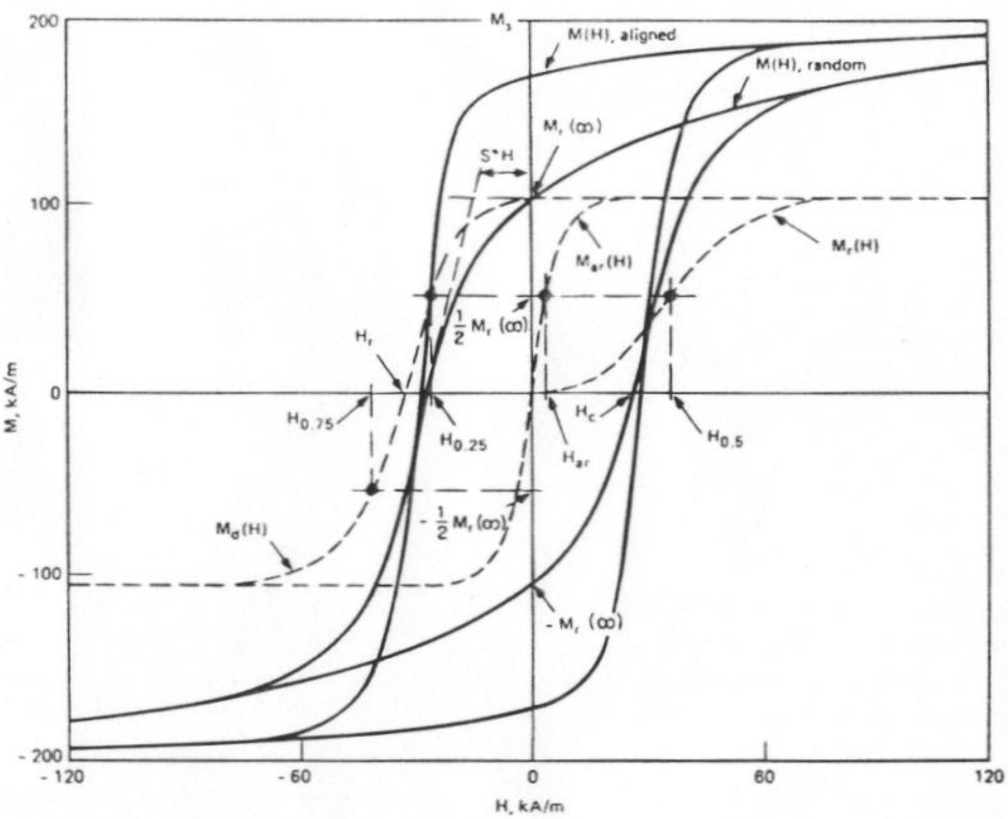
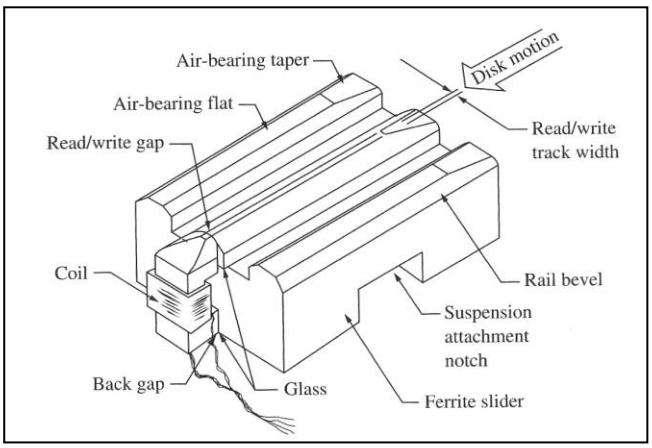
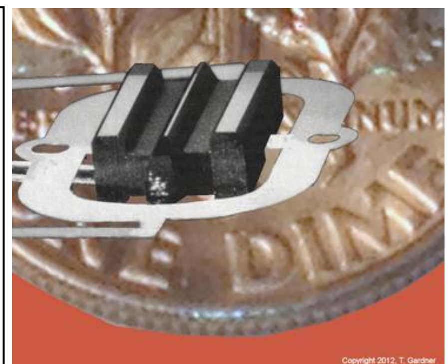


FIGURE 3.4 Magnetization $M(H)$, remanence $M_r(H)$, dc demagnetizing remanence $M_d(H)$, and anhysteretic remanence $M_{ar}(H)$ of a sample of randomly oriented γ-Fe_2O_3 particles. For the same particles, partially aligned, $M(H)$ is also included.

From Magnetic Recording Technology, 2nd Edition by C. Denis Mee & Eric D Daniel. Taller hysteresis loop is for the oriented oxide media, while the less square smaller loop is for random (isotropic) oxide without the magnetic field alignment during curing (Reference 4-7). (Figure 4-5)

IBM had some experience in components of Winchester technology when they developed a system earlier for a disk refresh buffer for a high-resolution display for the Rand Corporation using a 24-inch particulate disk. IBM's own head did not work very well apparently, and they licensed a new head design called a Tripad that was being used by Data Disk Corporation which also had a disk drive using a 24-inch plated disk for an instant replay video buffer. The Tripad head was a low mass slider that required only 20 grams load. Data Disk Corporation had other key innovations that I will speak about later. When Joe Ma at IBM tried the Tripad head on the oxide disk that he had, he experienced some wear on the media. He had been working on this program, and he decided to lubricate the disk using silicone oil. It is said that this was the first use of lubricant on the disk by IBM. Kenneth E. Haughton, who was in charge of the 3340 drive program, adapted this concept, except that the head he decided to use was a newly developed ferrite head based on the design that they had used previously. But the concept for making them very small and with a light suspension load was clearly from the Tripad design from Data Disk Corporation (story taken from Denis Mee's Magnetic Recording: The First 100 Years (Reference 4-4)). An illustration and photo of the ferrite head used in the IBM 3340 is shown below in Figure 4-6. The ferrite head provided excellent surface finish and it was quite hard and this allowed the head to be used for the Winchester drive. All subsequent hard disk drives can be traced to this innovation that came from the IBM 3340 drive.



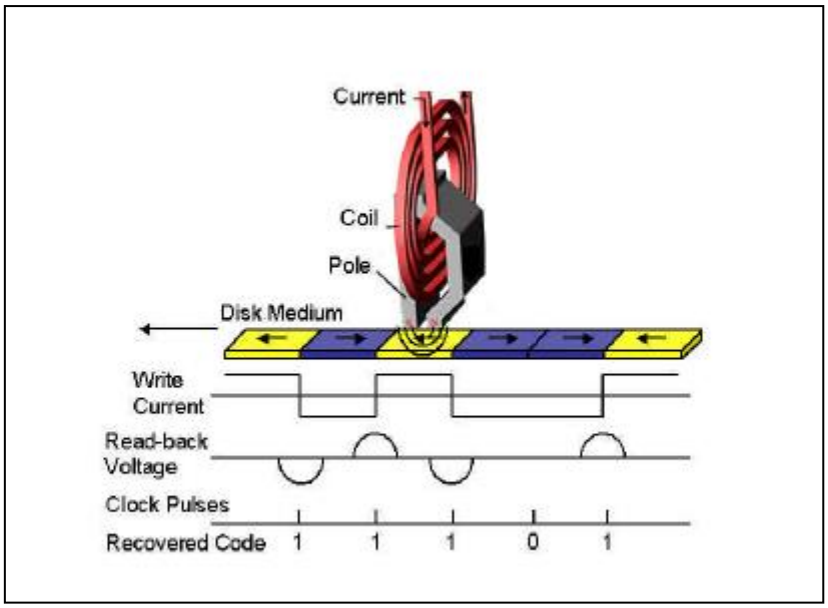


Schematic of ferrite slider used in IBM 3340 drive, and photo of the head. (Figure 4-6)

The reason for spending a considerable amount of time here on the IBM 3340 drive is that it changed the requirements for media in all future drives. On the one hand, the move to a very small slider with light load greatly simplified the drive design and made it much more inexpensive to manufacture. The opportunity to miniaturize the drive, and hence reduce cost further, can be traced to the Winchester design. However, it put very large new burdens on the media in that the head now had to come to landing on the disk, and also during start-up. Protecting the disk surface from wear and damage during this process became a central issue for development of the media. The on and off cycling of the drive with repeated landing and take-offs of the head is called "contact start stop" or CSS, and being able to pass the CSS requirement for the media and the drive often leads to a "make or break" situation for the drive design (and companies) for many years to come. The industry began to develop a whole new science of tribology around this central issue and development of a tribological solution to disk and head interfaces became as important as magnetic and recording physics in the development of media, heads and the drive itself. During my time at PARC, I had mainly concentrated on magnetic design and materials science around the magnetic properties of materials. I did pay some attention to tribology—the science of friction—for example, on an overcoat to use for corrosion protection and a reliability issue with the thin film media. But it was not at the same level of effort that I spent on magnetic development. When I founded Komag, it was actually the tribology performance of the media that had the biggest impact on success or failure of the business. IBM had its share of issues with tribology when they introduced the IBM 3380 drive in 1981. This drive had a wear issue, apparently related to the lubricant that was used on the media, and it took about a year to solve and this was after they had already announced the product. Solving this issue took a massive effort at IBM, and the delay in shipping the product cost IBM over $1 billion dollars in lost revenue. This issue had taken place just 2 years before I had started Komag in 1983, therefore the reliability issue associated with Winchester technology is something that everyone was keenly aware of and very sensitized to already.

In talking about magnetic recording, one cannot do this without bringing in the role of the magnetic recording head. Before the commercial introduction of magnetoresistive (MR) reading elements in the mid-1990s, all the recording heads for hard drives used a single inductive electromagnetic element for both writing and reading. The inductive element consisted of a magnetic core such as ferrite (as shown before), and a yoke for the electromagnet. A coil of conducting wire is then wound around the core. During the writing process, alternating current representing the data stream is applied to the inductive coil and the media is magnetized correspondingly by the magnetic field created at the gap of the magnet. As a result, the media under the head would produce alternating small-magnetized regions according to the input of the data information. The small alternatively magnetized regions behave like a row of small bar magnets that are connected together. However, instead of the normal attraction between two bar magnets having their north-poles connect to south-poles, the array of the magnetized region will have the same sign (that is south or north) of the magnetic pole between two neighboring magnetized regions facing each other. The resulting arrangement of having poles with the same sign will create a magnetic discontinuity between neighboring magnets. This junction is identified as a magnetic transition and it will generate a strong demagnetizing field at the transition. Depending on the sign of the pole at the junction, the direction of the flux would be radiating-out or converging at the junctions.

During the reading operation, the inductive head would sense the rapid change of the external magnetic flux as the transition region on the media passes rapidly below the gap of the horseshoe magnet as the disk is rotated rapidly. Consequently, voltage will be generated in the conducting coil by the magnetic flux. The polarity of the voltage is determined by the direction of the magnetic field at the junction. The output signals are the representation of digitized data that had been recorded, with some modification due to the recording and read-back process. With some clever signal processing to take into account of these modifications in the original signal, the read back information can be transformed back to a near faithful reproduction of the original data. Schematically, the arrangement is shown below in Figure 4-7.



Longitudinal magnetic recording signal reproduction. (Figure 4-7)

Each bit of information is related to each magnetic transition. Before the invention of advanced coding schemes in the late 1980s, each transition was encoded as a single bit. For a given encoding scheme, the maximum BPI and TPI that can be put onto a disk is determined by the physical properties of the medium and the head. These properties will now be briefly described.

As described earlier, a recording head consists of magnetic core with a gap at the end of a horseshoe magnet structure, and a conductive coil winding around the yoke of the core. The gap has a certain length and width or separation distance. In principle, the smaller the gap, the smaller the pulse width of the signal and this provides higher “resolution” between the signal and it can provide higher linear recording density. However, there is a limitation on how small a gap one can use. If a gap is too small, the magnetic field generated at the gap during writing would be confined too close to the gap. This makes writing the media more difficult, and it becomes sensitive to parameters such as flying height or separation between the gap and the media. Furthermore, before the introduction of the thin film head, gap length was limited to a certain minimum. In terms of the width of the gap, the wider the gap, the more the magnetic flux can be collected by the magnetic core from the medium, and hence a stronger signal can be generated in the coil. The stronger signal will result in higher signal-to-noise ratio. Unfortunately, a wider gap results in a smaller TPI due to a greater fringing field created at the edge of the gap. The other factors that affect the output signal are the physical dimensions of the core, the number of coils in the windings, and the permeability and maximum magnetization strength of the core material.

In the early 1970s, head cores were made with ferrite material, which has a relatively low permeability and low saturation magnetization. The head structures that used ferrite cores were called “ferrite heads” by the industry. Both manual and semi-manual processes were used to fabricate these ferrite heads. The wire windings were done manually by thousands of skilled workers, often in foreign factories. Due to such manufacturing processes, it was difficult to manufacture the core and to wind the coil. Therefore, it was hard to create a geometrically optimized head for improving the output signal efficiency of the head. In principle, one can increase the inductive signal level by increasing the number of coils. However, this increases the inductance of the head, and hence limits the frequency response of the head. Higher inductance degrades high frequency response of the head, and hence limits the data rate capability of a drive. Due to these constraints, the highest TPI achievable for the most advanced drive was around 300 TPI for the ferrite heads as used on the IBM 3340 drive.

As for the media, three key factors that affect the recording density are transition length, thickness of the magnetic layer, and the separation between the media and the recording head. Transition length, which is usually termed the “***a***” parameter by the magnetic recording physics community, is the distance or separation between two opposing magnetized regions in the media. The transition region occurs because of the demagnetizing field generated between two identical poles, which oppose each other. In the transition region, the magnetization direction must change from one direction to another by a 180-degree rotation. Therefore it has a certain physical distance, and it must remain stable in order to be useful for data storage. The value of "***a***" represents a space which is not available to store information; therefore it must be minimized in order to increase the linear recording density. There have been many theoretical works on the "***a***" parameter, in terms of factors that determine its value, and modeling as well as experimental determinations of it. These theories are quite involved and often complex. Discussion of it is beyond the scope of this book, but I would like to mention some of the physical parameters of the media that affect the "***a***" parameter. Based on the demagnetizing effect between the neighboring magnets, the length **"*a*"** can be related to the media parameters as follows:

$$a \cong \mathbf{Mr}^* \, t \, / \, \mathbf{Hc} \qquad (1)$$

Mr is the remanent magnetization, "***t***" is the media thickness and **Hc** is the coercivity of the magnetic medium. To explain it in another way, **Mr** is the strength of the magnet after the external field is removed, "***t***" is the physical thickness of the magnetic layer and **Hc** is the magnetic field strength needed to demagnetize or switch a magnetized medium from one state to the other. According to Equation (1), the transition length "***a***" would be reduced if the value of **Mr** and "***t***" were reduced, and for a higher value of **Hc**. Things are not quite so simple unfortunately, as one cannot just arbitrarily select the value of $\mathbf{M_r} * t$ and **Hc.** The reasons are that during the read back process, the signal strength from the magnetic induction is proportional to **Mr*t**. A larger value of **Mr*t** gives a stronger read-back signal, and a larger signal leads to an improved signal-to-noise ratio. Because of these two contradictory requirements in performance, compromises are made to optimize signal strength and the "***a***" parameter. For example, for designing a medium with optimum **Mr*t** and **Hc** values, a magnetic material having the highest **Mr** and simultaneously having highest **Hc** is the most desirable if the medium can be made as thin as possible. In general, reduction of the **Mr*t** product is achieved by reducing the thickness "***t***" while maintaining **Mr** to the maximum value possible for the media. In the early 1970s, the state-of-the-art material was "oxide media" with ferrite particles. Oxide media has a relatively low value of **Mr**, but it was still possible to lower the media thickness "***t***" sufficiently to allow a large enough **Mr*t** product for signal generation. In other words, **Mr** was not the limiting factor but the thickness reduction was the constraint in making media that works at a higher recording density. Spin coating and burnishing the media using sandpaper would impose a severe limit on how thin the oxide media could be made too. For example, the media thickness for IBM 3340 to 3370 drives remained flat at 41 µ-inches (1.0 µm) and it took about 8 years to finally go to 0.5 µm thickness in the IBM 3380 drive. For **Hc**, the oxide media has an upper limit as well. In the early 1970s, the maximum value of **Hc** for oxide media used commercially was 300 Oersteds or less. The value did not improve above 400 Oersteds until the 1980s. I will discuss later how oxide media had other basic limitations in achieving higher recording density and that it could not hope to provide a solution path for the ~ 6.7 X improvements that was needed for the LCDM recording density goal.

Finally, I would like to elaborate on the important relationship between the head-to-disk spacing and the recording density. During the writing process, if the head is flying too high, it will have difficulty in writing data to the media. If the coercivity is also high, then the writing will be even more difficult. As described earlier, the gap of the magnetic core provides the magnetic field to magnetize the medium. However, if the distance "***d***" between the gap to the medium is too far away, then the strongest field generated at the vicinity of the gap cannot reach deep enough into the medium to magnetize it during the writing process. The external magnetic field strength in the vicinity of the gap will be strongest at the gap and it will drop away rapidly away from the gap. The separation distance between the magnetic gap and the media is of course a function of the flying height of the head. Therefore, in order to use a high **Hc** medium to increase the recording density, one must bring the head as close as possible to the media.

For the reading process, the higher the flying height, the less "resolution" one would have in being able to see the magnetic transitions in the media. This is because of the fact that the magnetic field lines that radiate out of the magnetic transitions are the densest at the immediate surface of the media. The farther apart you are from the surface, the less field there is to detect. This is similar to a bar magnet, which has the strongest and concentrated field lines at the pole and the field lines will be rapidly dispersed away from the pole. Therefore if the head is flying too high, the gap of the core will be too far above the medium, and hence the pulse signal generated from the coil will be broader. Wider pulse width means less "resolution" in the signal and it results in having to reduce the recording density in order to get more "resolution" in the signal.

In summary, the signal-to-noise ratio of the data signal that is recovered is related to the resolution of the string of data output from the reading sensor on the head. The resolution in turn is related to the pulse width of the sensor output. The smaller the pulse width or the sharper the pulse, the higher the resolution would be for the bit stream. The sharpness of the pulse width that can be achieved in a system in turn is related to all the physical parameters in the head, media and flying height, on which I have elaborated earlier. The effects of all these parameters to the pulse width had been theoretically analyzed for various types of head and media combinations, and, it can be expressed in the following relationship:

$$PW_{50} \cong \sqrt{g^2 + \frac{4}{3}(a^2 + d^2 + t^2)^{\frac{1}{2}}} \qquad (2)$$

Where PW_{50} represents the width of the pulse at half maximum and $\boldsymbol{g}$ is the gap length, $\boldsymbol{a}$ is the transition length, $\boldsymbol{d}$ is the head gap to media surface distance and $\boldsymbol{t}$ is the media thickness. As one can see from Equation (2), PW_{50} is narrowed by reducing all the parameters in the equation.

In the early 1970s, the areal density of the most advanced drives such as the IBM 3330 and 3340 models, which were introduced in 1971 and 1973 respectively, had recording densities of 0.78 and 1.69 Mb/in^2 respectively, as was shown in the previous table in this chapter. For the IBM 3340 the linear density was 5,636 BPI and the track density was 300 TPI. The physical dimensions of the head and media used in these drives were extremely large by today's standards. The values for the physical parameters, $\boldsymbol{d}$, $\boldsymbol{g}$, and $\boldsymbol{t}$, from the previous equations in units of μ-inches for these drives were $\boldsymbol{d} > 20$, $\boldsymbol{g} > 60$ and $\boldsymbol{t} > 40$. Also the coercivity (Hc), which affects the transition length $\boldsymbol{a}$ in an inverse manner, was only about 300 Oe. Based on the technology known at that time, it was very difficult to imagine how the above parameters could be improved so that recording density could be increased from 1 Mbit/in^2 to more than ~ 10 Mbit/in^2 for the LCDM project goal. By simple analysis of the components used in the drive, it was easy to realize that the biggest problem lay in the oxide media itself. Of course, the head technology also constrained the density improvement, but this constraint could be overcome by improving the manufacturing process to reduce the gap length and thereby increasing the areal density. On the other hand, use of oxide media caused high flying-height, large media thickness and low Hc, all of which were very difficult to change. The ferrite particles in the oxide media has an upper limit for the Hc, and the media produced from the ferrite slurry could not be made thin and smooth enough for 10 Mbit/in^2 areal density and beyond by the manufacturing methods known at that time. Furthermore, aluminum oxide particles that were introduced into the media as a by-product of the milling process for ferrite particles, or often added additionally into the slurry to improve the mechanical wear resistance of the media, prevented the flying height from being lowered beyond certain limits. The diameters of the alumina particles were generally larger than 10 μ–inches (0.25 μm) and they would stick out of the media surface in the finished product. Lowering the flying height too much would cause a catastrophic head crash if the head started to encroach on the height of the alumina particles. These constraints were well-known in the disk drive industry at the time, therefore alternative media technology using metallic thin film media to replace the oxide media were already being proposed as early as the 1960s.

In the 1960s, there were many research and development efforts on hard disk drives using plated thin film media, and several made it to market. The media used plated cobalt phosphorus (Co-P) alloy magnetic film. I will cite several examples. In the 1966 Winter Olympic Games in Montreal, Ampex Corporation introduced its now famous instant video replay device, which was based on a disk drive using plated thin film media. Also in 1966, National Cash Register Corporation (NCR) introduced a disk drive, which used a removable disk pack using plated media. By the early 1970s, SDS had already introduced drives using 24-inch diameter plated media for their computer systems. I also mentioned that the IBM 3340 drive design was based on heads used in Data Disk Corporation's drive using 24-inch plated media and using heads with many read/write elements (a scheme called "head per track"). However, these products using thin film media were never fully accepted by the industry, and generally existed in a smaller niche market. The long-term reliability concern about thin film media, particularly for corrosion and head and media wear due to tribology problems prevented more widespread use and acceptance. Oxide media used already oxidized media so that it would not be expected to corrode further under normal use.

In principle, it was clearly possible to push the recording density well beyond the 100 Mbit/in^2 range using metallic thin film based on what we knew at the time. However, in order to consider thin film media, one had to solve the reliability problems. Additionally, demonstration of the extendibility of thin film media technology was also important, as this demonstration was needed to convince the market of the future viability of the technology. One such demonstration would be to show that media Hc could be extended well above 400 Oe to 1,000 Oe or more, which would be needed to increase the areal density. If one could introduce a media with this capability, the industry might be more willing to switch to thin film media technology and improve the performance of the drive. In order to create such media, and also deal with many reliability concerns, it required commitment to a long-term research effort to search for a new metallic alloy system and development of practical film formation or deposition processes. We clearly did not have such knowledge at that time. In the early 1970s, there were no publications that indicated the existence of such media. There were some publications from IBM in the 1960s which showed coercivity as high as 800 Oe could be achieved in plated media by addition of nickel or zinc, but reliability concerns or performance were not addressed (Reference 4-8). Furthermore, there was relatively little interest in academia or in industrial laboratories for hard disk media research. Hard disk drive technology had already been around for more than a decade, and it was not an attractive subject matter for most researchers in the world.

Hard disk magnetic media research at PARC

Based on the published literature and on the basic principle of magnetic recording, we were able to quickly come to a conclusion that the LCDM goal could be met if thin film magnetic media was used instead of oxide media. Simply based on Equations (1) and (2), it is possible to reduce the thickness, "***t***," of the media by factor of 10 or more from oxide media by using metallic thin film media with the same amount of magnetic moment or magnetic strength. First, it is much easier to make a very thin media film of high quality by depositing the film atom by atom in a vacuum deposition system or by a chemical plating process compared to the oxide media manufacturing process. Secondly, by using a smoothly polished substrate, one can maintain this smoothness by vacuum deposition or plating, allowing the head to fly much closer to the media compared to oxide media. The value of "***d***" in Equation (2) would be much smaller. Thirdly, as it was reported in literature, it should be much easier to obtain high coercivity in a metallic film by alloying than for an oxide media. Bulk magnets made with cobalt-platinum alloy were already available which had a coercivity of well over 1,000 Oersteds.

Based on a literature search and calculations, we estimated that the storage density should be easily increased by a factor of 10 by using a metallic thin film over the oxide media. Achieving high areal density has an immediate and very significant impact on the cost of the drive. By adopting metallic thin film media, one can reduce the size and number of disks needed in a drive for a given storage capacity. Hence, a smaller motor can be used and the drive will be smaller and its power supply will be smaller as well. Heat dissipation will also be easier to manage. Consequently the drive will be much simpler and less costly. So with this idea in mind, we quickly formed a team to develop thin film media. In hindsight, it turned out that building a prototype drive and demonstration of concept was a lot easier than making a commercially viable drive and components.

During the period when the LCDM project was started, SDS already had considerable experience in making the 24-inch plated media used in their commercial drives. This media had a coercivity of 300 Oe. New media for LCDM requirements needed a coercivity of over 500 Oe, and SDS could not figure out how to do this by their plating method. In principle, it is possible to obtain a plated Co-P film with an Hc of 500 Oe or more by the electroless plating process, and there were publications that indicated that was possible. However, there was no commercially available plating solution that was capable of creating 500 Oe media. As a matter of fact, even though many publications claimed high coercivity, there was no clear understanding of the relationship between coercivity and plating chemistry and the plating process. I have been talking so far about coercivity but this is not the only parameter of importance for the media. Another very important parameter was the magnetic hysteresis loop squareness. Substrate characteristics were also very important. The surface must be very smooth to allow the head to fly low, and at the same time the surface must be hard so that it can withstand occasional impact by the head. In order to make plated media viable, even the substrate had to be very different than those that were being used by oxide media.

In the case of oxide hard disk media, an aluminum disk substrate was used. The surface of the aluminum was either polished or precisely machined to a high degree of precision for flatness and smoothness. The surface was then chemically cleaned before the oxide slurry was applied. Typically by this method the substrate still had many micro-defects, such as pits and mechanical scratches. For an oxide media application, such micro-defects did not pose much of a problem because the thick layer of oxide slurry coating would eventually fill-in the defects and level them out. As for surface hardness, oxide coating was quite thick, and the surface characteristics were dominated by the coating itself. Addition of hard alumina particles to the coating provided some additional hardness to the media surface.

For thin film media, however, the thickness of the magnetic layer is one or two orders of magnitude thinner than the oxide media. Also the film is deposited slowly, literally atom by atom onto the substrate surface, and the film tended to conform around the topology of the substrate surface. In another word, defects such as pits, scratches and asperities tended to show right thru the media. Consequently the surface and substrate requirement for the thin film media was much more demanding than for oxide media. Furthermore, the thickness of the magnetic layer was only a few µ-inches thick. There was not enough film material to help support the occasional impact by the head during operation. Because of these issues, substrates used for the thin film application needed to be plated with a thick, non-magnetic hard coating of nickel-phosphorus (NiP) film which was at least 400 µ-inches (10 µm) or more to fill in the defects on the aluminum surface, and at the same time provide the hard surface needed for the occasional head impact. Once plated by NiP coating, the surface had to be polished to make the surface flat and smooth again so as to allow the recording head to fly low as possible.

Looking back now, the infrastructure necessary for making thin film media was not very well-developed then. The right sort of chemical solutions for electroless plating of nonmagnetic nickel-phosphorus (Ni-P) coatings for aluminum disk substrates were not available commercially. Those that were available were pitifully inadequate for hard disk applications. Chemical solutions were made for things such automobile parts for example. Therefore we had to develop chemical solutions suitable for a hard disk application from scratch by ourselves. The lack of availability of commercial chemical plating solutions for the magnetic film and Ni-P coating for the hard disk application was the result of the fact that few people believed that the plated hard disk media would become a viable technology. Consequently, no chemical company was willing to invest in developing an advanced Co-P and Ni-P chemical plating solution specifically for use in hard disk applications. It is interesting to mention here that once thin film media became commercially accepted in the early 1980s, many companies jumped into the business and started to develop chemical solutions for plating Ni-P and Co-P films.

In 1973, when I started to become involved in developing the plated thin film media with SDS, the process for making the substrate was similar to the process for making the 24-inch disk for oxide media. The aluminum substrates that we used were identical to those used in oxide media. The aluminum alloy compositions for these disks were made specifically for the hard disk application. The alloying element and the processing of the metal provided the right sort of mechanical strength needed for the high rotational speeds used during drive operation. Unfortunately, the aluminum alloy contained certain impurity elements, which would cause etched pits on the surface during the chemical cleaning step that was necessary before plating. If the pits were too deep, it could be difficult to fill-in by Ni-P plating. Consequently, a separate leveling layer had to be applied on the aluminum surface before Ni-P plating to get rid of the pits.

The process we used in SDS for preparing the aluminum substrate consisted of first cleaning and anodizing the polished aluminum substrate, followed by electroless plating of a copper leveling film, then plating with a thick layer of electroless Ni-P film. Compared to the process used after the 1980s, this approach was quite cumbersome. However, we were able to obtain a defect-free surface that was also hard. Precision polishing processes could be applied to the thick NiP coating to produce a useful substrate. By 1975, we had improved the Co-P plating solution so that we were able to consistently produce disks having relatively low noise, high magnetic hysteresis loop squareness and a coercivity of over 450 Oe. With these disks, and the progress made in the design of the 12-inch disk drive, we were able to produce several 300 MB capacity drives with 3 platters to demonstrate the feasibility of the LCDM concept. I was told that by 1979, there were some 30 drives that were made, and they were incorporated into various computer and laser printer systems to verify the performance and reliability of the LCDM drive.

The successful demonstration of the LCDM concept prototype drives made our team quite excited and everyone involved in the project hoped that upper management would make a decision to create a business out of it. Unfortunately in late 1979, IBM announced the introduction of the model 3370 drive to cover the lower cost segment of the market. The drive used seven 14-inch diameter oxide disks and a recording head based on a new thin film head technology, providing a total of 581 MB storage capacity. The IBM 3370 drive was also the first to use a thin film head instead of a conventional ferrite head, and this change allowed a large increase in areal density. In terms of storage capacity per disk, the IBM 3370 drive had 81 MB per 14-inch disk versus 100 MB per 12-inch diameter disk for our LCDM drive. Also in that same announcement, IBM gave the reason for its adopting proven oxide media technology rather than thin film media technology. They claimed that they did not believe that thin film media could provide the sufficient reliability performance, namely corrosion resistance and mechanical reliability, needed in a file system. They also reasoned that by adopting the new thin film head technology to replace the ferrite core head, they could continue to push the areal density further for some time.

The announcement of the new IBM 3370 drive immediately caused a big stir at Xerox PARC and at the Xerox subsidiary of Century Data Systems and its Diablo division. Century Data Systems and Diablo were two companies which Xerox had acquired earlier to help get them into the data storage business. A few days after the IBM announcement, Xerox management and its subsidiaries decided that Xerox should not pursue the LCDM as a commercial product. They reasoned that IBM is a technology pacesetter for data storage products; therefore no one would be able to sell a storage product which used technology drastically different from the IBM standard. This decision was a big disappointment for me and to the whole group of engineers who worked on the LCDM project. We thought as engineers that IBM's arguments for rejecting thin film media technology were flawed. The case in point was that the thin film head that they used in the IBM 3370 drive used a plated nickel-iron (Ni-Fe) film, which in principle would not be any better in corrosion resistance than Co-P film in the media. As a matter of fact, we had proven that in a sealed drive the corrosion resistance for the Co-P thin film media was not a reliability issue based on our extensive evaluation of the drive.

In addressing IBM's concern about the possible mechanical wear issue with thin film media, we also proved that to be not an issue as we had figured out how to lubricate the disk with a fluorocarbon lubricant. In any case, we believed that the decision not to adopt the thin film media by IBM and consequently by Xerox was strictly based on business factors and not on technical merits. We believed the corrosion and wear problem could be overcome by any number of methods, such as applying a thin protective overcoat and the use of a proper lubricant. However, IBM's opinion, and the products that they decided to put onto the market were so strong that most companies avoided doing anything that went against it. Although we had confidence about thin film media, it did have issues, which could have been very problematic or maybe even fatal. First, I found out that the as-plated media had a lot of surface asperities and required an extensive amount of mechanical burnishing after the fluorocarbon lubricant was applied for the head to fly properly. Secondly, I found out that it was quite difficult to increase the coercivity of the media and also to reduce the media noise. These two problems were quite a concern to us and we made a big effort to try to overcome them.

For solving the surface asperities problem, we made a careful analysis of the media surface and found that the asperities occurred as the result of an abnormal growth of Co-P grains on mechanically induced defect sites, which were caused by the polishing process on the plated Ni-P. Therefore, it was concluded that without rigorous mechanical burnishing of the media surface, the head could not fly stably. Unfortunately, we could not use the conventional sandpaper burnishing process to burnish the media surface, which was typically employed in finishing the oxide media. This was because the Co-P film appeared to be too soft or too delicate if we used a conventional method. In addition, it was difficult to remove any excess material by burnishing in a controllable manner because the film was very thin. To overcome this problem, we employed a specially designed ceramic burnishing head and loaded the head on the disk surface with a large applied load to force intimate contact between the head and the media surface to attempt to grind down the protruding asperities. To monitor the effectiveness of the burnishing process, we attached a piezoelectric transducer to the suspension, and connected the output to a speaker so that we could hear the progress of burnishing. This was an interesting process in that we could hear the asperities being burnished as the sound from the speaker gradually diminished. This process was effective from the standpoint of producing experimental media but the process was hardly practical, and probably not manufacturable. We also created magnetic defects and the process was cumbersome and costly. When the plated media started to be marketed in early 1980s, I saw that companies were actually resorting to the same sorts of methods to deal with plated defects.

Raising the coercivity and improving the signal-to-noise ratio of the plated media were much more complicated. Hc and media noise are structure sensitive properties. Hence, they can only be improved by changing the microstructure and crystal structure of the plated Co-P film through changes to the plating chemistries and the process. There were several extensive efforts reported in the 1960s and early 1970s to improve the Hc of plated media. Work by two groups comes to mind in this effort, one was by R.D. Fisher and W.H Chilton at National Cash Register (Reference 4-9), and the second by D.E. Speliotis, J.R. Morrison and J.S. Judge at IBM (Reference 4-10). Their results suggested that it would be difficult to raise the Hc of plated Co-P film beyond 600 Oe using the Co-P composition. By adding a third element such as zinc or nickel salts in the Co-P plating solution, they saw that Hc could be raised to slightly more than 1,000 Oe. After reviewing these prior publications and conducting my own experiments and an analytical characterization of the plated CoP films, we were able to construct a fundamental model of the relationship between coercivity and media noise to the material characteristics of the film. I hypothesized that in order to increase Hc; the magnetic moment of the media had to be reduced drastically from that of Co-P alloy. Furthermore, the Co crystal had to maintain the hexagonal-closed-phase (HCP) structure, and the grains had to be sufficiently separated from each other to prevent the inter-crystalline exchange interaction. Once I established this basic model, I had to decide whether it was worthwhile to continue pursuing this path with a plating process, or to embark on a new course, which was to sputter the film in a vacuum. I chose the latter.

The Development of Sputtered Thin Film Media.

By 1976, I had decided to switch my attention to the vacuum sputtering process. By using a sputtering process, I would have greater freedom in choosing the Co alloys that looked interesting. One could not do this so easily in the plating process because one would have to develop chemicals and a plating process for each alloying element that one might consider, and this was an added complication that greatly added to the time to develop anything new in plating. After I made this decision, I made a proposal to the management of the Advanced Development Laboratory (ADL) and got approval and funding of $25,000 to start the project. Even in those days, this was not very much money. However, by modifying the old vacuum deposition system that was used for making MnBi films for the MO project, I was able to create a sputtering system suitable for magnetic media development in a few months, even with such a small amount of funding.

The design of a proper sputtering system for creating thin film media depends on the choice of alloys, film structure, and the type of sputter process that one chooses. Unfortunately, literature on creating magnetic thin film media by sputtering was quite limited before the mid-1970s. Only a few publications were available as prior art that I could use to plot the course for our new research. Against this background, I decided to use an RF sputtering process to create our new cobalt alloy films. The sputter system was capable of making only one layer but this was sufficient for my purposes. I thought that RF sputtering provided the best chance of selecting a wide variety of alloy systems and creating a high quality, homogeneous and uniform film. I also believed that this path provided a commercially viable production process for creating the films. The few years of experience that I had in RF sputtering at IBM and Northrop provided me with the knowledge needed to start on this path.

Once I had modified the vacuum system to run RF sputtering, my first attempt was to make pure Co film with various sputtering parameters at different substrate temperatures. I quickly concluded that it would be impossible to obtain high Hc film from a pure Co film. The highest Hc that I got was barely over 100 Oe. This was too low to be of any use. However, we made careful analysis of the microstructure of the Co films to try to understand what was happening and concluded that the low Hc of the sputtered pure Co film comes principally from the formation of Face-Centered-Cubic (FCC) phase grains having a high density of stacking faults, and also due to the grains of the film being all closely interconnected. These features in the microstructure of the film would indicate that the film had a low magnetic anisotropy constant and a high exchange interaction between the crystallites. After this, I tried to copy the work by Lazzari et al for Co film on Cr (Reference 4-11), which is to grow epitaxial cobalt film on top of chromium film. This path also failed as I was not able to obtain a high coercivity film as Lazzari et al had claimed. I believed that the reason for my lack of success was due to the fact that I did not have sufficiently well enough vacuum in my sputter system to obtain the epitaxial film. It is a known fact that Cr is quite easy to oxidize, and unless you have very good vacuum, the Cr film will quickly oxidize and prevent good epitaxy by the Co film. After this discovery, I reasoned that I must alloy the pure Co with another metallic element or elements to achieve high Hc film with good microstructure and magnetic properties.

I went to examine the binary phase diagrams of various cobalt alloys to choose an alloy system that looked promising. Co-Pt (platinum) and Co-Re (rhenium) alloy systems stood out as good candidate systems to study. The phase diagrams indicated that these alloys have a higher transition temperature from hexagonal close packed (HCP) to face centered cubic (FCC) crystalline phase and hence the sputtered film would have a better chance to retain the HCP phase. I decided to select Co-Re over Co-Pt systems for my research work, primarily due to the high cost of platinum. Rhenium was much cheaper, but at the time I did not know much about availability of Rhenium metal. It is as rare as platinum but rhenium did not have as much market demand as platinum and hence it was much cheaper. In terms of magnetic properties however, the Co-Pt system has a better chance of producing higher Hc films because of the high spin orbital moment associated with the platinum atoms that provides a higher magnetic anisotropy and hence higher Hc than rhenium atoms. I thought that if I could figure out how to make high Hc Co-Re film, it would be trivial to switch to a Co-Pt alloy if needed in order to obtain higher Hc.

By the end of 1977, we were able to produce a series of single layer Co-Re films having an Hc above 600 Oe - and even as high as 800 Oe - in the simple one-gun sputtering system that I had been using. I chose to use a relatively high argon sputter pressure of more than 50 mTorr because I got better results. Additionally, I was obtaining good sputtering results at room temperature. When the film was deposited on a substrate heated to above 100 ^{o}C, Hc was generally lower and I noted that the squareness of the magnetic hysteresis loop was higher than that of the room temperature samples. Once I had determined that high Hc media can be produced in a simple sputtering process, we proceeded to make 8-inch diameter disks with a series of Co-Re alloy films on the conventional Ni-P plated aluminum substrate for recording performance measurements. I also made many sister series of films on carbon coated glass slides for microstructure analysis by a transmission electron microscope (TEM), which we had at PARC. Carbon coating on the glass slide was for the purpose of easily lifting the film off the glass for TEM analysis. I made certain that I was getting the same magnetic properties on the glass slides as I was obtaining on the 8-inch disks. These film series were also examined by a technique called Lorentz Electron Microscopy in the TEM so that I could try to make sense of the magnetic domain patterns in the film and determine whether I could begin to address the question of basic physical limits to magnetic recording. Lorenz microscopy allowed imaging of recorded bits on the media, and I could compare this image against the image of the microstructure of the film obtained by conventional TEM imaging. I used the simple drag testing setup that was used in previous magnetic film studies to record tracks on the films deposited on the carbon coated glass slides.

TEM analysis of the films reaffirmed our earlier hypotheses that were obtained from the microstructural studies of plated Co-P films. This was that in order to obtain a high Hc in the film, the crystal structure of the Co-alloy grains must have a high HCP crystalline phase and the grains must be physically separated from each other to prevent exchange interactions between the grains. Furthermore, recorded tracks of low coercivity films which have high inter-particle exchange interaction generated characteristic extended magnetic ripple structures in the Lorentz images. I discovered that in these films, the recorded transitions had a jagged and high amplitude saw tooth-like structure between the recorded bits. Additionally at the saw tooth region, the magnetic ripples formed into a characteristic "vortex" pattern. For the high Hc films that had low inter-particle magnetic exchange interactions between the grains (by virtue of higher separation between the grains), in the magnetic ripple pattern, the transition was much more smooth and narrow. From the actual disks made using different processes, we knew that the signal-to-noise ratio of the media was much better for films with exchange decoupled grain structure. Those with higher Hc and narrower and smoother transitions had an improved signal-to-noise ratio. These were very key findings. This body of work provided a much better understanding of the importance of inter-particle exchange interactions and the role that they play in media noise performance in thin film magnetic media.

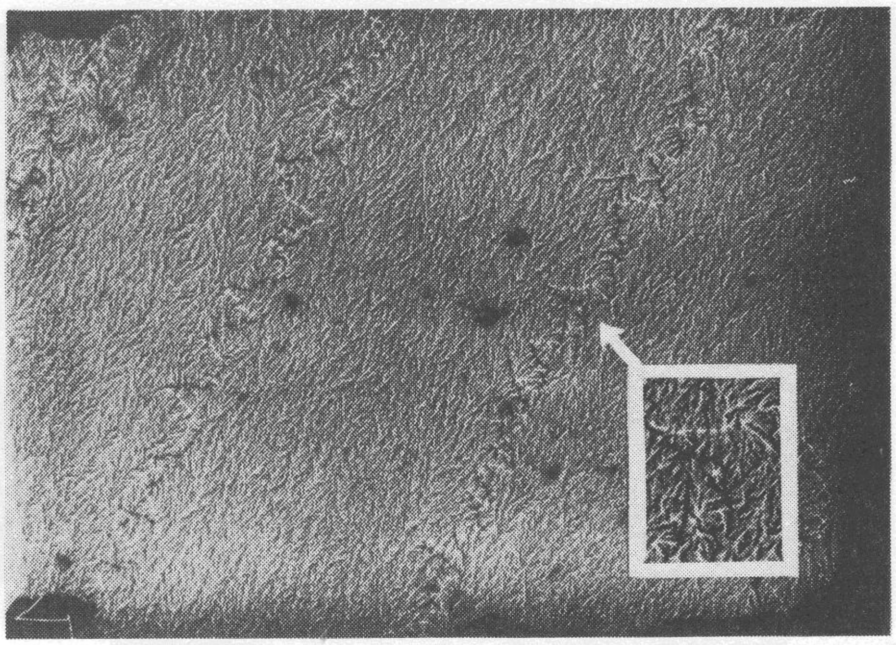
In terms of trying to understand the behavior of sputtered thin film media, I was able to make very rapid progress at PARC because I did most of the work myself, with the help of my technician, Bruce Charlan. I even prepared the TEM samples and looked at the samples by myself most of the time. I did have the TEM technician to help me with the tool, but I was the one making all the determinations of what to look for. Making the sputtered samples to making magnetic hysteresis measurements, and drag testing for recording characteristics and TEM microstructure analysis were done right after each other, and I made a tremendous number of samples. I had thousands of TEM micrographs that covered the sputter process parameters and different alloys. I had made microstructure characterization followed up by Lorentz images of recorded tracks. I don't think it is an exaggeration or self-promotion to say that no one else in the world had the sheer amount of data that I had on sputtered cobalt alloy film. The microstructural data from TEM was backed up with magnetic hysteresis measurements, and Lorentz images of recorded tracks. I also had recording data on selected disks that were made. It was based on this quantity and quality of data that I had which allowed me to form a clear theory as to what limits magnetic recording in thin film media. The research environment that I had at PARC and the measurement tools all contributed to this end. I had very good tools and I was left pretty much to myself to use these tools to develop my theory.

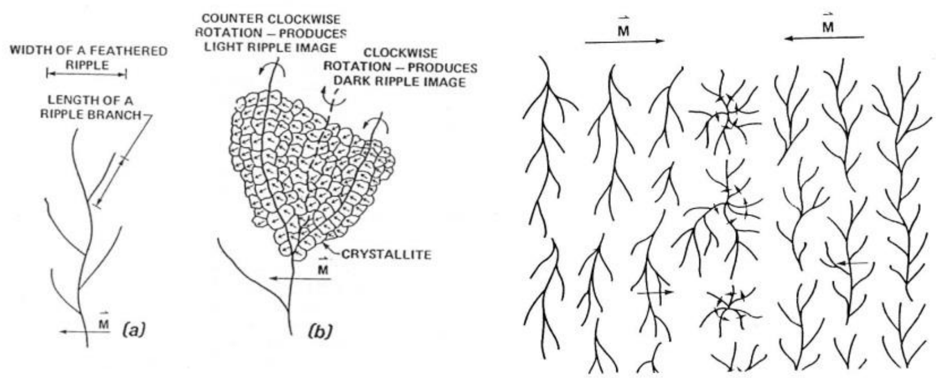
Armed with the data that I had taken, I became convinced that the physical origin of the limits to magnetic recording in thin magnetic media would be primarily caused by inter-particle exchange interactions between the magnetic grains. I concluded that in order to make viable thin film recording media, one needed to develop a film deposition process that would isolate the magnetic grains so as to suppress the exchange interactions between them. This conclusion became the theoretical basis for future improvement in the development of recording media at PARC as well as at Komag Inc. Today, the idea that one must isolate the grains in the media to reduce inter-particle exchanges and hence the noise, is taken for granted, and well-accepted by everyone that works on media. When I started to talk about this in late 1970s, most people did not understand what I was saying at first. Even within PARC, it was not well appreciated. Even Gordon Hughes, who went on from PARC to Seagate to become their top magnetic recording guru and later to become a professor at UCSD, had trouble appreciating it at first also. He kept asking for more hysteresis squareness, which at the time by conventional wisdom meant that the media would generate more signal, and it was believed by him and others that this would lead to a better signal-to-noise ratio. This bias had probably come from a mindset that came from working on particulate media, where the squareness of the media is actually very poor. The poor squareness is actually due to the fact that ferrite particles are very well isolated by grain boundaries and by epoxy. In an isotropic ferrite media, the squareness is so bad that getting the amplitude (signal) was indeed hard. But the noise was never a problem. By orienting the ferrite particles along the circumference of the disk with a magnet during curing, the squareness is improved and one obtained much larger amplitudes. In the plated Co-P film or sputtered Co alloy film however, the situation is completely reversed. The metallic grains are all connected together with all the grains in intimate contact with each other. The squareness is actually quite good because of it. Any lack of squareness will come mainly from grain separation effects. Consequently, a significant effort has to be put into the media to try to isolate the grains, and this will actually degrade the squareness. Only in this state, then, can the noise be reduced. In those days, the higher noise power observed in thin film media was something of a mystery, and many people accepted it as something that was inherent in the media. Therefore it was usually cited as another item in the list of disadvantages of using thin film media. However, I knew where the noise was coming from.

I was convinced that these new findings would be beneficial to the magnetic recording research community. Therefore, we put together a paper entitled "The Physical Limit of High Density Recording in Metallic Magnetic Thin Film Media" for the 1979 Intermag Conference in New York City (Reference 4-12). It was received well enough so that it was an invited talk at the conference. When I gave the talk, I was surprised by the huge number of people that showed up to hear my talk. The room that was intended for 300 people was filled to standing room only capacity. In the past, conference presentations on thin film recording media were always sparsely attended and the room was generally more than half empty. The large attendance for my presentation really puzzled me at first, but then I realized the room was filled with researchers that had been working on bubble memory before. The "bubble" literally burst for bubble memory in 1979, and researchers working on them were looking for something else to work on. This experience taught me a lesson that a new emerging technology may not always be the one to become commercially viable, and that I should be always aware of this possibility. Many people had jumped into bubble memory research because it seemed very interesting and also glamorous. But if it does not become commercially viable, the "bubble" will burst and the interest will fall precipitously, with the pun intended.

Even after my talk, I did not think that most of the audience understood the significance of what I was trying to say about thin film media, and how one might deal with its inter-particle exchange problem. I might have given the impression to everyone that the talk was about "limits of magnetic recording" with the thin film media having ~~as~~ some sort of inherent limitation. One person that did get it however, was Professor H. Neal Bertram from UCSD. He asked me lots of questions after the talk and commented that inter-particle exchange may cause excessive noise in the media, and asked me whether I measured the noise or not. I did not say in the talk that I actually had measured the noise. So I knew right away that he understood what I was talking about. At a later time, he would send his graduate student at the time, Jimmy Zhu, to Komag after I started the company to ask me in detail about my model for exchange induced magnetic clustering and resulting ripple structures observed in Lorentz microscopy. After this, Jimmy Zhu went back to UCSD to develop a theoretical model based on inter-particle exchange energy coupled with magnetostatic energy and crystalline anisotropy of Co particles in thin film, and built a computer simulation program for his Ph.D. thesis. Jimmy Zhu now heads the Data Storage Systems Center at Carnegie Mellon. Gradually over time, I believe that my theory on thin film media was accepted and people started to actively work on it as a way to change the structure of media. In the course of founding and establishing Komag Inc. as a major player in the thin film media market, I trained many people on media design and they went on to other companies and carried on the work. I published a paper on Lorentz microscopy of recorded transitions in 1981 (Reference 4-13), and followed it with more detailed description of the work in 1988 (Reference 4-14), which is shown on the next two pages.

With all these experiences and promising results I had obtained in sputtered thin film media research, I was not ready to give up my pursuit of trying to make a commercially viable product even though IBM had rejected thin film media for their new IBM 3370 drive. I continued to try to make improvements to sputtered media and tried to understand the relationship between media and its signal-to-noise ratio after 1979. I continued to write papers and give presentations on our research findings.





Above, from 1981 publication (Reference 4-13) showing Lorentz TEM image of recorded bits and transitions. Drawings below explain the Lorentz TEM image in terms of how it relates to the microstructure of the sputtered Co-Re grains. The Lorentz image appears as it does, because of the strong inter-particle exchange coupling between the grains. Bright star-like pattern signify strong inter-particle exchange which widens recording transitions. (Figure 4-8)

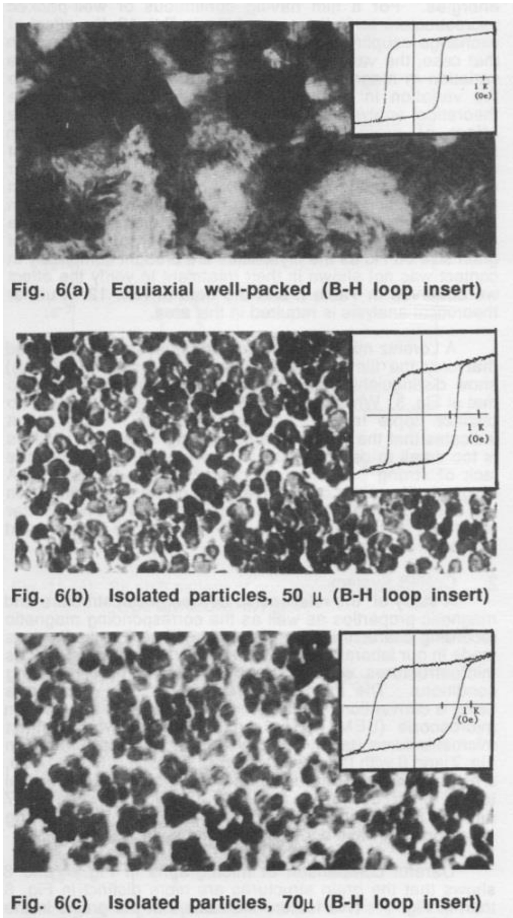


Fig. 6(a) Equiaxial well-packed (B-H loop insert)

Fig. 6(b) Isolated particles, 50 μ (B-H loop insert)

Fig. 6(c) Isolated particles, 70μ (B-H loop insert)

From 1988 publication (Reference 4-14).

Films sputtered at successively higher sputter pressure to show the grain isolation effect, and effect on the magnetic hysteresis loop. Sputter pressures were 5, 50, and 75 mTorr. As the sputtering pressure is raised, the squareness of the loop degrades.
(Figure 4-9)

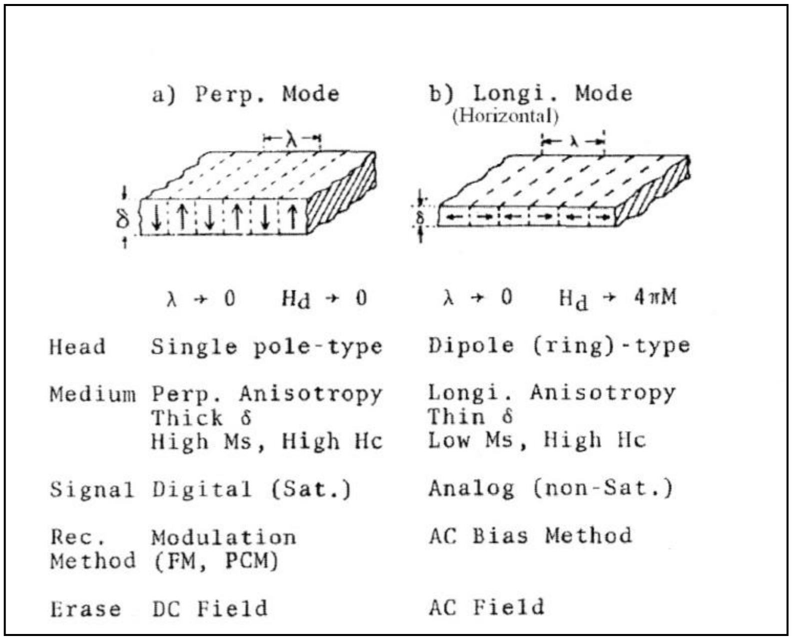
I also worked on the issue of corrosion protection for the thin film media, which was the concern raised by IBM and which kept them using oxide media. I spent much effort to find a suitable overcoat material and process for sputtered magnetic film. By 1982, I was able to develop a carbon overcoat by RF sputtering of a vitreous carbon target. A few years earlier, Francis King who was at Data Point, which was a subsidiary of Data Disk Corporation, had published a paper on carbon over-coated media (Reference 4-15). So there was interest in using carbon as an overcoat on media already but the history of how the carbon overcoat was actually developed is not so clear. Francis King's publication is the earliest one that I am aware of, but Data Point never patented this process and commercialization really did not occur for Data Point or at Data Disk Corporation. Supposedly SyQuest, which was founded by Syed Iftikar who had earlier also founded Seagate, was the first to introduce carbon over-coated plated thin film media in their SQ306 removable hard disk drive, and this product was announced in August 1982 at Comdex. They also had followed the Francis King publication to do the carbon overcoat. They used

Montedison AM2001 as the lubricant on the disk. They had made their own plated and carbon over coated disk, but later Ampex started to supply them with plated media without their lubricant so that SyQuest could coat the disk with carbon afterwards. Eventually Domain and PolyDisk started to make turnkey media for SyQuest and SyQuest stopped making the media by themselves (Reference 4-16). The carbon overcoat film that I produced incorporated a large amount of hydrogen in the film from the target itself, which I did not know or appreciate at the time. Unfortunately, because of the decision to terminate the R&D effort on computer and peripheral products by PARC management by that time, we were not able to evaluate the performance of the carbon overcoat on the corrosion resistance and mechanical performance (including tribology) of media. It was not until I co-founded Komag Inc. that I was able to evaluate the performance of a carbon overcoat on media. One thing that was very clear to me was that the sputtering process in vacuum is the preferred way to make workable media. The need for some form of overcoat to protect the magnetic layer also made it that much more imperative that the coating to be done right after the magnetic layer in the same system for easy manufacturability.

Perpendicular Media

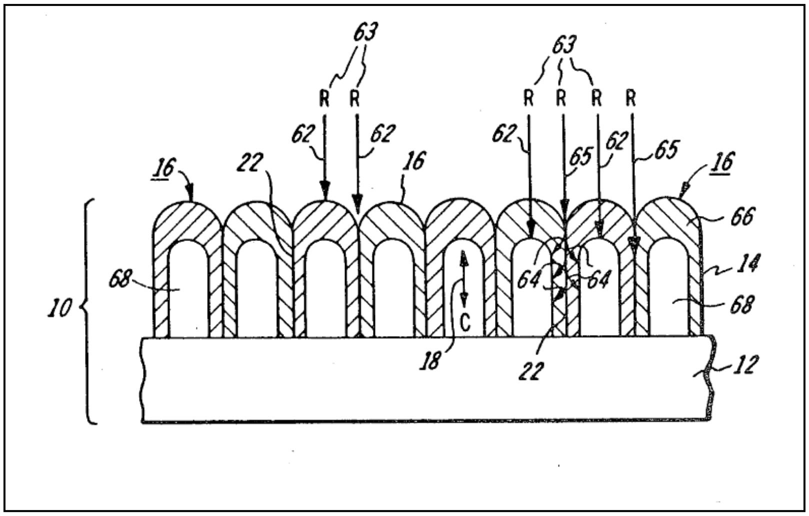
In 1977, Professor Shun-ichi Iwasaki of Tohoku University suggested the idea of perpendicular magnetic recording (Reference 4-17). The idea was to orient the easy direction of magnetization perpendicular to the plane of the media, so that recording took place perpendicular to the media plane. The beauty of the idea was that transitions do not have the usual demagnetizing effect from neighboring bits oriented in the opposite direction as is the case with longitudinal recording. Hence, it was thought that higher recording density might be possible. This proposal caused quite a stir in the magnetic recording community, and raised considerable interest amongst researchers in the field.

Perpendicular recording as a concept was actually tried out by IBM much earlier, starting in 1955 when they considered a perpendicular recording version of the famed RAMAC 305 drive as a second generation product. The project was code named ADF (Advanced Disk File), and they worked on it for 5 years. Many drives were apparently built and tested. Al Hoagland recounts this project in a publication in 2003 (Reference 4-18) and in his memoir (Reference 4-19). IBM had developed a complete system around perpendicular recording, including new media based on a steel disk, which was oxidized to create a magnetite coating on its surface as the perpendicular recording media. By 1960, there were numerous problems with the drive and IBM reverted back to longitudinal recording with oxide media. This history about perpendicular recording was not well-known outside of IBM, and was soon forgotten. It remained largely unknown in the magnetic recording community for a long time. I certainly did not know about it until I had long retired from Komag.



From Professor Iwasaki's 1977 paper explaining the perpendicular magnetic recording concept. (Figure 4-10)

Many researchers jumped into perpendicular recording after the announcement by Professor Iwasaki, and I started to work on both sputtered perpendicular film as well as on a plated version of perpendicular thin film media. The reason for considering plating as a process was due to the fact that there was interest in using perpendicular recording for tape and floppies, and the plating process could be a low-cost fabrication method for such applications. For the plated media development, I invited Professor Pietro Luigi Cavallotti from Politecnico di Milano in Italy to develop an electroplated perpendicular Co magnetic film in 1978. We were able to create very nice perpendicularly-oriented cobalt film, and we published a number of papers on the subject (Reference 4-20, 4-21). One of the key findings from the work on electroplated Co film was that I could arrange to isolate the individual columnar grains of cobalt by several methods: by chemical etching of grain boundaries and by anodic treatment of the film to create an oxide phase at the grain boundaries. By applying these processes, I was able to obtain a higher Hc and to decrease the demagnetizing field from the film. Both are important for improving the performance of the media. As it was in the case for longitudinal media, the concept of physically isolating the grains to suppress the inter-particle exchange interaction between the grains and was also important for perpendicular media as well. I was able to demonstrate this effect on sputtered Co-Re and Co-Ru perpendicular media as well by oxidizing the grain boundaries of sputtered film with post deposition annealing in air. These findings became the basis for several patents that we were able to obtain for perpendicular media.



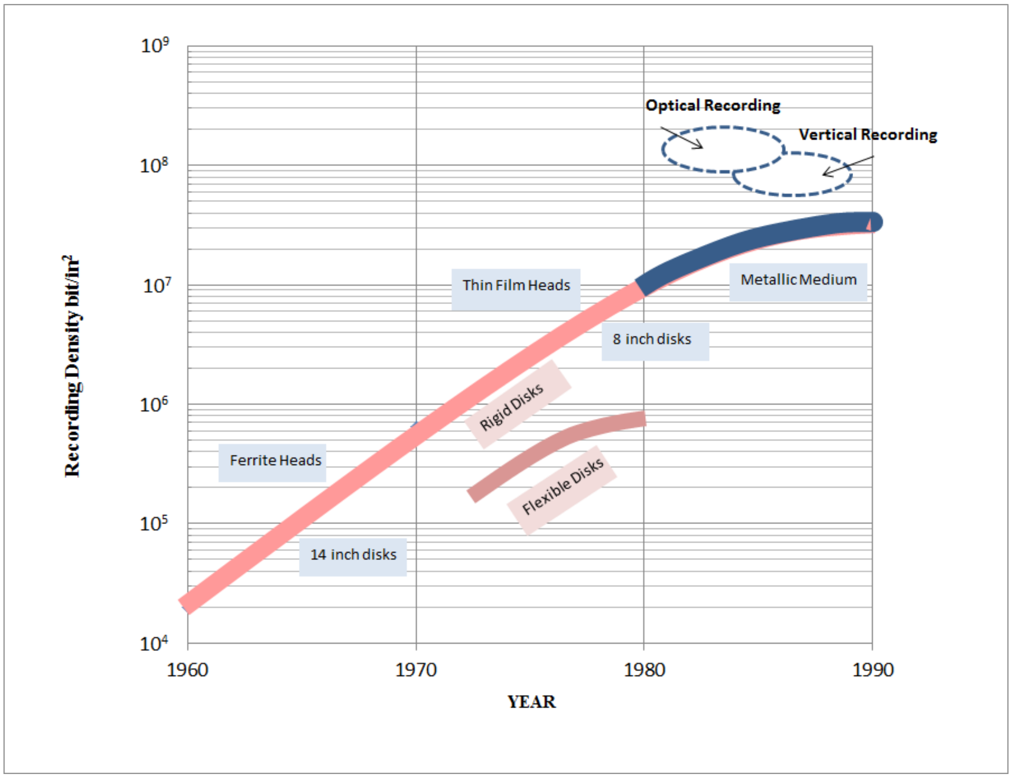
From US Patent 4,405,677, "Post Treatment of Perpendicular Magnetic Recording Media", Tu Chen, Sept 20, 1983, shows the schematics of grain isolation by chemical treatment of the grain boundaries (Reference 4-22). (Figure 4-11)

I also sputtered CoCr thin film and studied its microstructure. These films contained a very high amount of Cr, as much as 20 atomic % or more, and it was a popular alloy for perpendicular media at the time. The interesting thing about this alloy for me was that perpendicular anisotropy and Hc of the film were higher than they should have been based on the phase diagram of the system. On studying this material further, I became convinced that the accepted phase diagram of the CoCr was wrong, and I investigated past metallurgical studies on the CoCr binary alloy system. I found an old and obscure Russian reference that suggested there is a phase separation that occurs between the Co and Cr at high Cr content. I conducted extensive annealing experiments on CoCr films of different compositions and concluded that there is indeed a phase segregation that takes place in the film. More importantly, the Cr segregates into the grain boundaries between the CoCr grains. I published a paper on this topic with Tom Yamashita, then a graduate student at Stanford that worked with me (Reference 4-23). After publishing this work, I did not think much of it until many years later. As it turned out, the phenomenon of Cr segregation was of critical importance in isolating the grains in CoCrTa-oriented media that came to compete with the isotropic Co-Ni-Pt based media that we had developed at Komag. In any case, the importance of grain isolation in reducing inter-particle exchange was very clear to me. This effect, besides coercivity, was the single biggest factor in increasing the areal density of the media, whether it was the isotropic media which we had developed, or the oriented media based on CoCrTa alloy. It also became a critical factor in finally making perpendicular media work some 30 years after Iwasaki had proposed it. I dare say that particle isolation and how this was to be accomplished was the single most important factor in getting the areal density of thin film media to where it is today. However, I still would never have imagined that by using this concept the recording density could be extended to near the 1 Tb/in^2 that we have today.

View of the Areal Density Trend in 1980

It has been common practice in the magnetic recording field to plot the areal density trend by year to show the progress and also the prediction for the future and what technology might be used. It is interesting to go back to 1980 and see what we thought about the future and expected areal density trends. One example is shown below, reproduced from a *Scientific American* article dated August 1980 entitled “Disk Storage Technology,” authored by Robert M. White (Reference 4-24). This was a cover article on that issue. Robert M. White by the way was my manager at Xerox at one time, and he used some of my data in this article.

In this timeframe, the IBM 3340 drive with Winchester technology was already out, and thin film head technology was being explored. 14-inch diameter disks would be going to smaller 8-inch, but the 5.25-inch drive was not yet on the radar screen. Thin film media technology was going to carry the recording density out to about 30-40 $Mbit/in^2$. Obviously we were not very clear about whether recording density could be extended beyond this using thin film media. Perpendicular recording was thought to be something that might be used in several years, as well as optical recording. The future beyond 1990 was not so clear at that time. We can come back to the current areal density plot circa 2013, and see how far we have come, and how wrong we were from the 1980 perspective.



Areal density expectation from 1980. (Figure 4-12)

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Chapter 5

Leaving PARC and Founding Komag Inc.

The Final Phase of My Employment at PARC

In 1982, Xerox decided to exit the computer and computer peripheral business and my own work on magnetic film was to come to an end. Many researchers decided to leave PARC to start new companies such as 3Com, and Adobe, or formed spin-off or joint venture companies. Others left PARC to join the growing PC companies such as Apple Computer. Similarly, people working on data storage devices also started to look for new positions within PARC or seek new employment opportunities outside the company or contemplate forming a new data storage company. This was a sad ending for an organization that had done so much to develop the foundation for the computer revolution that was to come.

In 1973, they had started to work on the Alto computer, which was the predecessor to the Apple Lisa and Macintosh. By 1979 they had about 1,000 of these systems scattered throughout PARC and other Xerox facilities, and another 500 scattered at collaborating universities and government offices. It had a mouse that controlled the screen and graphical user interface (GUI) that all computers eventually adopted. It was also connected to laser printers and servers through the Ethernet. Disk storage was provided via a single 2.5 MB removable 14-inch disk drive, using oxide media. It was the Model 31 made by Diablo Systems, designed to use the IBM 2315 compatible cartridge. Diablo Systems was bought by Xerox in 1972. This single disk stored all the system software and also supported the local storage needs for the Alto computer.

Many of the things we take for granted in current PCs were all present or being incorporated into the Alto computer. When I wrote papers for publications for example, the secretaries that worked in the department would input my work into Alto and type out the papers as one would see it in the publications, with all the final fonts, scientific notations and mathematical equations. I don't think such a computer existed anywhere else. Xerox never attempted to commercialize the Alto. It reportedly cost about $32,000 each to produce.

Alto Computer and IBM 2315 Cartridge.
(Reference 5-1)

Disk: 2.4 MB, 14 inch oxide media
2200 BPI, 100 TPI
Areal density: 0.22 Mb/in^2

Images courtesy of Computer History Museum
© Computer History Museum
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(Figure 5-1)

The computer system in which Xerox attempted to commercialize the Alto technology was the Star Workstation (Xerox 8010 Information System) in 1981. But this system was too expensive and did not sell very well. The basic system cost was $75,000 plus $16,000 for each additional workstation. It has been said that Bill Gates bought one system immediately, just so that he and his team could study it.

The most famous story is that Steve Jobs came to visit PARC in 1979 and took much of what he saw in the Alto computer into his Lisa computer, followed by Macintosh. The reality however is that many of the components that were in the Alto were already being developed elsewhere. For example, Apple had projects to create computers with a mouse and GUI (Reference 5-2) and had hired several former PARC employees before Steve Jobs's visit to PARC. Apple also hired additional key people from PARC after the 1979 visit. Therefore, it is fair to say that PARC influenced the future of computing in a significant way. Apple started the Lisa computer project in 1978, and it was not until mid-1983 that the machine became finally available, but it cost almost $10,000 and never sold well. It was superseded by the Macintosh, introduced in 1984.

As for the Xerox Star Workstation, this foray into office computing turned out to be a disaster for Xerox. It lost so much money that Xerox management decided to get out of the computer business altogether by 1982. It was a continuation of the mismanagement and failure by Xerox to master the computer business that started with the purchase of SDS in 1969, followed by failure to capitalize on what they had in PARC. The SDS division was shut down in 1975 with large write-offs. Xerox's failure in computing was well-underway long before 1982 when everything came to a halt.

As it became clear that data storage activity would come to a close at PARC, people working in the MO data storage group under Dr. John C. Urbach, in the Optical Science Laboratory (OSL), decided to form a MO data storage company, by spinning-off a group out of PARC. They knew that in order to be successful they would need my expertise in MO media. Therefore, John asked me to transfer under him and help write a business proposal addressed to the upper management of PARC. The proposal was submitted by the end of 1982. One of the big supporters of the project (and perhaps the only one) was Frank Squire, head of human resources at PARC. A couple of months passed and we did not hear any response from upper management as to whether they would support the idea. Therefore, I decided to see Frank Squire to ask him about the status of our proposal. His replied that upper management was too busy with the reorganization of PARC and Xerox, and they hadn't had any time to look into the matter. However, he was still enthusiastic about the project and wanted to see that it got supported. Perhaps sensing that I might leave PARC, he asked that I wait for the decision from upper management. He emphasized that my knowledge of materials science was valuable for PARC, and if I wished, I could be transferred to the semiconductor research lab right away while waiting for the decision. It was an interesting proposition, since I did some research on semiconductor materials at Northrop before. After thinking about it for a few days I decided to not ask for the transfer. I thought that it would be better to find new employment in a magnetic data storage business or to start my own magnetic thin film media company. The decision was based on the fact that if I had switched to semiconductor research, I would have had to start all over again to rebuild my knowledge and reputation. I had devoted a major portion of my research life to magnetic recording and built up significant knowledge and reputation in the field. Finding new opportunities in the same field was much more attractive.

Another big factor was the rapid emergence of the personal computer (PC) business in the early 1980s. It finally represented the opportunity which I had looked for during the 12 years of work at PARC. The demand for low cost, small form factor and large capacity data storage devices for personal computers was so urgent that the opportunity to commercialize either the magnetic thin film media or the MO media was finally upon us. Therefore, it should have been easy to find a job in an existing company that saw this as opportunity, or else start my own company to produce the storage media for the drive companies that were after this market.

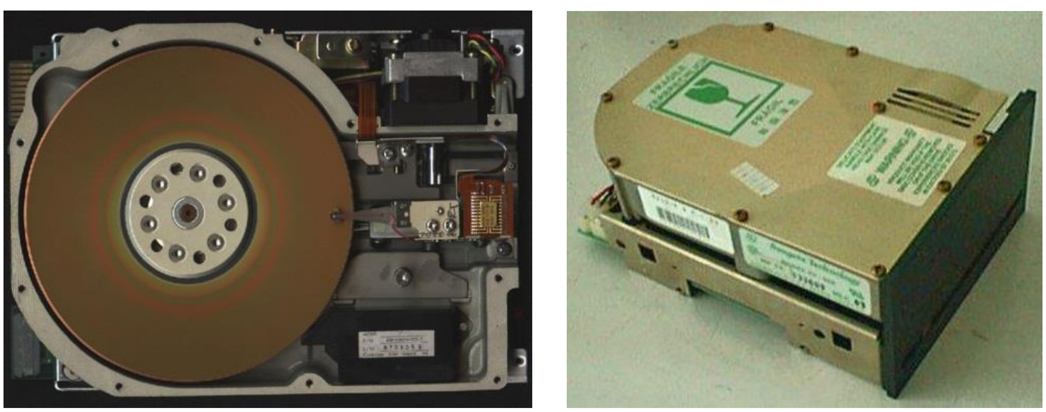
When the PC was first introduced in the beginning of the 1980s the only low cost data storage device available at that time was the 5.25-inch diameter floppy disk drive. The floppy drive was adopted as the input and output (I/O) data storage medium for computer programs as well as data. Unfortunately, the capacity, data rate and access time of floppy drives were woefully inadequate to support the much faster semiconductor memory. For the PC to perform at its full promised capability, a low cost and large capacity peripheral memory, which could deliver data rates and access times closer to those of semiconductor memory, was needed. To meet this requirement in the mainframe or mini-computer, the Winchester hard disk drive had been developed. The hard disk drive still had slower access time than a semiconductor memory, but it was able to deliver sufficient data rate to be useful as an in-line buffering memory in conjunction with the semiconductor memory.

The reason for using the hard disk drive instead of or in conjunction with semiconductor memory in the mainframe or mini-computer was quite obvious. The cost per megabyte of storage capacity in a hard drive was several orders of magnitude less than that of a semiconductor memory. Also, the total cost of the hard disk drive was only a fraction of the cost of a mainframe computer. On the other hand, if one had adopted the hard drive of that era to the PC, it would not have been practical. The disk drive at that time was much too large and too expensive. Clearly a much smaller and much cheaper version of the hard disk drive, one suited and matched to the PC was needed, and there would have been a huge market for it if it could have been designed and delivered at a low enough prices.

Emergence of the 5.25-Inch Form Factor Disk Drive and of Seagate Technology

Around the end of the 1970s and the beginning of the 1980s, many people and companies in the peripheral data storage industry saw a gigantic opportunity in the growing PC business. The first company that emerged for making a low cost hard disk drive specifically for the PC was Seagate Technology. It was founded in 1979. Seagate took the approach of creating a hard disk drive with the same form factor as the 5.25-inch diameter floppy disk drive. It should be noted that the smallest form factor hard disk drive at that time was 8-inch. Seagate figured out correctly that the 5.25-form factor would be the appropriate size drive for the PC because it could have fit into the same size slot used for the floppy drive. Therefore, it would be accepted more easily by the PC industry. It very quickly became a standard. Another big factor was that by making everything smaller, the cost of the hard disk drive was expected to decrease significantly enough to be able to play in the PC market. A smaller drive with smaller disks would have used a much smaller motor; hence it would have generated much less heat; the drive would not have needed large and sophisticated cooling, etc.

To meet the time to market requirement and reduce the cost of the drive, Seagate took the approach of adopting technologies that had been established a few generations earlier. With this innovative (and improvised) approach, the company was able to successfully introduce a commercial hard disk drive for the PC industry having a storage capacity of 5 megabytes on 2 disks in 1980. This drive became the famous ST-506 model, which cost around $1,500. It is of interest to note from a technological viewpoint how Seagate was able to introduce a commercially viable product only one year after its founding. The engineers who designed the drive used much less stringent requirements for the components than the state-of-the-art components being used in the very expensive high performance drives being made at that time. For example, the ferrite heads and oxide media they used in their first drive supported an areal density of about 1 Mb/in^2, which was similar to the density of the IBM 3330 to 3340 disk file technology that had been introduced eight years earlier, in 1971 to 1973. For the track servomechanism they used a stepping motor because they did not have very sophisticated design capability for the closed-loop servomechanism used in the high-end drives. They could not afford it either. As a matter of fact, Seagate was never able to design a workable closed-loop servo system on their own until they acquired Imprimis Technology from Control Data in 1989. I looked at Seagate's business strategy with some admiration. It made me realize how important it is to focus on introduction of a product that can be made quickly. Speed was of the essence therefore, one had to be very careful about selection of the technology to use for the product.



Seagate ST-506 Drive, designed to fit into 5.25-inch floppy bay of IBM PC. Drive had 5MB capacity on 2 oxide disks.

From Wikipedia (Reference 5-3)
(Figure 5-2)

Seagate's success in making its ST-506 model in 1980 prompted existing companies as well as new start-up companies to jump onto the bandwagon to try to make hard disk drives for the PC market. From 1980 to 1983, many new companies got their start: Maxtor, Vertex, Evotek, and AIM among others. Maxtor was founded in 1981 to create a higher capacity drive by using more disks (up to 16 disks) in a standard full height 5.25-inch form factor, by using an innovative spindle motor designed to fit into the inner diameter of the disk stack to save space so that more disks could be stacked inside the drive. They planned to jump ahead of Seagate by introducing 40 to 80 megabyte drives by using the oxide media technology and a larger number of disks in the drive to start their initial production. The follow-up products planned to use thin film media from plated media suppliers such as Ampex or SAE for drives with capacities up to 160 megabytes or higher. AIM was even more aggressive in considering use of perpendicular recording technology.

The strategies for Vertex and Evotek, founded in 1982, were based on using drive designs similar to Seagate's drive, but adopted the use of thin film magnetic media to achieve higher capacity. By using thin film media, they could instantly double the capacity achievable with oxide media, or at least so they thought. It was all due to the one-upmanship being practiced by the start-ups so that they could get funding from the venture capital firms. The promise of mass production of plated thin film media by Ampex Corporation also played a part. Ampex started media production 1981. This product was called "Alar", and Ampex promoted it heavily.

It is also interesting to note that while there was a flurry of hard disk drive startup companies all trying to capture the market for PC hard disk drives, the well-established drive companies did not pay much attention to this market. The captive drive makers such as IBM, Control Data, HP, Digital, Fujitsu, Hitachi, NEC, and Toshiba had little interest in developing hard disk drives for the PC market, at least initially. In addition the independent OEM manufacturers such as Quantum, Memorex and Priam were also slow to pay attention to the PC market. Most of these companies were too busy looking at the mainframe or the minicomputer markets and did not seriously consider the 5.25-inch drive technology until after 1983. For example, IBM did not make a 5.25-inch drive until 1986.

Looking back now, one can see a clear progression from the 5.25-inch small form factor drive that was originally designed for the PC in the early 1980s to the current 3.5-inch drive that presently dominates both the large enterprise application market as well as the desktop PC arena. In the early 1980s however, no one would have imagined this scenario. Mainframe computers and very expensive hard disk storage made by IBM and other major players such Control Data and Memorex dominated the computing and storage worlds. PCs started out as a hobby by computer enthusiasts, and there was going to be a growing market for them. But few imagined that they would actually takeover much of the computing environment. Even in 1983, when I started Komag Inc., the IBM 3380 drives generated revenues of over $6B per year for IBM, one of the most profitable products that IBM had ever produced. It was priced from $81,000 to $142,200. It appeared that IBM was content to let small start-ups deal with "cheap" and small capacity disk drives.

Media for 5.25-Inch Drives

The 5.25-inch form factor drives spurred the start and growth of OEM media companies that were to supply media to drive companies. By 1979, companies such as Memorex, Magnetic Peripheral Inc. (MPI) and floppy media companies such as Nashua and Dysan were quickly getting into the business of making oxide media for the 5.25-inch form factor drives. Also a startup company, Charlton Corporation was founded in 1980 to produce the oxide media disk. Charlton Corporation was co-founded by Mr. Charlton and the CEO of Dysan based on a new aluminum substrate grinding technology to reduce the cost of producing the disk. By 1982 the aforementioned companies all geared up to mass-produce oxide media and some became quite successful. For example, Dysan and Verbatim became the darlings of the stock market as they rode the wave of the PC market boom, making oxide media for hard disk drives as well as floppy disks. Introduction of the plated Alar media by Ampex Corporation in 1981, along with the new start-up drive companies planning to use the media, such as Vertex and Evotek in 1982, started the transition away from oxide media to thin film media for the small form factor HDDs. Some oxide manufacturers saw this coming and started to plan accordingly. Others, Dysan, Charlton and Verbatim among them, did not. For whatever reasons, they believed that thin film media would not catch on and these companies came and went quickly, like a shooting star.

Emergence of Plated Media Companies in the Early 1980s

By mid-1982, because of the surge in interest in thin film media technology, it was becoming clear that oxide technology was running out of steam. Anyone with knowledge of thin film magnetic media technology became highly sought after. It was fortunate for me that the number of experienced people in the field was still quite limited. I received many invitations from existing and startup companies for interviews. After accepting several interviews I found out that most of them were only interested in pursuing plated media technology, along the same line as Ampex's Alar technology. AIM and Lanx were among the few start-ups that wanted to make sputtered perpendicular media.

Ampex Corporation is a legendary company in the field of magnetic recording. It was a pioneer in developing audio tape recording and developed much of the early video recording technologies. Their instant replay machine based on disk drive technology is quite famous. The successful demonstration of instant replay in the TV broadcasts of the 1966 Winter Olympics using a recording system based on plated media, allowed them to establish a strong in-house technological base in magnetic recording technology. The company did considerable fundamental R&D in recording media technologies, ranging from metallic particles to oxide magnetic media, and magneto-optic media to plated media. Many key engineers and scientists in the magnetic recording industry got their first start at Ampex. With this historical background, it was quite natural that Ampex got into the business of commercializing the 5.25-inch form factor plated thin film disks. They had the experience and the knowledge to do this. Accordingly, Ampex invested $40 million to mass-produce the plated media for the anticipated market demand. With such an investment and being Ampex, plated media gained instant credibility as a viable technology. This in turn got the interest of the venture capitalist community, and several new plated media companies were funded to join the bandwagon. By 1983, there were at least 13 companies involved in trying to make plated media for the OEM hard disk drive market. They were:

Ampex, Domain, SAE, Ultradisk, PolyDisk, Megastore, KSI, IMI, Eikon, Grenex, Microdisk, Burton, and Evotek.

In addition, existing media companies such as Nashua, MPI and Memorex were also working diligently to try to develop plated media. I also mentioned earlier that SyQuest had a plating operation to supply media to them for their removable disk cartridge. Among all these companies, I interviewed only a few of the bigger ones, and a few start-ups. After all the interviews with the plated media companies, I concluded that none of them had good prospects. Based on my knowledge and experience, I did not believe that plated media technology could succeed. I actually tried to convince the interviewers that they ought to focus on sputtering technology instead. They were probably offended by my comments, but I was speaking my mind. I also found that many of the interviewers did not have sufficient experience and expertise about thin film technology, magnetic recording and what it would have taken to make the technology really work. Nevertheless, the interviews were useful for me to size up what was taking place in order to meet this coming demand for thin film media. I started to realize that there might be a real opportunity in trying to start up a sputtered thin film media company.

During this time period I also interviewed with Cyberdisk, which was a sputtered disk start-up. The leader of this effort was Virgle Hedgcoth. He had a chrome mask background. I was not very impressed as Hedgcoth and the team did not know anything about magnetic media and magnetic recording. But he learned a lot about sputtering from the chrome mask business. Hedgcoth’s name will come up again later as you shall see. I also interviewed with Southwall Technologies. They wanted to coat magnetic films on to plastics. Their expertise was on roll coaters, where giant coating machines are used to coat rolls of plastics for a variety of applications, such as for making potato chip bags, for example. At this time period, clearly "everyone and their brothers" were trying to get into the business. I also interviewed with Domain, who for a while was to become one of our big rivals in the business. Needless to say, I did not seriously consider working for any of these companies.

Interest in Starting up Sputtered Thin Film Companies

During 1981 and 1982, there were talks of many start-ups that were planning to use sputtering to create thin film media. There were many engineers and scientists from well-established data storage companies such as IBM, Control Data and Memorex who also had the idea that sputtered thin film technology could have been adapted to service the new hard disk drive demand. There were many others from other fields such as the chrome mask and semiconductor industries that also thought that they could have applied their expertise to making sputtered thin film media. Those in the "sputtering technology camp" thought (correctly) that vacuum processing would have been cleaner and could have been more extendable to produce higher recording density media than plated technology. Also, the sputtering technology could have been adapted to make perpendicular magnetic recording media in the future. At that time, the leading proponent of perpendicular recording and media development was Professor Iwasaki at Tohoku University, Sendai, Japan. His team preferred to use CoCr films prepared by sputtering. It was quite easy to see that the future would be with sputtering, at least to all of us in the "sputtering technology camp".

Energized by the thought that there was a real opportunity with sputtering technology to create a viable business, I started to formulate a business plan in 1982 to start a company. For the sputtered film technology there were two paths. One was to take the traditional in-plane longitudinal recording technology path, with the strategy of replacing the oxide media and competing with plated media for extendibility. The second path was to invoke the new perpendicular recording technology.

For the perpendicular recording technology, two early companies that were funded were Lanx and AIM. Lanx was founded principally by IBM employees Bob Potter and Neil Heiman, and AIM was founded by a group of people from various drive companies, including the media expert Bob Fisher from Memorex Corp. Bob Fisher had tried to hire me to work on plated media at Memorex, and also at AIM. AIM tried to make both the drive and media, while Lanx was going to make only the media. They had an agreement to supply media to a major drive company. Perpendicular magnetic recording was going to be a new paradigm shift, which promised greatly enhanced recording densities, and extendibility far into the future. By starting on this path from the beginning, these start-ups would be well-positioned to take advantage of the new technology and they would do away with all the old technology based on longitudinal recording. It must have been a good story to tell to the venture capitalist community and they were funded. What was not discussed or perhaps not well-understood was the fact that a disk drive based on perpendicular recording had not yet been developed, as well as the perpendicular recording heads, and all the associated electronics and signal processing capabilities needed for this new form of recording. Consequently, the market for the perpendicular film media did not develop at that time. In fact, it took almost another 25 years before perpendicular recording started to be used in hard disk drives.

During 1981 and early 1982, the talk (and the hype) for perpendicular recording technology was so high, that I received several inquiries from venture capitalists for my opinion about whether to invest in companies touting perpendicular magnetic recording technology. One of the venture capitalist investors, Mr. Blake Downing from Robertson and Coleman venture fund division came to see me personally at PARC to ask for my opinion about Lanx and AIM. It was an odd meeting as I ended up having to pay for breakfast at PARC for a rich venture capitalist, and my advice was given for free. I advised him that investing in perpendicular recording startups was not wise at that time because it required developing a completely new set of drive technology, which had not yet been well-defined. I reasoned that perpendicular magnetic recording technology would have required many more years of investment, and venture capitalists cannot wait so long. On the other hand, I made the pitch that if he wanted to invest in the hard disk drive industry, he should consider investing in a startup that would produce sputtered longitudinal media to compete against the plated media. The market for sputtered thin film media is there already, and sputtered media should be superior to the plated media.

After several sessions with Blake about the pitfalls of investing in a perpendicular recording technology startup, he became impatient and told me that if I was such an expert on the subject, why was I not starting up a longitudinal sputtered thin film media company on my own? He even offered me a million dollars if I decided to start a company. But I told him that I was still quite satisfied working for Xerox PARC, and I intended to keep working for PARC as long as they thought that I was useful. However, he didn't want to take no for an answer, and told me that the offer is there any time I decided to start a media company. When I finally did start Komag, he kept his promise and provided that million dollars. That free breakfast and free advice I gave to Blake was the best investment I had ever made.

My Final Decision to Leave Xerox PARC

I waited six months for a response from PARC management on whether they would sponsor the MO memory venture, but none came. I decided that I could not wait any longer, since I would soon miss the opportunity of starting up a sputtered thin film media business. In many respects, I was already late into the game, and as I found out later, much of the venture money was already being tied up with start-ups that started earlier. By April of 1983, I decided to put together a business plan for starting up a new media company to compete with plated media.

While I was preparing the plan, I contacted one of my best friends, Dr. Jim Shir from the IBM Research Center in San Jose to invite him to join me in starting up the company. Jim was a very talented researcher. For many years he had worked on magnetic bubble memory technology and produced many patents on the subject. He became very knowledgeable about the theory of micro-magnetism based on his experience with bubble memory work. His academic training was in mechanical engineering, but he had acquired very good theoretical knowledge of magnetism and magnetic recording. His knowledge of modeling combined with his mechanical engineering background would complement my knowledge in materials science for developing viable hard disk media. I had known Jim since my time at University of Minnesota as he followed me a few years later in the mechanical engineering department. When I moved to the Bay Area in California, we had formed a Taiwanese Association, named the Taiwanese Alliance for Inter-Culture (TAI) to promote better human rights treatment in Taiwan. Jim and I were the founding members. As good friends, we had often talked about the possibility of starting up a company. In early 1983 we took our families on a trip to Lake Tahoe, and we spent a couple of evenings talking about our chances of starting up a media company. When I asked him to join, he quickly accepted without any second thought. We started to work on our business plan.

Once we had our business plan, I made an appointment to see the head of Xerox PARC, Dr. William J. Spencer. I was hoping that I could ask him about the status of the MO venture, and also to present him with the new thin film media business proposal. I thought, perhaps naively, that if I could convince him to sponsor either of the two proposals, he might make a recommendation to the Xerox venture fund group to fund the startup. However, Bill Spencer was quite busy, and I had to wait more than two weeks to see him. While I was waiting, I contacted several people I knew in the same business, and Blake Downing from the Robertson-Coleman Venture Fund. Once I told Blake of my plans, he became quite excited and promised me that he would convince his fund management to invest the one million dollars that he talked about before. However, he told me that I needed to find a good lead venture company to raise the rest of the money. Blake’s company was an investment banking firm and as such, could not be a lead investor. With the million dollars already promised from Blake, I thought that I could use this to convince Bill Spencer that the business plan is for real, and perhaps help convince Xerox to invest in my new company.

However, in my heart I did not have much confidence in Xerox management to act decisively. In order to learn more about the business landscape, I decided to talk to people that I knew at new drive start-up companies such as Maxtor and Vertex. One of my acquaintances from PARC was Max Ross, who had just left the PARC/ADL group to join Vertex. I had heard that he was looking for viable thin film media to use in a high capacity 5.25-inch drive they were designing. So when I met him and told him of my plan to startup a sputtered thin film media company, he became quite excited and introduced me to one of the Vertex cofounders, Jim Atkinson. It turned out that the meeting with Jim Atkinson was very useful. He told me about Vertex's strategy, which was to use thin film media and conventional drive technology to introduce a series of high capacity drives, between 40 to 160 megabytes, in the 5.25-inch form factor. Most importantly however, he also told me about the serious problems that they were having with the plated media they were trying to use. The media had serious corrosion and mechanical interface problems in the drive and they were desperately looking for a solution. This problem was named "the black plague" and apparently everyone that had been trying to use plated media had the same problem. The talk with Jim Atkinson really hit home, and I realized that I had a tremendous opportunity for creating a new product that would have a real and immediate market if I could solve the problem.

Jim Atkinson, having started Vertex, knew many venture fund companies, and he introduced me to a young general partner of the Hambro International Venture Fund, Arthur C. Spinner. Jim told me that Art had just raised over $70M dollars for Hambro and was looking to take the lead to fund new start-up companies. As it turned out, he had already looked at several media start-up companies to take the lead investor position, but somehow he had missed the opportunity to do so. In brief, Art Spinner was looking for a good media start-up to fund as the lead venture investor. Jim told me that if I could convince Art, Art would find the money to start the company.

Armed with the promise of a million dollar investment from Blake Downing and news of a viable market need for thin film media from Jim Atkinson, I was quite excited to see Bill Spencer. A couple of days after I met Jim Atkinson I finally got to meet Bill Spencer. The first thing I asked Bill Spencer was about the MO venture proposal that we had submitted a few months earlier. He responded by saying that he was too busy to look into the proposal, and added that Xerox is in the middle of trying to redefine the future business strategy and would not be interested in anything to do with the computer peripherals business that we had proposed. Upon hearing this I immediately changed my direction and asked for his opinion on the proposal for a sputtered thin film magnetic media business. He was really not interested even in this subject, as he said he was quite busy so that I had only 5 minutes to give him my presentation of the business plan. So I had to quickly tell him the basic outline of my thin film hard disk media business plan, and that I could probably get outside venture funding. Once I summarized my presentation, his first question was rather peculiar. He asked me how old I was. I told him that I was 48 years old. After this, he said to me that Xerox would not be interested in the project, and told me that it would be advisable for me to start the company, as it would be my last chance to start a company at my age. I was surprised to hear him say this to me. I then asked him whether he would be willing to license the patents that I filed for Xerox. He told me that Xerox was not going to use the patents anyway, so he was willing to write a letter of approval for licensing of the patents, and to indicate his willingness to bless my new venture. On the one hand, I was disappointed by the lack of interest by Xerox in my business proposal, and lack of interest for all the years of work that I put into development of magnetic media. But Bill Spencer's blessing for me to leave Xerox and use all the knowledge that I had gained with the promise of a license to practice it was huge. I could start the new business in good conscience. I owe a huge debt of gratitude to Bill Spencer for helping me to decide on leaving Xerox PARC to start a new venture.

I was so excited to get Dr. Spencer's blessing that I went to see Jim Shir and another friend, Scott Chen, right away, to ask them to become partners in starting up the new company. Scott had more than a decade of experience in the engineering of hard disk drives at IBM San Jose, California. Scott's experience would add strength to the application side of hard disk media. With the three of us as co-founders of the company, I felt that we could be successful. Therefore, I set a formal meeting with Jim Atkinson to seek his help in learning how to incorporate the company and establish the valuation for the funding, and also in finding the lead venture firm. I also set up a dinner meeting with Blake Downing to introduce my two partners to him and reconfirm with him his million-dollar promise.

About a week later, Jim Atkinson introduced us to Arthur C. Spinner at his house and we presented to him our plan, and our background. Art Spinner was a co-founder and General Partner of the Hambro International Venture Fund. Art was very excited with our plan and he was immediately convinced that we would be a good bet for him to invest in and take the lead investor position for the venture. He told us right there that he would be able to commit $1.25 million to the project if I could go to New York to present the project and convince the managing director and other directors of the Hambro Venture Fund about the idea. He told us that he would invest the first $250,000 as seed money but in return, we had to show our commitment by quitting our jobs and start to work full-time on starting up the company. We all agreed that once his partners approve the investment, I should resign from PARC immediately and incorporate the company to seek the rest of the $3.5 million dollars of funding which we had proposed. As it was conventionally done back then, Art would get some discount for his $250K seed money as the lead venture investor. We all agreed to this.

Art Spinner and Jim Atkinson would also help look for an acting president to organize the company and to raise the rest of the money. We also agreed that if by end of September of 1983 we did not raise the rest of the money, Art would lose all his seed money and we would have to find a new job.

A couple days later after the meeting at Jim Atkinson's house, Jim and Art persuaded Ferrell Sanders to be our acting president. It was easy for us to convince Ferrell to join us because he was an experienced veteran of the business. Ferrell was a co-founder of Shugart Associates, which was founded in 1973 to produce various computer peripheral devices, including floppy disk drives. He served as vice president of marketing for Shugart Associates for many years. He had familiarity with the magnetic storage market and industry, and he was on the board of directors of Adaptec, so knew how timely the business of starting up a thin film magnetic media company was to the hard disk drive business.

About a week after I met Ferrell, we were invited by Art to visit Hambro's New York headquarters to present our plan. It was an exciting and scary moment for me. I had plenty of experience in making presentations but it was always for scientific conferences and in company settings. I had never made a presentation in front of venture business people that controlled a lot of money. Now I had to ask for a lot of money and convince them that I had a great idea for a new business. My whole future depended on this one presentation. Furthermore, I found that before I got any funding, I had to spend my own money to cover all the expenses for this trip. I was truly on my own now, and in a very different situation than anything I had experienced before. When I arrived in New York the day before my presentation, the hotel reserved for me by Art was near Park Avenue and it cost over $200 a night for a tiny room. This at the time was the most expensive lodging I had ever paid for on my own or on company business.

For the presentation the following day, I had to use different tactics than what I might have used in front of scientists and researchers. I would be talking to businessmen with little knowledge or appreciation of the technology. I had to become a salesman now, and I thought that I should use some "props" to make the pitch more dramatic. In order to show the differentiation between sputtering technology vs. plating technology and show how we are going to compete with existing plating companies like Ampex, I brought along a thin film medium that was coated on a conventional NiP-coated aluminum substrate and also on a Corning Photoceram ceramic substrate. I needed to make the point that the sputtering process for making the media had the advantage of extending the basic process to any future advanced substrates, whereas the plating process could only work on aluminum substrates coated with plated NiP. To dramatize how important the strength and hardness of the ceramic substrate were for future low flying head applications, I took the medium on the ceramic substrate and slapped the disk on the table in front of me. As I expected, everybody in the room except the big boss, Mr. Goodman, were quite impressed to see that the disk was still intact even though the disk made a big dent on the table.

After my presentation, Ferrell and I were asked to go to the courtyard to wait for their decision. When Art walked with us to the courtyard for the recess, I asked Art why Mr. Goodman was rather glum and appeared rather unhappy with my demonstration. Then he told me, with grave seriousness, that the table which I had just dented was Mr. Goodman's prized piece of antique furniture, which he collects, and it was supposedly really expensive. Right then I thought that I had just blown my chance for getting funded. My desire for dramatics by showing off completely backfired on me. About ten minutes later, Art came down to the courtyard to fetch us back to the meeting room to hear the verdict. The first thing that Mr. Goodman said was that he doesn't like the name "Sputtered Tech Corporation" which we used as the name for the new company. My heart sank after hearing this because I thought that he must be really mad at me for what I did to his prized antique, and I really felt like I had made a fool of myself. However, after a few moments, he started to say that if I can come out with a better name for the company, he would commit to the investment as Art had proposed. And that was it. It was a huge relief, and at the same time, I was immensely excited. I apologized profusely for the damage done to his table.

On the way back from New York in the plane, Ferrell told me to come up with a new name, which would have a good sound and a meaning, and it may be Asian in origin. I searched through Japanese, Taiwanese and Chinese words for a whole sleepless night. The next day I came up with a new name, "Komag Inc.". When I called Art about the name, Art said he liked the sound, but asked me what it meant. I told him that "Ko" means "character with good virtue" and I liked it so much that I used it for all my children's middle name, whereas "Mag" is magnetic. Therefore, Komag meant "magnetic media with good character and virtue". It turned out that he liked this name so much that I didn't have to come up with any alternatives or lose more sleep over it.

Once Hambro formally committed to the deal, I resigned from PARC with two weeks' notice, left PARC on June 3rd, and incorporated Komag on June 7, 1983. After that, I rented a temporary office and Ferrell helped me to rewrite the business plan so that we could raise the rest of the funding that we needed. However, it turned out that raising the balance of the money we needed was much harder than we had originally thought. That is because we were already quite late in starting up a sputtering media company, and most of the venture fund companies had already invested in one of many competing thin film media startups. Consequently, it was quite hard to find additional venture companies to invest in us. I will discuss more about this later.

Other Competitors for Sputtered Thin Film Media

With everyone looking for emerging opportunities in the hard disk drive market, there were already many drive and media companies that jumped into this market by the early 1980s. It was well-known at that time that in order to increase the capacity and reduce the cost per megabyte of a drive, one had to change the recording media from conventional oxide to metallic thin film media. Therefore, the newly funded companies were all focused on thin film technology. As mentioned earlier, there were three competing metallic thin film technologies being considered at that time. They were plated CoP thin film, sputtered longitudinal thin film and sputtered perpendicular thin film.

Ampex was the leader in plated media technology, and by virtue of their size, reputation and investment in the technology, many drive companies were planning to use their media. They were Atasi, Cogito, Control Data, CMI, Evotek, IMI, Maxtor, Micropolis, MiniScribe, Priam, Quantum, Rodime, Seagate, SyQuest, Tandon, Tulin, and Vertex. There may have been more that I might have forgotten, but there were many companies trying to make disk drives and it was quite crowded.

By the time we started our business venture, there were other plated media companies besides Ampex that had already started. They were Burton, Domain, Eikon, Evotek, IMC, KSI, Megastor, Tandon, Microdisk, SyQuest, PolyDisk, National Micronetics, SAE and Ultradisk. Again, there may have been more that I might have missed. As for sputtered thin film media, companies touting perpendicular magnetic recording media were Lanx, Censtor, National Micronetics (Lanx licensee), CDC (Lanx licensee), and AIM. AIM was originally funded to make perpendicular recording media and the drive, but after a few years of trying, they changed to longitudinal recording technology. For longitudinal recording media there were Komag, Akashic, Cyberdisk, Gemini, Lin Data, Oktel (Xidex), Grenex and Trimedia.

There were several longitudinal sputtered thin film media startups that got their start between 1982 and 1983. The earliest startup company was Lin Data Corporation which was founded by Charles Lin. Charles Lin worked at IBM for a long time during the 1970s on bubble memory research. By the early 1980s bubble memory research was coming to an end at IBM and Lin started to look elsewhere for a topic to work on. Like many researchers in the storage technology field, he also realized that opportunity lay in starting up a company to make sputtered thin film media. With his knowledge of the IBM research effort in this area, he identified a technological strategy for making a thin film medium and prepared a business plan to start the Lin Data Corporation in late 1982. Initially, he started with his own money but later started to solicit outside venture funds. I believe that Charles Lin had a reasonable knowledge of the magnetic recording and thin film media technology, at least based on his access to the publications in the field and being at IBM, but as far as I know, he never worked directly on thin film sputtering technology. Therefore, he was looking for researchers who had in-depth knowledge of thin film technology for his venture. The story is that he approached Dr. Ken Lee, who was then a manager in charge of research on hard disk magnetic storage components at IBM to join him. However, Ken Lee turned him down.

Charles Lin then talked about his business plan to another researcher in Ken Lee's group, Dr. Atef Eltoukhy. Eltoukhy had apparently joined IBM shortly before that time from USC where he was an assistant professor. Clearly, he had been working on sputtering thin film media development and must have possessed knowledge in this area. However, Eltoukhy apparently had his own ideas about starting up his own company as well. Both Lin's and Eltoukhy's plans were discovered by IBM management and later they were sued by IBM. Charles Lin and Atef Eltoukhy left IBM, and Lin started his company, Lin Data, while Eltoukhy started Trimedia. Ken Lee also left shortly afterwards to join Southwall Technologies Inc. Ken Lee stayed at Southwall for a short time and he joined Domain in order to head up their development of sputtered thin film media. When Domain collapsed, he went to Quantum to become VP of Engineering and had a very distinguished career there. What Ken Lee started for the sputtering process at Domain became part of Conner Peripherals's media process, and it became the main part of Seagate's media operations after they bought Conner Peripherals. In a strange quirk of fate, many people that went from Domain to Conner then to Seagate would eventually end up running Seagate, starting with Bill Watkins who became CEO of Seagate. Others include Jerry Glembocki, Joel Weiss, Kevin Eassa and Tim Harris, to name a few. Tim Harris eventually became the last CEO of Komag before selling the company to Western Digital. Around the time that Charles Lin was working on starting up his company, I was introduced to him by Jim Shir who knew Charles Lin very well. I believe that Jim thought that we should join forces as we were all Taiwanese Americans and that we should try to work together rather than to compete.

After Jim Shir convinced me about meeting Charles Lin, we met together at my house to discuss whether we could join forces or not. I had thought that the meeting was about exchanging technical ideas as to how one might make the sputtered thin film media. I had already prepared my material based on my publications and presentation materials that I had used in my interviews with several companies before, and this was what I showed to Charles Lin. Charles Lin however refused to disclose anything about his approach as he said it was IBM confidential information. What I told Charles was how I would produce the media by using a single layer isotropic CoRe alloy film on the NiP-coated aluminum substrate that I had published earlier, so that we could quickly enter the market. I also told him about the reason for choosing the isotropic media. It was because Ampex's plated media was also isotropic and all the drive companies planning to use thin film media were already designing their drives around the Ampex media. This way we wouldn't have to convince our future customers to redesign their drives to use our media. With this approach we could spend most of our resources on solving the key tribology and corrosion problems that the drive makers were most concerned about at that time with the plated media. However, after he heard what I said, he told me that my media design would not be good enough and told me that he had a better technology for formation of the magnetic film. Not having seen anything from Charles Lin, I could not agree to work with him, and he left my home unhappy. After he left, Jim told me that I may be making a big mistake (with my design concept), and told me for the first time that IBM had discovered that they can make a media with high magnetic hysteresis loop squareness with over 90% squareness ratio along the circumference of the disk, and low squareness along the radius of the disk, much like the way oxide media were constructed using a magnetic field to orient the oxide particles during curing. He also told me that this "orientation" effect came from circumferential texture on the NiP substrates, and that it was based on the Cr underlayer. This was the first time that I heard about the so-called "oriented media" for sputtered thin film magnetic media. Jim speculated that Charles was going to use this technology to start his company and because of that, Jim thought that my approach would be inferior to Charles's and we would be disadvantaged right from the start. This then, was the main reason why Jim was so anxious that we should join forces with Charles Lin.

My reaction to this new method of creating the "oriented media" was to stay as far away from it as possible. I just would not have anything to do with it. There were several reasons, and the passage of time makes the reasons easier to state more clearly. First, I could not understand how this effect of magnetic orientation could occur, based on circumferential texture, on the NiP substrate. Having heard only a circumstantial story and also second or third hand at that, I thought that the mechanism for how the orientation occurred was not yet well-understood. I would be proved correct on this later, and in fact it would take years to understand and control this effect. I also guessed correctly that a film system that involves a Cr underlayer, and probably epitaxy between the Cr and the Cobalt films would require a very high vacuum sputtering system in order to prevent oxidation of the Cr underlayer. I did not think that commercially available mass production system could achieve such a vacuum, at least on a consistent basis. My CoRe or CoPt alloys that I wanted to use did not require high vacuum. In fact, I was able to obtain good magnetic properties using a vacuum system that used only diffusion pumps, which by today's standards have fairly lousy vacuum. I knew from my work at PARC that growing a Cr underlayer structure would be quite difficult to do. The key issue was that I felt very strongly that one should not risk a start-up on something that one did not know much about and on something that probably required a lot more work and inventions to bring it to manufacturability. On this point, I would also be proved correct.

The second reason for my objection to working with Charles Lin had to do with the suspicion that he intended to use IBM confidential information for his start-up. It was not clear to me how this issue was going to be dealt with therefore, it was a valid concern. I had taken the proper path of obtaining an agreement from Xerox PARC on IP so that I would not have any issue with Xerox. In fact, our venture capitalists insisted on it. IBM was quite well-known in the business to jealously protect their IP therefore, one had to be careful to avoid being in IBM's sights. In fact, a short time before the meeting with Charles Lin, I had a job interview with Cybernex, a company founded by several former IBM people to make thin film heads. At that time period, thin film head technology was the crown jewel of IBM San Jose, and Cybernex was being sued by IBM over their IP. It appeared that Cybernex was thinking of diversifying into thin film media, perhaps as an insurance against the litigation and they wanted to talk to me about media. They did in fact offer me a position, but their offer was quite poor. More to the point, it was in my opinion downright insulting with my background and experience, and I turned them down. In any case, Cybernex was completely shut down by IBM shortly after we started up Komag. As a punitive measure, key people at Cybernex were prevented from working on heads for many years as part of the settlement. Therefore, IBM was rightly feared for protecting its IP. As I mentioned above, shortly thereafter IBM sued Charles Lin and Lin Data, and Atef Eltoukhy and Trimedia, over misappropriation of confidential information.

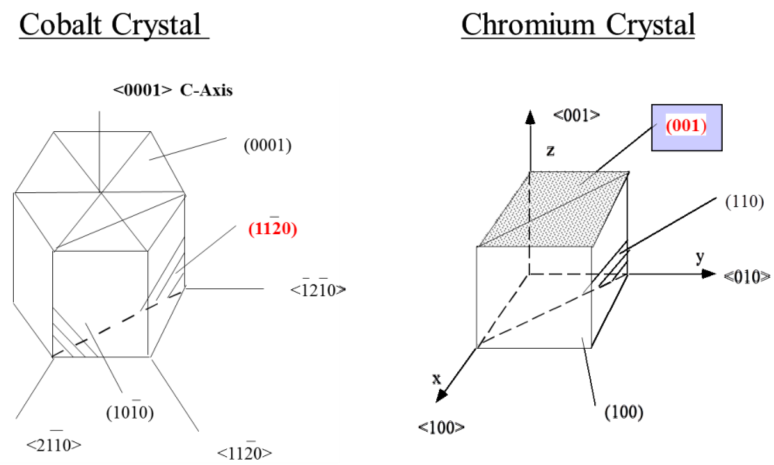
The third reason had to do with pride. I have often been accused of having too much of it at times, but I knew what I wanted to do with sputtered thin film media. I had personally worked on various aspects of its technology for many years, and I had utmost confidence in what I knew and what I planned to use. Charles Lin on the other hand clearly had not worked on thin film media, and he certainly did not invent this new "oriented media" that Jim had told me about. Therefore, I immediately took issue with his opinions as far as goodness of the media was concerned. In fact, I had been told that the orientation effect was discovered by Eltoukhy, but this was also second hand information. The origin of the "oriented media" is still quite mysterious to me to this day. Eltoukhy never took credit for it, even many years later when it should not have mattered anymore.

I think the key point about my strategy was that I would quickly get to the market with a product and establish the business first. It was very much like Al Shugart's Seagate, where the product was designed with what was already well-known and manufacturable. The effort for the start-up should have emphasized just a few key things that needed to be developed so that differentiation could be achieved quickly. Other start-ups such as AIM who tried to make both the drive and media proved to be too difficult and they could not come up with a product in time. As I mentioned above, I was of the same mindset as Al Shugart on how to start a company. In order to successfully start a company, you had to go with what you knew. It was difficult enough, as I found out, to start up a business even with a good level of expertise and knowledge of technology. Trying to do something that you didn't have much experience in was a recipe for failure. As things turned out, both Lin Data and Trimedia did not survive for too long before they had to be sold. Lin Data was sold to Nashua in 1987, and Nashua was taken over by Stormedia in 1994. Stormedia lasted till 1998. Trimedia got started in mid-1984, was sold to Xidex in 1986, then to Hitachi Metals in 1988 and became a subsidiary of Hitachi Metals as HMT Technology Corp. They went public in 1996, and merged with Komag in 2000.

Selection of Media Structure and Sputtering Methods

My work on sputtered thin film media at Xerox PARC was based mostly on single layer CoRe films. I was able to obtain relatively high Hc using a sputter system with a relatively poor vacuum. Therefore, I had the idea that it should be easier to develop working media for manufacturing based on this design as compared to other ideas. There was already work going on elsewhere, especially in Japan to use CoNi and CoNiCr with a Cr underlayer to obtain a relatively good coercivity of around 600 Oe, but as mentioned previously, my work on the Cr underlayer suggested that obtaining good epitaxy on Cr would be difficult unless one had a very good vacuum. This structure involved a crystal structure of Co, which had an in-plane isotropic media structure. In this case, the hexagonal grains of cobalt had their c-axis direction of the crystals (labeled as "<0001>"as a direction) all lying down in the plane of the film. This became possible with the Cr grains having their (001) crystal plane lying on the plane of the film. My media structure on the other hand would be called "3D isotropic" meaning that the c-axis of the Co grains would be completely random in all directions. The crystallography of cobalt, which has hexagonal structure and chromium with cubic structure, is illustrated in Figure 5-4 on the following page.

I felt that my 3D isotropic media would be a natural progression from the plated media, which had the same type of 3D random crystallographic orientation of the magnetic film. I also knew that I could use the commercially established NiP polished surface "as is" and provide good isotropic magnetic film using my single layer concept. The importance of time to market was always paramount to my thinking. Just as successful drive companies such as Seagate Technologies used well-established and currently available technology for the ST506 drive, I thought of my business in the same way. Therefore, I had a pretty good idea as to what type of film and film structure I should be using when I left Xerox PARC.



Crystallographic notation for cobalt and chromium lattice structure. To obtain epitaxy between cobalt and chromium crystals, (001) planes in chromium would be mated with $(11\bar{2}0)$ planes of cobalt. (Figure 5-4)

(Reference 5-1)
http://www.computerhistory.org/revolution/inputoutput/14/347/1520?position=0
Alto I CPU with monitor, mouse, keyboard and 5-key chording keyset
© 2006 Computer History Museum
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http://commons.wikimedia.org/wiki/File:IBM_2315_disk_cartridge.agr.jpg
IBM 2315 Disk Cartridge
Creative Commons Attribution-Share Alike 3.0 Unported license

(Reference 5-2)
http://www-sul.stanford.edu/mac/parc.html
Making the Macintosh, technology and culture in Silicon Valley

(Reference 5-3)
http://en.wikipedia.org/wiki/ST-506

Chapter 6

Getting Komag Started

Starting Shop

Once the company was incorporated, Art Spinner made his $250,000 available, and I started to draw salary at half pay. I also got some stock options. An old friend, Winston Chen, who had started a company called Solectron, provided me with a room at his company in Milpitas on Fortune Drive for free. We rented a desk and a chair, and W. Ferrell Sanders spent a week re-writing the business plan. By the end of June however, both Jim Shir and Scott Chen still had not joined the start-up. They were identified as SC and JS in presentations to the lead venture capitalists who were starting to ask who they were. The investors started to say that they couldn't support the investment unless there was a commitment from the two to join. Both Jim and Scott said that they couldn't join unless the money was committed, but the investors would not commit the money unless they joined first. It was a dilemma. I told Jim and Scott that they must quit IBM and join Komag by July 4th as demanded by Art Spinner. Jim Shir quit IBM accordingly, but one month short of qualifying for IBM retirement benefits. Scott Chen on the other hand was still waffling. Scott's wife was worried what the start-up would do to Scott's health that she was the one preventing Scott from making the move. I had sold Scott so much to the investors that if he were not to join, I would be in big trouble. Art Spinner was also worried and he started to recommend someone else to replace Scott. All of us, me, my wife Nancy, Jim and his wife went to visit Scott and talk to his wife to try to convince her. Finally after a couple of weeks, Scott quit IBM and joined. Each of us in one way or another was risking everything to get involved in this start-up and we had to succeed. Venture capital also would not have it any other way. It was their money and they wanted total commitment to the effort from us.

There were further complications. I was expecting funding from Robertson Colman & Stephens but they were involved with Seagate and were the underwriter for the Seagate IPO. Seagate had invested in Grenex and Robertson Colman came to see us to see if we would consider joining forces with Grenex. The fact was that I had been approached by Grenex while I was still at PARC and I knew all about them. They had started out focused on plated media, and they sought my expertise at the urging of Gordon Hughes who had moved to Seagate from PARC. It was actually Gordon Hughes who wanted me to interview with Grenex. By the time Robertson, Colman & Stephens came to talk to us about Grenex, they were now supposedly interested in sputtered longitudinal media. They were literally all over the map. In any case, Seagate was behind the fact that Robertson Colman was now appearing to be backing out from investing in us. I felt betrayed. We already had spent $250,000 of Art's money and we were counting on $1 million from Robertson Colman. This time period was very difficult for me because the money already promised still had not yet come in. Robertson Colman seemed to be backing out and Hambro money was only from the initial $1 million promised from Art Spinner. I still had to get things started, so we were already talking to various vendors, making commitments for purchasing equipment and renting a new space in Milpitas so that we could begin the prototype work. We had the excuse of being under Art Spinner's pressure and we refused Robertson Colman's push to join forces with Grenex. To their credit however, Robertson Colman actually kept their promise and finally provided $1 million to our venture.

By August, we moved into a temporary development facility in Milpitas, located on Valley Way Street near Highway 880. We had started to get some tools into the building and hired a few more people to start the work. I hired Ron Allen, who had worked for another researcher at Xerox PARC, and he would be the key equipment guy to deal with the new sputtering tool. I also hired my technician of many years at Xerox PARC, Bruce Charlan. We gave them some stock options to make up for leaving their stable jobs at PARC. A VSM (vibrating sample magnetometer), SEM (Scanning Electron Microscope), and S01 sputter machine were also installed. The S01 tool was just like the small tool I had at PARC. PARC was kind enough to let us borrow the sputtering chamber and some sample holders that I used there. They weren't using them anyway. Things were quite primitive in the new location. It was basically a warehouse, more suitable as a garage shop than a development facility. We didn't have enough power at the right places, and didn't have water to cool the tools such as the sputter machine. We used some garden hose to plumb water from the bathrooms to run to the tools. If California's occupational safety and health administration (CAL OSHA) had come in to inspect us, they would have been horrified and probably would have shut us down. There were other distractions during our fundraising efforts in August. Jim Shir brought back the idea of working with Lin Data again, and we met with Charles Lin at his house this time. We still had a difference of opinion as to how to proceed with the technology; I had to say no again, this time for good.

Since W. Ferrell Sanders was our acting president, we needed a permanent president to deal with various activities related to raising money from the investment community. We had to organize, setup financial and HR systems, and be able to deal with various regulations. Ferrell started to recruit for the position, and through a headhunter we narrowed the candidates down to two people. One guy was a strong semiconductor operation guy, but Art and Ferrell thought that he had too strong a personality to work with me. Knowing all too well now that I too had a very strong personality, Ferrell thought that it might not work out so well with this guy. The other candidate was Steve Johnson. He had excellent credentials and was quite personable. We thought that his personality would be much better matched with mine. So Ferrell made the offer to Steve Johnson. Ferrell brought Steve to my home, where along with Jim and Scott, we could talk and get to know each other. We discussed how we were going to organize ourselves. I was to become the chairman, Scott was going to be vice president of operations and Jim was going to be the CTO. Steve Johnson wanted CEO and title of president. We all had to take a big cut in salary when we joined the venture, but Steve Johnson wanted more salary. He also wanted a new car as well. We were already starting to have our cultural differences in how we should run the business. Steve was also very careful and wanted to see more of what we were planning before he would sign-up. On September 9, we finally completed our initial fundraising and we got $5.5 million, against the original plan of $3.5 million. We actually had offers of up to $7.5 million, but Art decided that it would dilute the ownership too much. $5.5 million was a good figure as we wanted enough money to have as a cushion, just in case.

The day after obtaining the $5.5 million commitment, I headed to Japan to meet with ULVAC to have a session with them on sputter tool design. The trip was actually planned for earlier, but was delayed due to the fact that Steve Johnson wanted to come along to see for himself what we were doing with ULVAC, before he would sign on and join us. The meeting with ULVAC took place in marathon sessions from September 12 to 14, and finally at the dinner hosted by ULVAC on the 14^{th}, Steve announced that he would join once we returned back to the U.S. This was quite an exciting conclusion to the ULVAC visit, as we needed a new president in order for the venture funds to start releasing money to us.

For our first round of funding, we got support from Hambro, Robertson Coleman & Stephens, Merrill Pickard Anderson & Eyre, Max Palevsky, Venrock Associates and Continental Illinois Venture Corporation. Of all the investors, the most interesting one was Max Palevsky. Art knew Max from previous dealings, and he asked Max to join, as Max had his own venture fund that pooled his own money and that of his Hollywood friends. One day in July during our fundraising period, Art asked me to pick up Max from San Jose Airport and bring him to our office to make the sales pitch to him. When he showed up at the airport, he was wearing pink sunglasses, a colorful shirt with an old leather jacket and carrying a beat-up old leather briefcase under his arm. I almost did not recognize him, as he looked very different from the time that I saw him once at PARC. I took him to our office and made the sales pitch about Komag. He gave us an hour because as it turned out, he was also going to Akashic to hear their sales pitch after us. Next day, Art told us what Max thought about the presentations from Komag and from Akashic. Max was going to invest $1 million into the thin film start-up, but he was going to split the money equally between Akashic and us. He told me that his reasoning was that if the technology was relatively easy to implement, then he thought Akashic would be more successful because their team, headed by John Scott, had more mass production experience. If the technology proved to be more difficult, then Komag would have a better chance because of our knowledge and technical capability. After one year when we showed off our prototype disks from the S1 sputter tool, Max was so impressed that he put in another $1 million during our 2nd round of fundraising in October 1984, and asked to be put in as a board member. We were so delighted to have Max on our board. It turned out that he was a great board member, and contributed significantly to the success of Komag, until he retired in 1998. I really enjoyed working with him because he had vision and insisted on having a very meticulously prepared business strategy.

Designing the First Production Sputter Tool

Having the right tool to sputter media was probably the single most important factor in whether we would succeed or not. So I put an enormous amount of time and attention into this matter. As mentioned earlier, there were no ready-made sputtering tools that met my specifications that one could buy "off the shelf." It would have to be custom-built. But what are the key characteristics of the machine? How to make it production ready? And who will build this tool? These were very critical questions. I also did not know much about production-level vacuum sputter tools as I worked only on small research-level tools before. In the research lab, I never really thought much about such things as machine uptime, number of disks made per hour, or cost per disk, for example. So when I decided to start the company, I had to become more familiar with the vacuum deposition tool for manufacturing. While I was still at PARC, I had to search for a machine that would address the issues that I thought mattered in a production environment. I looked at various systems made by companies such as Leybold and CPA as well as other systems made by smaller companies, but none really fit the bill. So the system required some modifications, and I thought that Leybold and CPA had some promise. When I talked to them, I asked for a promise that they would keep those modification ideas confidential and protected for myself. They refused however, and I needed to find a company that would be willing to work exclusively with us. To be fair, I had no credibility with them and they had no reason to accommodate my demands regarding confidentiality. I attended the SEMICON convention while I was still at Xerox to try to learn more about the equipment market. SEMICON is held every year in San Francisco to cater mostly to the semiconductor industry, and it continues to this day. At the conference, I met an ULVAC salesman, Mr. Suzuki. Then Suzuki-san sent Peter Ducza who was the US sales agent, to talk to me at Xerox. Once I started Komag, I called on Peter again. Peter was concerned about our credibility and told me that ULVAC might not deal with Komag and me. But around that time, it turned out that ULVAC was heavily engaged with IBM in San Jose to design and build a huge and sophisticated sputtering machine for a thin film media process. The man in charge at ULVAC was Mr. Takei and he was coming to the US to visit IBM along with Mr. Suzuki, and also his right hand man, Mr. Minami. In order to do business with ULVAC, Peter told me that I must meet Takei-san face-to-face and show him that we are a credible start-up company. Luckily, they agreed to meet us at our humble office in Milpitas. I brought a teapot and cups from my home to the Milpitas office. They showed up in the early morning, and I served them the tea myself. Takei-san really enjoyed the tea and we had a good talk. I showed him my many publications and gave them a copy of my publications related to magnetic and MO memories, and told him about my ideas. Both Takei and Minami-san showed that they were very interested, and that they would have to think about it. Minami-san told me later that Takei-san gave the publications to him and asked him to review them with people that he thought were experts in the field, before deciding on whether to do business with me or not. In the evening, I offered to have a dinner with them, and picked them up for a nice Chinese dinner. Takei-san told me that it was the best dinner that he had in over a week, because he was getting very tired of the western style food that he had been eating. The dinner may have been the most important event affecting his decision to work with us. They had to leave for the airport after the dinner so I drove them to the airport myself. They promised to come back in August with a plan for the system they could discuss with me. The meeting with Takei-san proved to be momentous. He was a crusty and hard-boiled veteran at ULVAC, and he had built many large and complicated vacuum tools over the years, many of them one-of-a-kind. He had a tremendous amount of experience in designing and building vacuum tools, and had a group of young and energetic engineers that worked for him that he was training to be the new leaders for ULVAC. Takei-san could make the decision on whether to do business with us or not, and his word was a contract. He decided to gamble on me, and on Komag, to take the job of designing a new tool for us to my specifications. I believe that he put as much effort and time on our tool as he gave to IBM. I was very fortunate in having met Takei-san, and that he decided to take a big chance on Komag.

In August, ULVAC came back with a machine proposal. It turned out to be an in-line sputter tool, similar to what they were building for IBM at the time, with some modifications to the disk carrier. This design called for using giant DC magnetron cathodes, which were very expensive, and the price tag for the machine was between $3 million and $4 million. Unfortunately, Takei-san's first design proposal for the machine and disk carrier would not work for me. I made a request to Takei-san to have a detailed design review meeting in Japan after we raised the money for starting up Komag. On September 10th, Steve and I went to Japan and we had a marathon design review session at ULVAC which lasted three days, as described earlier. I frankly told Takei-san that his proposal would not work for us. I outlined the issues we had with their proposal. Firstly, the machine was too expensive, and secondly, the DC magnetron cathodes that they proposed were quite impractical because this method of sputtering depended on using strong magnetic fields above the target to enhance the sputtering rate. The material, which we wanted to sputter, was itself magnetic; therefore it would shunt the magnetic field from the cathode. Therefore only very thin targets could be used, which turned out to be about 2 mm thick at the most for the cobalt alloy targets. It meant that the target has to be replaced very frequently, probably every day by my estimation. ULVAC claimed that a 10 mm thick target could be used, but this required that the targets be cut with many diagonal slots across them so that the magnetic field from the magnetron can penetrate through the target. Such techniques looked very forced and risky, and it would also make the target very expensive to fabricate. Additionally, the system proposal included many cryopumps, which was a relatively new technology at the time and also very expensive. The ULVAC proposal was probably based along the same design rules they were using for IBM at the time, and the design was hardly practical and much too expensive for what we could afford. What I had in mind for the cathode was to use RF (radio frequency) sputtering, with which I had become familiar at Xerox. With RF, I could consider using thick targets that are 1 cm thick or more, and additionally it should be possible to obtain high utilization from the target because it should be possible to erode the targets uniformly across the whole surface of the target with a properly designed ground shield. What I had in mind was to use a Co-Pt (cobalt-platinum) alloy for the magnetic layer, which was very expensive due to the platinum. The target would be also expensive to fabricate, and I needed the target to last a long time in the sputter system. With the DC magnetron cathode, the utilization of the target is quite poor. During those days, for non-magnetic metal targets, 20% target utilization (and only half of that for a magnetic target) was considered very good for DC magnetrons. I needed to get to 80-90% target utilization in order to be able to use Co-Pt alloy, and RF sputtering was the only method that could achieve such numbers. This was a critical piece of knowledge and strategy; therefore I had a special NDA (Non-Disclosure Agreement) signed with ULVAC wherein they promised to maintain secrecy for the life of the machine model, so that we could discuss a system design based on RF sputtering. The RF sputtering the way I wanted to practice it required relatively high sputtering pressures compared to DC magnetron sputtering. This was a critical step as we now could settle on the machine vendor and focus on solving the problem of sputter tool process. To create a magnetic film with isolated crystalline particles, I was looking for a sputtering pressure of around 50 mTorr, while DC magnetron sputtering needed pressures in the low teens of mTorr or less. This meant that the sputtering tool had to be designed differently to accommodate my process. Jim Shir was busy during this time period talking to target vendors and trying to estimate what the target utilization would have to be in order to get a reasonable per disk target cost for Co-Pt alloys. One big challenge for RF sputtering was that the production machine would have to sputter both sides of the disk simultaneously. This was an area that I had no experience in. RF deposition is very tricky to implement. Running several cathodes on opposing sides and in a series with cathodes right next to each other was even harder as the cathodes would interact and interfere with each other. Luckily Takei-san had experience in RF deposition. He suggested that RF on both sides of the disk could be triggered by use of a device called a “common exciter”, which fired both cathodes in a pre-determined phase relationship. Otherwise the various RF fields would interfere with each other and it would be very difficult to maintain a stable plasma on both sides of the disk. The other known disadvantage for RF sputtering was that the sputter rate was low compared to magnetrons.

Each disk would take longer to coat for a given thickness, so that the machine would be running slower than if the magnetron cathodes were used. One would have to make up for this with larger cathodes and running the disk carriers slower through the machine while maintaining a higher utilization rate for the tool. Jim did a careful economic analysis for what the sputter process had to deliver and this formed the basis for the system performance requirements that we gave to ULVAC.

Our “budget” for the entire sputter system was $800,000 and extra for the panels and carriers to hold the disks through the system. This was a very challenging price. We could not afford fancy pumps (such as the cryopumps) and decided on a Roots blower pump so that we could maintain high gas throughput and high sputter pressures of 50 mTorr or more during sputtering. One critical issue was how to pump water vapor from the system, which the expensive cryopumps excelled at. The inexpensive compromise was to use a cold trap, which is essentially a liquid nitrogen-cooled plate to freeze the water vapor from the chamber. We agreed on pricing and design by September and ULVAC went to work.

One key problem that developed afterwards was the realization that we had to use DC magnetrons for sputtering a carbon overcoat on the disk. I had not originally settled on carbon but we carried out work on overcoating using the S01 sputter tool. A graphite target was needed to sputter the carbon overcoat, and we realized too late that the sputtering rate was much too low for carbon, even using DC magnetrons. Our design criterion was that the system operates at a 4 minute panel cycle, meaning that a panel containing eight 5.25-inch diameter disks would enter and exit the sputtering system every 4 minutes using load lock chambers on both ends of the sputter tool. The total throughput then would be 120 disks per hour. The panel holding the disks would travel slowly at a steady speed across the cathodes to get the sputtered film deposited on the disk. The sputter rate was important because we had to put down the required thickness of film during the time interval that the disk was passing by the target. What had happened was that with the RF cathode, the rate for sputtering carbon was too low to make it through in the 4 minute panel cycle. This was a big miscalculation on our part. We actually didn’t know about the carbon deposition issue when we designed the system with ULVAC. In order to deal with this carbon sputter rate problem, Minami-san who was working under Takei-san, suggested that we must use DC magnetrons for carbon. Not just one but two pairs of them! Worse yet, we had just designed the system to run RF at 50 mTorr and DC would only work at low pressure, typically under 10 mTorr. So how to achieve two very different sputtering pressures within the same system chamber? One way was to put in another load lock chamber in the middle of the system to deal with the different sputtering pressures, but this would have added significant cost and complexity to the system. So again, ULVAC came up with an interesting design proposal of placing a differential “slit” inside the middle of the system, where two different pressures can be maintained within the same chamber. A slit is basically a very narrow set of apertures placed between the two chambers which is just enough for the panel and carrier to go thru. Because the space between the slit and the panel and carrier is very narrow, the gas inside of the system will have a harder time flowing between the two sides of the slit, thereby allowing two very different pressures to be maintained on either side of the slit. This was a very good proposal and innovation on the part of ULVAC. Nevertheless, the addition of two pairs of DC magnetron cathodes and additional pumps on either side of the system along with the slits raised the price of the system up to around $1.2 million**.** We had no choice, but it would only add an incremental cost to the disk. These modifications were made in early December, and caused some delay in the construction and delivery of the machine.

Magnetic Layer Development

From mid-July of 1983, we had started development activities on the first R&D tool, which was the "S01" tool. This was basically a copy of the tool I used at Xerox. We also ordered an S02 system that was much larger and which could lay down three different films and make 3 disks, one side at a time, in each load. This tool could accept an 8-inch diameter sputter target, which was big enough to cover the 5.25-inch diameter disk. The S02 system was purchased from a local company called Comptech. We also had a similar machine at Xerox. We also contracted R.D. Mathis to design the special RF network to work with the RF cathode. They would also be hired as consultants to work on the S1 production tool. Don Hall, who was the co-founder of R.D. Mathis, was also someone I knew from my days at Xerox and Northrop.

Having the S01 sputter tool was very critical in many respects. Even though it could only sputter one film at a time, it could sputter a target as large as eight inches in diameter in its tiny chamber. One of the most critical pieces of data we needed was for developing the magnetic alloy. At Xerox, I worked extensively on CoRe alloy, and published a considerable number of papers on this alloy film. Re (rhenium) was much less expensive compared to platinum. However, rhenium had limited capability for generating high coercivity. An alloy with high intrinsic coercivity was essential for future extendibility. The CoPt alloy was more attractive and there were a number of publications that suggested this. One was by Yanagisawa et al at NEC (Reference 6-1) on CoNiPt alloys and Aboaf et al from IBM (Reference 6-2) on CoPt alloys. Aboaf et al had worked with a range of Pt compositions and reported Hc as high as 2,000 Oe depending upon thickness and Pt compositions. Yanagisawa et al added Ni to the alloy and with only 10 atomic % Pt, along with 10 atomic % Ni, he was able to obtain high Hc. Both demonstrated that Co-Pt alloys are attractive candidates for thin film media. The issue was to know the minimum amount of Pt to put in to obtain the desired value of Hc. Our initial product did not require Hc much beyond 600 Oe, but 800 and even 1,000 Oe would be needed very soon. Yanagisawa's paper was of particular interest because he showed that by using a combination of Ni and Pt, the Pt amount could be minimized and yet still obtains high Hc. Using the S01 sputter tool, we worked out various alloy combinations of cobalt, nickel and platinum to determine what coercivity one could obtain for the film thickness we would need for the first product. With a 10 atomic % nickel base, we determined that we could obtain 600 Oe easily with 6 atomic % Pt. This was the starting point for calculating the cost of making the disk using other variables such as target utilization estimates.

To do this type of work, the method we used was to bond many small square pieces of thin platinum sheet on top of the Co-Ni target and sputter them onto the disk. We then approximated the amount of Pt that the film contained, calculated by the total area that Pt covers on the target surface and the amount actually in the film measured by an EDX (Energy Dispersive X-ray) detector in the SEM microscope. RF sputtering was uniquely suited for this type of work because the plasma spread out uniformly over the entire target surface and the amount of Pt could be rapidly adjusted (by bonding more or fewer squares of Pt sheets). Pt squares could be bonded to the target surface using small amounts of low melting point indium between the Pt squares and the target, and pressing on the Pt sheet to bond them to the target. I even enlisted the help of my wife, Nancy, to cut small squares of Pt from the sheet, and bond them onto the target.

When we talked to ULVAC, we knew what we had to achieve in the RF cathode and what Hc should be expected for a given target composition when we went from the small scale S01 system to the large targets used in the in-line sputtering system. Obtaining such data was still hard and took considerable effort. It took us till around October to know sufficiently what we needed on the alloys. By February 1984, we had our 2nd batch sputter system (S02 system) which could sputter actual 5.25-inch media, although only one side at a time. This system had three target positions, so that we could sputter two magnetic layers and a carbon overcoat. This system was to be used to make prototype media that our potential customers could evaluate while we worked to bring up the production ULVAC system. We also started to talk to target manufacturers who could make the magnetic targets for us. The two vendors we initially lined up were Dement Electronic, which was located in Gilroy, California, and Hitachi Metals in Japan. These activities were very important to preparing the production tool for use.

By taking this approach, we figured that we would have a higher probability of overcoming the extremely difficult mass production issues of making usable media with proper magnetic properties faster than our competitors. Consequently, we would have more time to focus on solving the two other critical and even possibly “show-stopper” issues. Those two issues were: (1) wear resistance of the media by a contact start-stop of the head during disk operation (tribological issues), and (2) corrosion resistance of the media in a drive under high humidity and temperature application environments.

The poor reliability of tribology and corrosion resistance that everybody had experienced with plated media in a hard disk drive cast a lot of doubt in the industry on the viability of metallic thin media in a drive, even if it was also applied to sputtered media. As a matter of fact, the old school of IBM hard disk drive engineers were so convinced that metallic film media would never be able to pass their corrosion and tribological requirements that they would kill any proposal for adopting thin film media. That is the reason why IBM did not get seriously into thin film media technology until 1985, even though they had very active R&D on thin film media development. This was happening even after many startup companies such as Maxtor and Vertec Corporation had shipped many reliable drives with sputtered thin film media made by Komag.

Solving the reliability and tribology issues with thin film media was the most important goal for us to succeed. It also represented a real opportunity for us to pull ahead of others in the race to become the first successful thin film media company. It was much more important, and also more challenging than providing good magnetic recording performance, as it turned out.

Carbon Overcoat

Other key data that we recorded was related to the carbon overcoat. I had done some work on this area while at Xerox. There was very active work taking place in the materials science field for creating hard carbon coatings, which were apparently very hard, and also offered corrosion protection. Varieties of methods were used for making the carbon film and there was a bewildering array of terminology used to describe the film. Some were even claiming actual diamond film. By using sputtering, which was the simplest method of obtaining carbon films, the films were amorphous and yet had some properties of diamond by being hard, and some characteristics of graphite in terms of frictional properties. General terminology used for this type of carbon was “diamond-like” carbon film. Initially, we had used a carbon material called pyrolytic graphite as a target to make carbon film. Pyrolytic graphite is made by heating the hydrocarbon or carbon fibers to near decomposition. The graphite sheets then bond with each other, creating a carbon material that is extremely hard and conductive. They are difficult to make, but you can obtain blocks of this material that can be machined into target shapes and sputter them. Apparently this material contained a considerable amount of hydrogen inside, so that they became incorporated in the sputtered film. This is something that we didn’t quite appreciate at the time. We did appreciate the property of the film however. The film could be made thin, and it had very good protective properties. It was not a bad material to put on top of the magnetic films that were very prone to corrode. So when we started to sputter this material in our S01 system by RF sputtering, we realized that the deposition rate was extremely low. In fact the rate was something like 4 to 5 times slower than for a metal. We realized that in order to obtain our target thickness of around 100 Å for the carbon overcoat, we didn't have enough time in the system to get this thickness by using RF sputtering. We had to use DC magnetron sputtering because it would be much faster. Even then, we needed a very powerful DC magnetron cathode with a large magnetic field to maximize the deposition rate. This was what was suggested by ULVAC, and this cathode was expensive because of the large electromagnet that was used to generate the magnetic field and the power supply to supply its power. The carbon sputter rate issue appeared to be a complete disaster for us at the time. However, we were still very glad that we had the data and could respond to the challenge… just in time, in fact, so that proper modifications could be made to the S1 sputter machine.

There were other key development activities during this time period. We needed a disk certifier to be able to test the quality of the media. In those days, there was no vendor that produced such machines commercially. Scott Chen then came up with a design for a 5.25-inch disk certifier. He contracted a guy named John Scott to build it, and by around October, we had our first certifier. Scott Chen took some of the disks that I had made in Xerox to test. We also made disks on our S01 sputtering tool, one layer at a time. Everyone was busy building up the capability to make the media, and also to test it and check the quality of what we planned to make.

We also started to look for a new building to house the company. In November, we signed up to rent a building that was being built at that time and was nearly completed. However in December, just a few weeks before we were to move in, Scott Chen discovered that high-tension power lines near the building were generating a lot of electronic noise. This would affect his sensitive certifiers. Based on these findings and other criteria that we had for housing our sputter tools, we found the building we liked on Yosemite Drive in Milpitas. This building was also under construction, but we were late on our schedule, therefore we rushed the owner into accelerating the construction by paying extra rent so that we wouldn't lose much time. We moved into the new building by mid-February of 1984. Many people were also being hired. The delay in moving into the new building cost us about one and a half months, and it had a big impact because we could not take delivery of key equipment such as the S02 sputter tool and polishing machine. Key hires during late 1983 were Ven Kao, Sonny Wey and Sandy Fitch. Ven Kao was responsible for manufacturing, Sonny Wey for engineering, and Sandy Fitch became our vice president of finance. A few other key hires right after we moved in to the new building were Tom Yamashita from Stanford University, and Steve Miura from IBM.

As soon as we started to move in to our 591 Yosemite Drive facility in Milpitas, California, we started to install the polishing machine, made by SpeedFam Corporation. We planned to purchase plated NiP disks from outside vendors and were actively evaluating parts from companies such as Burton and SAE. All these companies were a mixed bag. Some were only garage shop operations, while others were involved in plating automobile parts. There was a lot to learn on both sides. A clean room was being installed into our Yosemite Drive facility, and a de-ionized water system was put in place in March. In mid-February, we took delivery of the S02 sputter tool. By March, we started to sputter films on 5.25-inch disks. We did not have the waste water system installed at the beginning so that we had to resort to dumping wastewater into the toilet. We still didn't have a suitable disk cleaning system, so we had to wash the disk by hand one at a time before putting them into the sputter tool.

Roughening the Disk Surface

It was first necessary to polish the substrate surface to a near mirror finish so that the surface was very smooth and free of any defects. This was necessary in order for the recording heads to fly very close to the disk surface. The recording heads were designed with air bearing sliders which sucked air under these slider rail surfaces so that the high air pressure that developed under these sliders kept the heads off the disk surface. The sliders were literally flying over the disk surface. By having the head suspended under a metal spring (suspension), the force of the suspension pushing the head into the disk, and air pressure developed under the slider pushing the air bearing up, created an equilibrium which kept the head flying at a constant height over the spinning disk. This worked while the disk was spinning, but when the disk stopped spinning, the head would slide back on to the disk surface unless one took steps to move the entire head off the disk surface by some mechanism. By 1984, Winchester technology was quite ingrained into disk drive design, so that all new disk drive start-up companies were using this approach. For small computer system applications, such as the PC, the drives were expected to be turned on and off quite frequently, so that it would have to survive many of these contact start-stops. So the industry termed this requirement "contact start-stop" or "CSS" for short. The media and the head had to survive many sliding start and stops over the life of the drive. The typical requirement was around 20,000 times. That would be equivalent to turning on and off the drive 5 times every day for 10 years!

Passing the contact start-stop requirement was actually very challenging. Clearly the lubricant was necessary to reduce friction between head and disk, but applying the lubricant to a very smooth surface caused another problem, which was termed "stiction" by industry people. Stiction is the breakaway force needed to start sliding the head over the disk. If a lubricant liquid filled the interface between the disk and the head, one can have a jaw-block effect that binds the two surfaces together. One can easily see this phenomenon, for example, if one places a smooth piece of glass over another very smooth surface, separated by a thin layer of water. The two surfaces stick together and it becomes very difficult to slide the glass over the smooth surface. The problem disappeared if one had plenty of liquid between the surfaces, in which case the surfaces would easily slide with each other. But for the head-disk interface, we could not flood the interface with liquid because in that case the head would not fly well over the surface. As soon as we made the lubricant thin enough and the surfaces were very smooth between the disk and the head slider, one obtained this jaw-block effect and the forces were so large that the head would be torn away from its suspension. In order to prevent this from taking place, we had to make the disk surface slightly rough, enough to break this tendency for the jaw-block phenomenon to take place. We wanted to do this in the most convenient and cost-effective way possible, and also not make the disk surface so rough that it caused its own problems such as increasing friction or causing defects. Also, if the surface became too rough, the head would not fly stably and would cause vibration that added modulation to the recording signal. We finally figured out how to do this by introducing a slurry of slightly rougher size at the very end of the polishing cycle for the substrate. The most difficult part of doing this type of development was the fact that the measurement tools needed to determine roughness and control the process were barely adequate for the job. Many new tools were developed by us and by the industry to support all of these new processes. With this roughening process now applied to the substrate, and a new overcoat made of sputtered carbon and new lubricant, we were able to meet the contact start-stop requirement the disk drive industry demanded. This entire field of science relating to friction is called "tribology." The hard disk drive industry uses some of the most sophisticated tribology known to science. We had a lot to do with making tribology of thin film media really work in the drive and I believe that it was the single biggest factor that contributed to our success in getting Komag off the ground. It was also the most difficult technically. Jim Shir, with his knowledge of mechanical engineering and experience working in this area at IBM during the height of IBM's 3380 drive reliability issue, had considerable experience in this area, and his contribution to solving our tribology issue with the media was very large.

Lubricant Development and Tribology

On the topic of lubricant development and demonstrating the tribology performance of our media, I should mention some more important background information regarding tribology and reliability. I have discussed already the importance of the IBM 3340 drive, which used Winchester technology for the first time. In essence, it involved the use of a lubricant on the disk, using a small and lightly loaded head that would land and take-off from the disk surface by sliding on the disk surface. This has led to dramatic simplification in some aspects of drive design, and indeed, it is the key contributing factor to miniaturization and the building of 5.25-inch drives. However, it put a considerable new burden on the reliability of the disk in being able to withstand the repeated take-offs and landings. In addition, the heads were generally flying much closer to the disk than before, which added to the reliability burden as some unique new failure modes started to be observed that were related to having two surfaces (between the head and the media) flying very close to each other at high velocity. New tests and tools to test media and heads were being developed at that time to deal with all the problems that drives were seeing, often in the field. One of the tests that the media was expected to pass was the 20,000 Contact Start Stop (CSS) test. In addition to the CSS requirement, there was an added requirement that friction force and stiction must remain low throughout this test.

In designing the HDD for PC applications, it was important to fit the drive into the small enclosure that was available (floppy drive bay) and also limit the power consumption as much as possible so that it would not put undue strain on the available power supply in the PC. It meant that only a small motor could be used. The amount of torque available to start up the motor was low, so that CSS (landing and take-off zones) would be put at the innermost inner diameter of the disk to reduce the torque required to start up the drive. Also for the PC, the drive would be turned on and off repeatedly, often many times in a day. This would put considerable strain on the disk CSS zone, and the media surface tended to wear out and lose lubricant over time. During CSS, the friction and stiction force would gradually increase. To reduce the stiction, the disk surface was roughened to reduce the contact area between head and disk. However, the roughened surface would gradually wear out at the CSS zone after repeated starts and stops. Drive customers therefore placed a requirement that media must withstand 20,000 cycles of start-stop, with the maximum stiction force of 5 grams. One can always design a drive with a higher torque motor to accommodate the higher stiction, but if the stiction was high enough, the head would be torn-off from the delicate suspension arm and this would completely destroy the drive.

Some of the difficulties we faced with the reliability requirements from drive manufacturers were that test instrumentation was relatively immature and could not be easily bought. In most cases, it did not even exist. Customers were no help either. Drive customers themselves were still trying to develop the drive, and we could not obtain drives to do testing with. It would have taken too long to test with the drive in any case, as we needed an accelerated testing method. In addition, we needed to accurately measure the friction level between the disk and the head and also the stiction, the breakaway force to start sliding the head. Strictly speaking, the value of stiction is the tangential force needed for breakaway over the normal force, which is the suspension load of the head to the media. It is, therefore, a dimensionless ratio. In 1984, it was not possible to easily measure stiction as it required very specialized sensor arrays based on strain sensors (piezoelectric sensors) that could be calibrated to accurately measure the forces on the head. In this area, it was Jim Shir that set up the instrumentation and accelerated testing methods to evaluate our media and provide feedback to all of our work on tribology. Jim Shir was quite involved in the IBM 3380 reliability issue in 1981 when they could not ship the product for 18 months. Jim, being a mechanical engineer, had considerable familiarity with various aspects of media design and tribology testing as it related to Winchester design technology, and we could not have gotten Komag off the ground without his expertise in this area. Jim set up a tester to do the accelerated CSS test. There was also a very specialized piezoelectric strain sensor that he was able to obtain for the CSS tester, which was an octagonal copper ring with strain sensors attached to it that made it very sensitive. This gauge could measure the stiction level between the head and media very accurately but it required special electronics and careful calibration. The circuitry was based on a Wheatstone bridge and it was very sensitive at detecting stiction and friction forces. Jim had taken care of all of this, and the tool was ready when we started to make the media. Without this tester, we would have been quite blind as to how our media actually would perform in a drive. I cannot overstate the importance of having this tool in the development of the media at Komag. I believe that to be able to take accurate measurements of tribology performance on media was a major competitive edge for us.

When the IBM 3380 drive got into reliability issues, it was apparently due to wear between head and disk, in turn caused by a lubricant change that was made on the media. The reliability issue was something that would not manifest itself until about 3 months of operation, which made it very difficult to detect and deal with. IBM being a company that had a considerable reputation for reliability would not ship the drive until they were very sure of the solution; therefore it took a long time to correct the issue. Although many IBM alumni founded new drive start-up companies and were keenly aware of these types of issues that can happen to Winchester drives, they were not so thorough in their reliability testing. It is interesting to note therefore, that many drives that were shipped with plated media by these drive companies from 1982 to 1984 did not even have an overcoat, and they used rather questionable lubricants directly on top of the metal coating. Consequently many of these drives built were very unreliable and doomed to fail. It was noted later by Maxtor founder Jim McCoy (Reference 6-3) that the initial 10,000 drives they made were shipped with Ampex plated media. Every single drive failed in the field and had to be replaced within a year. He goes on to say that start-stop testing in those days was something of a “black art” and they didn’t know that media might fail, and suggesting that the test was not done because Ampex did not warn them of the issue. I also mentioned that SyQuest claimed to be the first to ship plated media with a carbon overcoat, and that it used a PTPF lubricant (AM2001) in late 1982. However the SyQuest cartridge had problems with reliability in the field as well and it took them year or more before they could produce cartridges with reliable performance. It turns out that IBM put in considerable effort on plated media and had considered using it on IBM 3370 and IBM 3380 drives. They experienced severe problems with reliability however, such as catastrophic crashes and wear. Unable to correct these issues, they reverted back to oxide media. The type of asperity issues that I had experienced at PARC with plated media were no doubt still there with plated technology when others were trying to use them. IBM, being a careful company that would not release unreliable products, ultimately decided not to use plated media in their 3370 and 3380 drives. They had very good reasons for not using plated media. The new drive start-ups that were trying to make 5.25-inch drives with thin film media either didn't know about these types of issues, or were simply willing to take much more risk. In some respects, it was because these companies were taking such risks that the business for thin film media got started. Reputable and well-established companies weren't going to take such chances. IBM HDD engineers had every reason to be skeptical about the reliability of thin film media because they actually tried to use it so many times and had seen some very severe problems.

Finally, to get to the point, by July 1st of 1984, we had to deliver the first prototype media to prospective customers to pass the 20,000 CSS test requirement. With Jim's CSS tester, we weren't getting to even half of that before the disk started to fail. I don't think anybody else's media could pass the test either. Some competitive disks that we had tested were indeed much worse, failing within a few thousand cycles. The drive customers acted like they had media that would pass the test, but the reality is that they either didn't know, or they were relying on unreliable data from their other media vendors. In the case of Maxtor, they knew that they had major problems with the media that they were using, but they didn't necessarily want to tell us. In many respects, the small form factor 5.25-inch drive was an even bigger problem in terms of CSS and reliability compared to the big drives such as an IBM 3380. Starting with the motors, the small drive didn't have the expensive high torque motors the large drives had as there wasn't enough money (budget), space or power available to use such motors. Consequently the start-up and wind-down times were much longer in these smaller drives, which meant that the heads would be dragging on the media much longer. They also lacked power so that even slight increase in stiction meant the drive could not start-up. The drive environment was probably much dirtier as manufacturers would not put the very expensive filters or air recirculation systems that large drives had. Things tended to build-up inside the drive and contaminate the head disk interface.

Our media had a single magnetic layer with about 10 nm of sputtered carbon overcoat. The lubricant we used was typical of those used in advanced oxide media, which was a Fomblin-based PTPF molecule, which was being made by Montefluos S.p.A. a division of Montedison in Italy. With this design, the stiction value was increasing very rapidly and it was clear that the media surface was wearing out. The hunt was on for a better lubricant to reduce wear. We looked at other types of PTPF type lubricants (and older Krytox lube made by DuPont) that were available at the time but nothing was helping. In desperation, we even tried solid lubricant, MoS_2 (molybdenum disulfide) which was a typical low friction "solid lubricant" that was known at the time. This didn't work either. We even tried to sputter Teflon onto the disk to see if that would lower the friction, but this didn't work. By around late April, after everything we had tried had failed to achieve the tribological reliability requirement, we were in a desperate situation. At that time, I had recalled that at a most recent Intermag Conference, a Japanese researcher presented some interesting results on hard disk media tribology using a polar PTPF-type lubricant on disks. However, such a lubricant was never used commercially and the lubricants themselves were not available. Out of desperation, I urged Jim to search for a possible source of a polar PTPF lubricant. Jim contacted Montefluos S.p.A., in Italy to see if they had any polar type PTPF lubricant. Luckily, Montefluos S.p.A. apparently had just developed two new types of polar lubricant called Z-dol and Z-del that they had a small quantity of in their laboratory. Apparently the lubricant was not even intended for disk applications. It was for use on metals. Polar lubricant has a polar end group (with an –OH end group) on the PTPF molecule that can be made to anchor the polar end to the disk. All the oxide media used non-polar lubricants. Montefluos S.p.A. had no idea about the potential for a polar lubricant in the HDD industry. Since we were so desperate, I told Jim to ask for these two lubricants to be shipped to us right away, and offer to pay the expensive airfreight charges of something like $1,600. I believe that Jim had reached Montefluos S.p.A. on Friday and by Monday of the following week we got the two lubes in our possession. As it turns out, this was a seminal moment in thin film media development. The polar lubricants eventually made the media work and Jim Shir can take credit for getting Montefluos S.p.A. to deliver us the lubricant. However, we had a lot more work to do in figuring out how to use these new compounds.

The lubricant arrived but we could not figure out how to apply this material onto the disk at first. When one tried to apply this lube dissolved in Freon to the disk by using a regular spray nozzle, the Freon would quickly evaporate and leave a residue of lube droplets that was very difficult to spread evenly over the disk using a Texwipe cloth. So try as we might, when we applied the lubricant, it actually made the stiction and friction even higher. The first critical step was to apply the lube-Freon solution in the finest mist that could be achieved. For this purpose, I raided my wife Nancy's perfume bottles to try using their sprayer. I dumped out the expensive perfume from the bottles and took the sprayer to try it on the disk. I should mention however, that these polar PTPF lubricants were much more expensive than any perfume she had. Ounce for ounce, this liquid might have been the most expensive liquid around. Amazingly, the mister from the perfume bottle worked much better. Atomization was much finer than what we have been using. The next step was to spread the lube around the disk, and this was done by aggressively wiping the disk surface using a Texwipe cotton cloth wrapped on a plastic stick. This product was already in use by disk manufacturers to clean disk surfaces. The cotton cloth was initially dampened with a small amount of methanol. The method was analogous to "buffing" the wax on a car, and that terminology stuck at Komag, and that was what we called the process, a "spray-buff process." The first lubricant we tried was Z-dol. Once we spread the lube evenly around the disk, we found that we could pass up to 50,000 CSS cycles and still meet the stiction requirement. This was a major breakthrough, and we were quite overjoyed. I don't think this sort of performance was ever seen before, and we were the first to demonstrate it. We tried the same method with Z-del lubricant and this also showed good results. However, Z-dol was good enough and we had more data, therefore we chose Z-dol and proceeded to make more disks. Today, all disks used in disk drives continue to use this type of "polar" lubricant on their disks. I believe that we were the first to figure out how to do this trick successfully on thin film media. We bought some base motor, the right type of atomizer nozzle and spread the lube manually. We made additional refinements to the process later. We added methanol to the Freon so that the mist when applied to the disk would not evaporate as quickly, and we setup many manual stations where the disks could be "buffed" with cotton wipes to spread the lube with the disk mounted on a motor spindle. Eventually we built an automated tool that did this whole process and we practiced this method for many years.

We were still not done yet as we couldn't figure out how to measure the thickness of this lubricant. The lubricant was only a nanometer or so thick and we had to develop a scheme to try to determine the thickness. This thickness was estimated based on very expensive x-ray photoelectron spectroscopy (XPS) performed at an outside service lab, using a million dollar instrument, but this would not be practical for production. If one applied too much lubricant, the interface between the head and the disk would get filled with lube and one would get a jaw-block type of stiction. If there was too little lube, the disk would wear out. Initially we depended on mechanical means of testing to determine whether the lube was correctly applied. We dragged a head over the disk on a sampling basis to make sure the friction coefficient was within the range of acceptability for the lubricant. Our initial attempt at measuring lube thickness was based on UV photoemission. Thicker lube would attenuate the photoemission, which can be measured with a very sensitive picoammeter. But it turned out this was very sensitive to room humidity and the measurement would vary with time of day and weather. Finally we decided to put a major effort into using Fourier transform infrared spectroscopy (FTIR) but this did not become available until around 1987. At that time the only commercially available FTIR tool for hard disks was from a company called Nicolet that had licensed the technology from IBM. Perhaps IBM originally developed the machine for measuring lubricant thickness for oxide media that used lube thickness much higher than that for thin film. When we started to analyze the performance of the Nicolet FTIR, we quickly determined that reproducibility of the measurement was about 1 nm, and we needed $\pm$ 0.1 nm resolution.

In order to improve the FTIR measurement method, I enlisted the help of my friend David Treves, who joined Komag in 1987 to develop MO media. He had some extra time before he could start on the MO project and he was also an expert on optical systems. We also enlisted a new engineer, Yasuo Sakane to work with David, who was assigned to us from Asahi Glass, with whom we had just signed a joint venture agreement in 1987. When David took a look at the Nicolet FTIR tool, he quickly determined that the optics for the tool was not designed correctly and that it would not work as it was. Additionally, there were spurious reflections that added noise to the measurement. David redesigned the optics and the holder to deal with these issues. He also determined that room humidity was affecting the measurement. He then isolated the measurement chamber and added a nitrogen purge to eliminate the effect of water vapor on the measurement. New thickness standards were made and a procedure was developed to calibrate the tool to the reference sample based on XPS. The work that David Treves and Yasuo Sakane did with FTIR was worthy of a Ph.D. thesis. Once this was done, we were able to make accurate lube thickness measurements down to $\pm$ 0.1 nm resolution in production. Combined with a lube process that was very difficult to copy and having very accurate means of controlling and measuring the critical thickness of the lubricant, we had a winning formula for producing very reliable media with high reproducibility. For many years, we kept this measurement method a secret and not even our vendor Nicolet knew what we did to their tool to make it work. When they came to service the tool, we deliberately took out all our improvements to the machine so that they could not see it. This was a decided edge for us for a while, but like all things we weren't able to keep it forever. Our competitors took some of our key people that had the knowledge and copied the method. Nicolet eventually learned what we had done and made the tool available to everyone else. The FTIR tool is still used by the industry for lubricant thickness measurement to this day, 25 years after David Treves and Yasuo Sakane perfected the method. For many years, our disk became the industry standard for tribology performance.

By July 1 of 1984, we had media that we could provide to customers for evaluation. Scott Chen delivered a few disks to Maxtor and Vertex and they really liked them. We had to make these media using the S02 and S03 sputter systems, which could only make 3 disks at a time and only one side at a time at that. We had to go into 24-hour shifts to try to ready the 100 disks that we had to provide to various customers. When we finally got our 100 disks ready, we wanted to celebrate and we all went out for lunch to a Chinese Restaurant to celebrate. Our celebration was short-lived however. After lunch, Steve Miura, a young engineer that was hired out of IBM to work for Scott Chen, put the media on a certifier to make sure it looked good before shipment. To his surprise, many of the media had developed a large amount of defects. It turned out that the problem was in our cleaning system, which had been designed for silicon wafers. It was really designed to wash only one side of the wafer because for silicon, only one side is used. There was a belt on the system that transported the Si wafer from one station to another, and this belt had contaminated one side of the disk and caused a blister to develop on the side that had the belt mark. We had to go back to work and make more disks, and correct the belt mark problem. By July 15th, we had 100 disks, but only after working feverishly day and night. When we got these disks out, everyone wanted more. It was a good sign.

ULVAC S1 Sputter System

By late April 1984, ULVAC was ready for us to conduct an acceptance test on the S1 sputter system. I took Ron Allen, Ven Kao and Rene Lujan to Japan. We shipped the expensive CoPtNi targets and also the carbon graphite targets so that we could conduct the system evaluation at ULVAC in the town of Chigasaki. We also brought with us a few cassettes of substrates to sputter with. We were quite excited to say the least. It was short-lived however. When the machine was turned on with the heater to bake out the system, the machine jammed and everything came to a stop after only a few cycles, which were only few hours. We came in the next day and the same thing would happen. The problem was with the slits that were placed between the buffer and magnetic chambers, and between the magnetic and carbon chambers. The slits were placed there to maintain a differential pressure of up to 50 mTorr between the two sides of the slits, and hence the slit opening was made very narrow. When the system was heated up, parts inside would move due to thermal expansion and it was enough to cause the walls of the slit to bend and the clearance between the panels and the slit wall to narrow. Eventually the slits would start to interfere with the panels and carriers going through the slit, causing the carriers to jam. When the team went back to the hotel in the evening after the entire day of struggles and problems, they were in a state of panic as they felt that the S1 machine would never work. I think they started to think that they made a big mistake in joining Komag. After the third day of mechanical problems with the S1 system, I think Takei-san wanted us out of the facility so that they could go work on the system without our meddling. They offered to show us around the city of Kamakura, which is a short distance southwest of Chigasaki. Kamakura is famous for its large bronze Buddha. Haginouchi-san, who was a young sales guy for ULVAC at the time, was our guide and he took me, Nancy, Ron, Ven and Rene on a tour. We could at least pray to Buddha for the S1 system to start working. While we were gone, the ULVAC team had re-machined the components and put everything back together. They did this with amazing speed. After the fix, the machine would run for a while so that we made our first attempt to make parts. These parts turned out to have coercivity of around 600 Oe, which was our target, and considering that this was being done for the first time at the ULVAC factory where the conditions were hardly ideal, the results were quite good. It even had the carbon overcoat on the disk. The bad news was that Takei-san told us that he had to re-make the rails which the carrier and the disk holder ride on. Thus it would take him until June before he could ship the machine.

During the ULVAC visit, I also conducted some business meetings with Kobe Steel and Nippon Light Metal on the subject of aluminum substrates before going back to the U.S. They were to become an important partner to Komag later, particularly Kobe Steel. The rest of the team returned back to the U.S. with the two cassettes of disks that were made on the S1, and Scott Chen tested the recording performance. The recording test showed excellent performance, and signal-to-noise ratio was actually better than plated media that we had purchased from other vendors. This raised the confidence level of everyone, including the officers.

By June, we conducted a second acceptance test and we produced media with the expected magnetic characteristics and with a carbon overcoat. The mechanical reliability of the system was deemed good enough for production so that we agreed that the system could be shipped. When the S1 system was shipped to the U.S. from Japan, for some reason it arrived at Los Angeles instead of at Oakland. So the machine had to be trucked from Los Angeles to Milpitas to our facility. I was so worried that something might happen to the machine during transport and lost many nights worrying. If anything were to happen to this machine, we would be set back so much that it could have been fatal. Such were the risks that we were taking. Thankfully, on August 10, the machine arrived at our facility. It was so heavy the truck had sunk quite a bit when loaded. We all took off our shirts to help move the machine parts into our facility. Staging areas were setup and we started assembly of the system. It took about one month to assemble the entire system. In the meantime, we continued to run the S02 and S03 batch machines flat out, 24 hours/day to turn out media to satisfy the customers. S1 started the initial production run in October, 1984. The process we used consisted of using low power RF deposition of sputtered NiP as a seed layer to cover up the residual film left on the cleaned substrates, and high pressure RF sputtering of CoNiPt film to promote the growth of uniform isolated particles. What I had been dreaming of producing using what I knew from all the years of work at Xerox PARC was now becoming a reality.

Other large equipment, such as the wafer cleaning system, also came in from the September to October timeframe. With all of these activities, by the end of September we needed to raise more money. One of the sore points was that Steve Johnson and Sandy Fitch, whose job it was to raise money could not get it done. To be fair, the whole economy had slowed down at that time and money had really dried up. Then the representative from Continental Illinois Venture Fund who was one of our investors suggested that we could try to raise money from Japanese companies. I went to the board to ask for their opinion and the deal was that if we could raise $2 million from Japan, they would match it. There was interest from companies such as Marubeni, Denka, Nippon Light Metal and Kobe. Also Hitachi Metals wanted to get a license for the technology. In order to prevent too much dilution from a share sale, I had to get top dollar for the investment. So at $5/share, we were able to sign up Marubeni, Kobe, Denka and NLM (Nippon Light Metal). We ended up getting a total of $5 million. Max Palevsky, who was the founder of SDS, stepped in with another $1 million from investors and he joined the Komag board. Max was quite involved with Hollywood those days, and he was a producer for the famous movie *Spartacus* with Kirk Douglas, and was managing a fund with money from Hollywood movie stars. So I believe Barbara Streisand and Jack Lemon were also investors in Komag through Max. Finally we got total of $7.2 million without taking any money from Hitachi Metals. Just as well, because licensing our valuable technology would have been the last resort. Max Palevsky was also a long-time board member of Intel as well, and later on he arranged to bring in Craig Barrett as a Komag board member in 1990 before he became Intel CEO. Once Craig Barrett became Intel CEO in 1999, he replaced himself with Michael Splinter who was one of the top vice presidents at Intel at the time, and he went on to become CEO of Applied Materials. We had some very prominent people as board members at Komag and we were quite fortunate in that regard.

By the time we shipped our first production parts to customers in October, they were already screaming for our product. It was apparently doing very well and customers really needed it for them to have a viable drive product. They also wanted to know too much as to what we were doing. Some customers like Quantum wanted to know all of our secrets, as stipulation for buying our product but we simply refused. The analytical techniques available in those days were such that no one could figure out what we were doing, such as having a lubricant on the disk and what type of lubricant we used and how much. We simply persisted with "no-comment" and basically got away with it. Quantum was making 8-inch drives at the time and had to migrate to 5.25-inch drives in order to survive. They were already a sizable company and threw their weight around but to no avail. As small as we were, we weren't budging. IBM had also tested our media, and came to the conclusion that our thin film media was the only one that could pass their reliability test. Having gotten such information, we had the upper hand and gained considerable confidence. Needless to say, we did not sell our media to Quantum for quite a while.

By November 1984, Thanksgiving time, we had to ship 1,000 disks in order to stay with the business plan. We had been making many modifications to the S1 sputter system since the installation, and by then the machine was starting to run smoothly enough to be able to make disks. We had numerous mishaps and near disasters with the system but somehow we got through them. ULVAC had stationed two of their top engineers, Mr. Ono and Mr. Hirano with us throughout this time period, and with their help we got the machine to start running better. To ULVAC's credit, they were also taking notes as they wanted to build a more advanced version of the sputter system based on what we learned together.

During this period of time, we suffered through some major problems and near-disasters. The first one was on Thanksgiving eve when we were trying to complete sample making of the first one thousand disks to meet the commitment we made to the board. We started the sputter run early in the morning and produced 1,000 disks by late afternoon, but because the machine was running quite smoothly, we decided to continue the production run to generate more disks. By the time we were approaching 2,000 disks around two in the morning, Jim Shir called me from the core room at the back of the S1 sputter system in a panic to inform me that there was a big flood on the core room floor. So I quickly shut down the machine and left the clean room to investigate. To my surprise, the cause of the flooding was from the cathode and the RF network. There is a cooling plate that is attached to the cathode power input shaft that is water cooled to keep the shaft cool during sputtering. Tin-lead solder was apparently used to attach the cooling pipe to the plate and it had melted. This caused overheating of the cathode power input shaft and the rubber gasket also melted and it had sprung a leak. We were lucky that Jim had caught the flooding. If it had not been caught then, we probably would have caused considerably more damage to the cathode itself and it would have taken months to recover. On Thanksgiving morning, I and ULVAC engineers Mr. Ono and Mr. Hirano fixed the water pipe contact with silver solder so it could stand up to higher temperatures. RF sputtering under high power caused various components to heat up, so that conventional methods used for DC magnetrons, for example, do not work. This incident taught ULVAC engineers a lesson and they used this experience to change the design of the cathode on subsequent machines that we purchased from them.

Another memorable disaster occurred in early May of 1985. This was when we accidently flooded all the vacuum pumps and pumping lines with water. ULVAC had installed a cryo-trap in the large diameter pumping pipeline from the sputtering chamber to the Roots blower pump in order to pump water vapor more efficiently from the chamber. This cryo-trap was designed with a copper coil insert in the pumping line of the Roots blower pump, and the copper coil is cooled from the outside by winding and contacting the coil on the wall of a liquid nitrogen container. The container was made with a copper can which was inserted from outside into the pumping pipe. During operation when the system is pumped down, the copper container will be filled with liquid nitrogen to chill the water that would be flowing inside the copper coil so the coil can act as an effective trap for the excess water vapor in the vacuum chamber. This was jokingly called the "poor man's cryo," for the fact that we could not afford the more expensive cryopumps that were much more efficient at pumping water vapor. When the system was shut down for service, the liquid nitrogen container was brought up to room temperature and the water was shut off to prevent water condensation on the cold trap. When the next cycle of sputtering was run, the water to the cooling coil was turned on after vacuum in the system achieved operating conditions. After the water was turned on, the liquid nitrogen would be added to the container to cool the running water in the coil. Unfortunately, due to operator error, liquid nitrogen was put into the system before the water valve was turned on, consequently the residual water in the pipe quickly froze and it ruptured the water line inside the vacuum. Once this took place, the entire pump line quickly filled with water, and mixed in with the pump oil in the Roots blower pump. Luckily we had the main valve closed to the chamber, so that chamber was spared. The entire roughing line to the S1 system had to be completely dismantled, drained of the water and re-built, including the roughing pump and the Roots blower pump. There were literally hundreds of gallons of water mixed in with oil in the pumps and in the pumping line when this took place. It was a huge mess. We put all of our manufacturing line workers and engineers to work, which was around 30-40 people at that time. We worked all day and rebuilt the system in one day. It was an amazing feat. If the water had gotten into the main vacuum chamber however, we would have had a much more serious situation and it may have taken us a week or more to recover. Everyone worked very hard in getting through this sort of situation. The embarrassing admission is that we actually had this happen to us twice. It was a hard lesson, and the machine had to be re-designed so that this sort of mishap would never happen again. This experience taught us not to use the complicated water cryo-trap in the follow up system, but rather spend the money on more expensive commercial cryopumps.

Another major issue that deserves some comment was the interference issue with RF when we were sputtering from both sides of the panel. We had the common exciter that Mr. Takei had recommended, but we weren't experienced in how to use it. We tried various phase angles between the two sides, and still we had significant problems in the RF networks interfering with each other. Eventually we had figured out how to measure the phase on the cathode, and tune the network controller in such a way as to stay tuned to good operating conditions without interfering with each other. To prevent operators (and engineers) from "messing" with the phase control knob, I put tape over the knob with a big sign that said that anyone that dares to touch it will be killed (by me). No one had ever attempted to do RF sputtering in this manner with the precision that was needed for control of magnetic properties in the film before, so the learning curve that we had to go up was very painful and hard fought. Looking back, it is still amazing to me that we solved so many problems in such a short time. We also had many issues with RF equipment, which was part of the learning curve for operating RF at fairly high power. The insufficient knowledge to operate a high power RF sputtering process was also the reason that we had encountered the flooding incident I mentioned earlier.

By December of 1984, we were able to produce media in good quantity and the system was running reasonably well. We implemented 24-hour production, and even worked on Christmas day. We were starting to ship media like mad. During this time period, the manufacturing organization that had been already formed under Phil Gahr (who was hired as vice president of operations) wanted to start exerting its control over the operation. Up till then, much of operations were directed by R&D under Jim Shir. It was as if Ven Kao's production team was reporting to Jim. Needless to say, I also had a lot on my own hands as chairman of the board. I was the one that designed the entire sputtering process and the equipment; therefore I had to see to it that they all worked properly and debug all the unforeseen problems. In any case, operations were under Steve Johnson as CEO and operations reported to Steve. Jim Shir also reported to Steve Johnson as well. However, I was still the founder of Komag and the main force behind the development of its technology. Hence I had a lot to say about everything. In any case, Phil Gahr had been complaining that engineers (R&D) had too much control and that operations had to take charge of manufacturing. Steve Johnson agreed with this and told me to give all control to Phil Gahr and perhaps I should take a vacation to let Phil handle things. I did so only grudgingly, but I did take a short vacation, to see what might happen. When I returned, we were in a crisis. At first, Steve came to ask for my help in the crisis, but I ignored it at first because I was being obstinate. Then Scott Chen came to tell me that we were dying and that I must come in to help, as all of a sudden we couldn't make any good product at all. Recording performance, especially the OW (overwrite) was deteriorating and amplitude (of the magnetic signal) was low for the media. He told me that they tried to raise the amplitude by thickening the magnetic film, which would have been the normal response, but this only made things worse. Everyone was at a loss as to what was happening.

So I was now back in the "operation" to try to fix this latest crisis. The first thing to look at was the media and I had the VSM measurements (vibrating sample magnetometer) done on a series of different film thickness made by Tom Yamashita. The hysteresis loops all looked terrible. They had very poor squareness, and low Hc. Also, the squareness and the Hc dropped with increasing film thickness. The loop showed many of the characteristics of perpendicular media, hence explaining the poor amplitude and low OW. To prove this hypothesis, I had Tom check the media by x-ray diffractometer, and sure enough, the film we were making now was significantly more perpendicular in terms of its crystallographic orientation, and the thicker the media, the more perpendicular the media became. Normally what we should have had was media with a random crystalline orientation and somehow the films were now growing much more in the perpendicular crystal orientation. So what had changed? I went to take a look at the S1 sputter machine, and I noticed that there was a new device attached to the tool. It was a residual gas analyzer (RGA). I asked who put this in and it turned out that Rene Lujan under Ven Kao's group installed the RGA. They offered that the RGA allowed them to detect leaks that the S1 chambers had and now the system was in much better vacuum than before. They were quite proud of it actually as they said that the system was leaking like a sieve. In thinking about this based on my past experiences, I had a hunch that the sputter system had become much cleaner and was operating at a higher vacuum level while I was away, and that this change was now causing the film to grow more perpendicular. To test this theory, I cut one of the inlet gas lines with a hack saw, and installed a small needle valve to deliberately introduce an air leak into the system. Immediately I got the effect I was looking for, the hysteresis loop became much squarer and Hc recovered. OW and amplitude performance immediately recovered. We actually needed a more leaky vacuum system to make better media! In order to study this effect further, I had our facility manager, Jack Sargent obtain an Argon gas bottle with 2 and 3% nitrogen added in and got Tom Yamashita to quickly generate a curve plotting the magnetic properties as a function of the amount of nitrogen deliberately introduced into the magnetic target area. We found that we could suppress the perpendicular component, and not only that, we could also control Hc as well. This was an added bonus, which we could now use to control coercivity of the media more precisely. Here, my previous research experience in growing perpendicular media helped. I knew that perpendicular media requires a good vacuum level, and this experience in relating hysteresis loop characteristics to crystallography of the media allowed me to very quickly identify the problem. It was also a lesson that leaving things to manufacturing people on their own can have unintended consequences, especially when the process parameters and factors are not yet fully understood. Things done with good intentions can drastically affect the process sometimes.

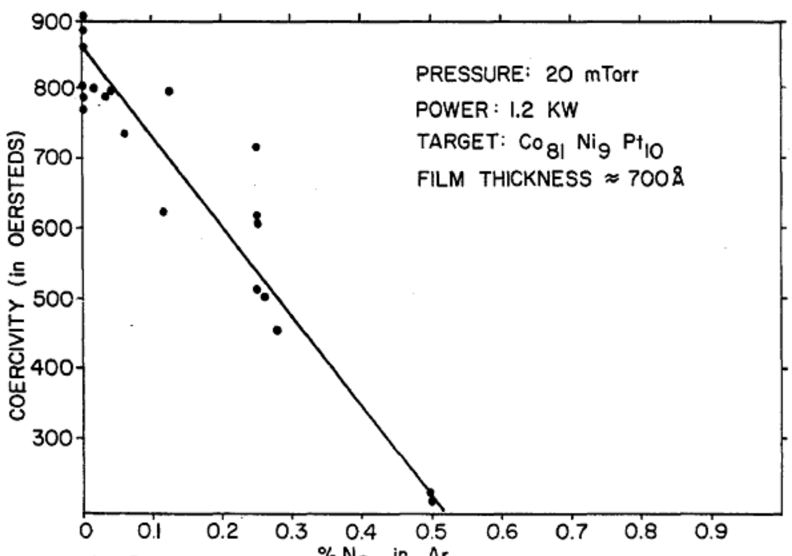

FIG._1b.

From US Patent 4,749,459, showing the effect of adding nitrogen gas during sputtering in reducing coercivity of the media.
(Figure 6-1)

The discovery of the effect of nitrogen on the magnetic properties was a big bonus. It was needed to suppress perpendicular growth of the film, but it also provided better noise performance. The ability to make fine adjustments to Hc by nitrogen addition became a key advantage that improved our productivity. For example, for a 6% Pt alloy target we could vary the Hc between 600 to 800 Oe by tuning the amount of nitrogen input to the machine without having to change target. We could make different Hc products within a sputter run with the same set of targets, and it provided great flexibility in how we manufactured parts. By the time we were running many sputter systems, we could run the sputter system continuously for 4 weeks between taking the system down for cleaning and target changes. With this discovery, we could make all sorts of Hc disks for customer specifications and needs within a sputter cycle. Therefore, we could provide new samples to customers quickly without having to shut down the system for target changes.

February Crisis

Our 600 Oe product was running well until around February 1985. We kept producing this product in the S1 sputter system, but the mechanical reliability of the machine was still terrible. It kept jamming, usually due to disks dropping from the panel. The disks would end up jamming the gears that drove the carriers, or get stuck in the wrong place and prevent the carrier from moving or getting stuck on doors. Initially the rail on which the disk carrier rode on was becoming crooked due to uneven heating inside of the system. But even after we replaced the rail, the disk drops continued and the machine uptime was terrible. The situation seemed to be that carriers were very heavy, and various mechanical parts were wearing out very quickly. Gears and bearings were getting damaged and seizing up under the heavy load. Ven Kao, head of the manufacturing under Phil Gahr, was complaining loudly that he couldn't run production with such an unreliable system. Dr. Hayashi, the head of ULVAC, had visited us in early January 1985, and in discussing the issue, he ordered his people to supply us gears with a TiN hard coating. Mr. Minami also recommended we change the AC motors to DC motors so that speed control could be better. While we waited for these new features to be put into the system, we had to constantly pay attention to the carrier, to make sure that it was moving inside the system. If it stopped, the panel in front of the cathode would overheat and warp it, causing even more problems. Jack Sargent installed a simple "clicker" on the chain that drove the carrier in the system, so that operators who were assigned to the system could hear the clicking sound during the sputtering operation from the clean room. If the clicking stopped, it meant that carrier had stopped moving and the operator could attempt to free it quickly before the situation got worse. It was a crude but quite effective signal for monitoring the machine. Machines back then did not have the fancy electronics and monitoring devices and software that exist on current production tools.

On February 14, the ULVAC team showed up with new gears and DC motors. Replacing all the gears on the system was major surgery on the tool. The system had 30-40 of these gear assemblies, and it was considerable work to replace all of them. But the hard TiN coating on the gears looked beautiful, and very promising. I learned later that the coated gears alone cost $60,000, and it was one of the latest coating technologies that ULVAC had developed. It happened that on the day when the ULVAC team arrived, the sputtering machine just had a major carrier jam and the system had to be opened up to un-jam the carriers. The machine had been running for only one week, which was about half the normal running cycle. In a normal run, the sputtering machine was expected to run continuously for two weeks, and then the machine would be shut down for clean-up to replace the shields around the cathodes. Disk panels also had to be cleaned and disk carriers serviced to make sure that moving parts such as bearings could be checked for damage. Panels had to be cleaned in order to remove the built-up sputtered film so that they would not cause flaking. The cleaning was done by sandblasting and scrubbing in soapy water, followed by bake drying for several hours. It took almost a one entire shift (12 hours) to complete the clean-up operation. When the ULVAC team showed up when we had a major carrier jam, we decided to give the machine to them so that they can do the retrofit work on the system. It was estimated that the retrofit work would take one week, and because the system had been running for only one week, we decided to leave everything else as is, without cleaning up the system. All the carriers and panels were removed from the system and kept clean and covered up during this upgrade. The upgrade did take about a week to complete according to the plan. Around this time, I had another crisis. My older son Glenn had suffered a stroke and he was partially paralyzed on one side of his body. Apparently it was due to the chicken pox virus, which had caused a blood clot to form in one of his arteries. I had to go see him in Oregon. Seeing that he would recover from it, I returned back to Milpitas. By the time I returned, the system was back together and we were ready to re-start the machine.

This is when we got into trouble. No matter how long we ran the machine, we lost coercivity in the media. Our targeted coercivity was 600 Oe, but we were seeing only 200 Oe to 400 Oe at most. Something was seriously wrong with the system. Initially we thought that the system simply needed to get baked out more, so we simply kept it running for a whole day, and Hc would recover somewhat closer to 600 Oe toward the end of the day. But because everyone was so overworked, we decided to shut down the machine at the end of the day and come back the following morning. Then the same thing would happen again, no coercivity. This situation repeated itself a few times and we had to look for the root cause. We suspected a leak in the system at first, but we could not find any. After we opened up the system a few times, Bruce Charlan thought that he smelled some oil in the chamber, so we even wiped down inside the system thoroughly with acetone. There was no effect, and coercivity continued to be low. We took a closer look at the cathode area, such as the shields and we noticed that the coating on the shields was black. Was it some sort of carbon material, perhaps residue from the breakdown of hydrocarbons in the chamber? Or was it some sort of oxide? While all of this was going on, the rest of the company was preoccupied with analyzing competitors' media and spending time and money analyzing them. I blew my top at this situation and asked for full attention on the analysis of disks, black coating on the chimney and the shield. I had asked Sonny Wey, whose responsibility was for this sort of analysis, to put all his attention on the analysis of the problem at hand. Unfortunately, the chemical analysis did not reveal anything that was obvious, and we were still quite lost.

The situation was quite dire because we could not ship anything to our customers, and we already had quite a number of people hired to manufacture the product. When production stops, everyone is wondering what the problem is. The board of directors was notified and they gave us six weeks to fix the problem, or else. If we could not solve the problem by that time, we would have to start letting people go. We were in a full crisis now. At times like this, there are only a few people who can actually help solve the problem while everyone else has to watch. Worse yet, everyone will start to offer opinions that sometimes distract from the work of those trying to fix the root cause. One such suggestion was that we should be looking at another sputter tool, and one such competing tool was a machine made by a small local company called CPA. It was the machine that our competitors, Trimedia and Lin Data were using. Phil Gahr wanted to check out the machine and I couldn't stop him from doing this. When Steve Johnson pressured me to consider the CPA machine for replacement following Phil's recommendation, I did relent and took Mr. Ono from ULVAC with me to see the machine together with Phil Gahr. I thought that maybe Mr. Ono might see something on the CPA machine that might provide some hint as to why we are not able to make good films on S1. Taking Mr. Ono along was not quite ethical, but I was desperate for any ideas. Around the beginning of February, we finally hired Bob Martell as full time vice president of marketing. Unfortunately for him, as soon as he arrived we could not make any disks. Every day he would come to S1 to see whether we had started to make disks or not, only to be disappointed. After about a month of this, he could not take it anymore and he left to join Seagate. For Bob, it turned out well because he went on to become the top sales and marketing guy for Seagate and he had a very prosperous career there.

In solving any problem, one has to be systematic and use all the intelligence that you can muster in trying to understand the problem. You must base your actions on your experience and on the scientific method. But when you have a big organization and many people involved, it is like trying to herd cats. Things tend to fly off in all directions unless you keep everyone focused and directed. During this period, I drove everyone crazy, but I really did direct the people and kept our focus on the issue. It was probably the most stressful period in my life when I look back on it. Getting to the solution was part process of elimination. We thought about leaks, and that was not it. We thought about hydrocarbon contamination and this did not seem to be a factor either. We then started to pay much closer attention to the RGA and what it was really telling us. We used it to check for leaks, but we could also see things happening dynamically in the system. We could watch hydrocarbon components with and without the sputter cathode being on. One thing that we always saw was the water vapor, which is mass 18. When we turned on the plasma, we saw a very high hydrogen signal, and a reduction in mass 18. The water in the chamber was being consumed by the plasma and water vapor was being broken down into hydrogen and oxygen. In discussing all of these results, it was Mr. Ono that uttered the words that were the key to solving our problem. What he said was, "perhaps the panels are too wet". After I heard this comment, it hit me right away that throughout this time period, we left the panels with sputter coating on them outside of the system while we were working on the system. The coating can stay on the panel for up to 2 weeks of continuous running, and when we stopped the machine for the upgrade the panels had only 1 week worth of coating on them. In normal production operation we run continuously for two weeks. But when we shut down the machine at the end of the day or for some other reasons such as running out of substrates for example, we would leave as many panels as possible inside the system in vacuum. There were always a few panels that stayed outside in the air, but this was often for few hours, and at most 7 to 8 hours when we shut down for the day. When we did the one-week shutdown for the system retrofit however, all the panels were removed from the system and kept in a location we called the "carrier farm" so that it stayed at least relatively clean, but in air. The key difference, however, was that we had all the panels that had 1 week of coating already on them, for about a week outside in air. The theory was that after a week of deposition of magnetic and carbon films, the surface of the panels had a thick layer of highly porous film with a large surface area to adsorb moisture from ambient air during one week of sitting outside. Therefore it was very hard to remove this absorbed water even in vacuum and during baking and sputtering. As we had seen in our earlier troubleshooting run, it took at least eight hours of continuous pumping and sputtering before the panels became dry enough to produce magnetic films with 600 Oe coercivity. Now realizing that the panels themselves might be the source of all the problems, we re-cleaned the entire system and installed a fresh set of shields and chimneys. All the panels were sandblasted clean and re-assembled. When we re-started with this condition, finally the coercivity came back to normal after a few hours of baking and everything was back to normal again. It took us about 4 weeks of absolute hell to figure this out.

What really happened was that when we took the system down for gear and rail retrofit, all the coated panels just sat outside collecting and adsorbing water vapor into the coating. When we put them back into the system, the chamber was overwhelmed with water vapor so that the coercivity was wiped out from the magnetic film. The black coating we saw on the shields was in fact oxidized cobalt film. After we ran the machine for an entire day, the water vapor finally had started to come down a bit, but as soon as we saw that Hc started to recover, we were calling it a day and going home. The few panels sitting outside of the chamber would re-absorb the water vapor and these panels then overwhelmed the system again with water vapor. The RGA was showing that this was taking place all along, but we didn't know how to interpret the data. This experience taught us a lot of lessons. It turns out that having too high a vacuum was not good for the film properties. It leads to film taking on perpendicular growth and causes poor hysteresis loop squareness and low Hc, as well as low amplitude. In order to prevent this from happening, we introduced nitrogen into the chamber to break up the tendency of the film to grow perpendicularly. If we had too much water vapor in the system, it would also lead to a bad situation. The film became too oxidized and drastically affected the film properties, namely a huge reduction in coercivity. What we learned was that too much water vapor hurt the film. What we didn't know then was that too little would also cause problems, but we didn't learn this for almost another year. I will talk about this experience in the next chapter. What was very clear was that the sputtering process was very sensitive to vacuum conditions and that we had to pay close attention to the sputtering environment and what we introduced into the system, namely through the panels that went in and out of the system. After this experience, we managed the panels and carriers very carefully. Residual gas played a very important part in film properties, and this knowledge we would use to our advantage in the future.

<u>Nucleation Layer</u>

For initial design of the media, I did not think so much about the need to grow the magnetic layer on top of another sputtered layer. We had been depositing magnetic film directly on the polished NiP substrates. When we had the problem with belt marks from the cleaning system on the disks made on S02, it did highlight the issue that perhaps the NiP surface layer needed to be covered up or cleaned up. One trick of cleaning up the surface of the substrate in a sputtering process was to apply a low-level plasma discharge to the substrate so that the plasma has the effect of gently cleaning residual contaminants off the surface. One has to be careful about this because too high a plasma power can disrupt the substrate surface sufficiently so that growth of the magnetic layer can be affected, causing changes in magnetic properties. The low-level plasma cleaning is typically called "glow-discharge cleaning". Several ways are possible to achieve this effect in a vacuum system. We applied the glow discharge method to the NiP substrate just before the magnetic film deposition on the S02 sputter tool, and this seemed to have helped with the belt marks.

In designing the S1 sputter system, I had asked ULVAC to install extra low power cathodes before the magnetic layer for the purpose of glow discharge cleaning of the substrate. This was done with a cathode coated with plated NiP, and we could apply low power RF to form low-level plasma in this station. We experimented with this process initially on S1, but we tended to get less square films and generally we weren't seeing much benefit. During the February 1985 crisis, we took out all the cathodes for this glow discharge station because we thought that it might be a source of leaks.

As we made more 600 Oe disks, it was noticed that we had an amplitude modulation problem with our media that was coming from localized changes in magnetic properties causing the signal to vary rapidly over a short region of the media. Customers were noticing this problem in the drive so that it was starting to become a big problem. Analysis of the amplitude modulation indicated that it could come from drying marks left over from the cleaning system as well as from belt marks from the cleaning system. We were using the cleaning system made by Silicon Valley Group Inc. (SVG) that was originally designed for cleaning Si wafers, and it moved the wafer from one station to another on plastic belts. We did as much as we could with the cleaning system, such as keeping the belts wet for example, but the amplitude modulation problem would not go away. We had to bring the glow discharge process back into the S1 system in order to overcome the modulation problem. The process that we ended up using was one that Tom Yamashita had come up with, which was to use the extra magnetic station that we had in the S1 system to sputter SiO_2 onto the NiP substrate before the magnetic layer. It had the effect of getting rid of the modulation issue quite well, but it also produced a much squarer hysteresis loop. As is the case with a process that is introduced in a hurry to correct a specific problem, we didn't quite understand what the SiO_2 process was doing, at least initially. It was low-power RF sputtering of SiO_2, but the power was so low that we were really depositing hardly anything. We actually did check using an outside service using an Auger depth profile that SiO_2 was hardly detectable on the media. But we were clearly removing the contaminants as indicated by the elimination of modulation, and we had a very nice square hysteresis loop, and magnetic properties which were much more uniform across the entire surface of the disk. We gained amplitude because of the high squareness, and Scott Chen and his team were happy enough with the performance of the media. We thought that what we were doing was to seed the magnetic layer in a more uniform and controlled fashion using the SiO_2 process, and it was clearly something that was important to do. Later, we came to realize how important the seeding process was for the control of the growth of the magnetic layer. Little did we realize then that higher squareness in the media was due to having made the film grains much more exchange coupled, and hence the media had more noise. Only when we approached the 950 Oe and 1,200 Oe coercivity range with higher recording densities, did we realize just how bad the noise was for the SiO_2 seed process. This is the area that I spent lots of time at Xerox PARC-investigating the effect of inter-particle exchange and its effect on the limits of recording. The conventional wisdom was to have very high squareness so that one obtains the highest amplitude, but this situation arises because the particles are highly exchange coupled. One has to give up some of this high squareness by decoupling the grains somehow, either physically by high pressure sputtering for example, or by introducing something in between the grains. For the 600 Oe product designed for ~10 Mb/in^2 areal density, the SiO_2 seed layer was sufficient but not for higher recording densities. We will discuss this subject in more detail in the next chapter.

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CHM Reference number: X3593.2006
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Chapter 7

Manufacturing Ramp

Having survived our February, 1985 crisis, we were back on track to ramp up our production. We were still learning all the time on all fronts, steadily gaining knowledge and know-how to make the product more reproducibly and with higher yields and better cost. Our productivity was improving as well. We started to run 24 hours/day in manufacturing and this actually helped to run the sputter systems more controllably. The S1 system continued to have many mechanical issues and it was a constant battle to keep it running. We had been loading and unloading the disks manually using operators but Ron Allen developed a robotic load/unload system to do this work, which made for more consistent operation. We also had one of the industry's first robotic media testing system as well, which allowed us to test the media 100 % and provide quality output to the customer. We were starting to run on all cylinders.

Most importantly, the performance of our media was very good. The key strength was not so much the magnetic performance, but reliability. The key issues that drive customers were suffering from in using plated media, caused by the phenomenon called the "black plague," all went away by using our media. The corrosion issue that everyone was concerned about also was a non-issue with our media, which had the carbon overcoat. At first we were still competing with oxide media and plated thin film media, but in terms of recording performance, the 600 Oe medium was already well ahead of anything that oxide could do, and the carbon overcoat, together with our secret weapon of lubrication, made it very tough for plated media to be competitive. Plated media had to add a carbon overcoat to compete with all-sputtered media, and they had considerable difficulties in doing this. The sputtering tool for carbon deposition was essentially the same as the type of sputtering tool as we were using for the all-sputtered thin film system, and any cost advantage that plated media competitors might have had evaporated quickly. As it turned out, the plated media surface was not a good surface to deposit carbon on, as it was difficult for carbon to stick to the surface. Doing all the films in one system, all in succession, from the magnetic layers to the overcoat, was the right way, and by the time the plated media companies realized this, it was too late. The sputtered film companies like us, Trimedia, Lin Data, and Akashic, had taken a foothold in the business, and one by one, the plated media companies disappeared.

By year-end 1985, we achieved \$3.7 M in sales with an operating loss of \$6 M, but we were on our way to a ramp. We had \$4.3 M in cash left. We had a third round of fund raising in the spring of 1986, also at \$5/share, as we needed to buy more equipment for production expansion. We raised this money by forming a joint venture with Asahi Glass and Vacuum Metallurgical Company (VMC), a subsidiary of ULVAC. Another reason for the 3rd round was that during the February crisis in 1985, we burned through $800K/month of cash when we were not producing, and $7.5M that we raised in the 2nd round wasn't enough to carry us through the IPO. By February of 1986 we knew that we needed an additional infusion of money. We had talked to a few possible investors, such as Singapore's Singakong Venture and Denka in Japan, who also wanted to do a joint venture. It was Dr. Hayashi of ULVAC who recommended Asahi Glass as a good joint venture partner and it was a good advice. By early 1987, we had a joint venture agreement to start up media production in Japan.

In late 1985, we took delivery of the 2nd sputter system, the S2 system in our Milpitas building. It was also made by ULVAC but the design was a radical departure from the S1 system. First of all, the system was larger, and we used all cryopumps to pump the main chambers, without using any Roots pumps. As a result, the system cost twice as much as S1. As a matter of fact, we didn't have enough money to pay for the system. Nevertheless, Dr. Hayashi of ULVAC let us have it and allowed us to pay for it once we started to make products from it. He had so much confidence in us that he was willing to give us that sort of credit. I thought that it was extraordinary that ULVAC or any company for that matter would take such a risk with a small start-up company. The panels for S2 were able to hold fourteen 5.25-inch disks. The footprint of the system was actually smaller than the S1 system's footprint because the return carrier went over the top of the machine instead of on the side. Mechanically, the system was a considerable improvement over the S1 in terms of reliability and speed. The cathode was larger to accommodate the larger panels. The process was basically the same, in that it had two RF stations for the seed and the magnetic layers, while the carbon chamber held two DC cathodes separated by a slit from the high pressure RF stations. The process that we developed for 600 Oe media in S1 continued to be used in the S2 sputtering system to make 800 and 950 Oe media. We used higher Pt-containing alloys to obtain the higher coercivities. By the time S2 was operational, in the beginning of 1986, we were paying close attention to the residual gas in the chamber so that we were able to produce good disks according to the plan we laid out for the system. A year after our first February crisis in 1985, we faced another crisis, this time on the S2 sputtering tool, after having produced-products from the system for several months. So, in February of 1986 we suddenly could not obtain the right coercivity after a routine system clean-up. It was getting late into the evening and I was still working in my office when the sputter supervisor came to ask for help. I quickly grabbed several engineers and headed to the machine. The first thing we looked at was the RGA, but unlike the previous experience with S1, the RGA did not indicate any issue with the water level. The panels had been freshly cleaned and if anything, the water level was unusually low in the system. I asked Tom to prepare freshly cleaned substrates and run them through the system immediately because we suspected the substrates the operator used may have been sitting in the cleanroom for too long and might have absorbed too much water. However, even when using the new substrates, the Hc was still very low. I was very puzzled by this result and headed back to my office to think about the issue. When I reached my office and as I reached for the doorknob, I received a nasty static electricity shock to my fingers. It was wintertime and the heaters were on to heat the building. It hit me at that instant that the cause of the low Hc may have been related to low humidity, and I went to check the humidity gage inside the building. To my surprise, all the gauges were completely pegged to the lowest level. I discussed the situation with Tom, speculating that very low level of water indicated by the RGA is related to the low humidity level in the room. That winter, we were experiencing one of the driest seasons we have seen in the Bay Area.

Additionally, we would turn on the heaters in winter to warm the room, and the building would become dried out, but this time it was just bone dry because of unusually dry weather. Therefore, it was very easy to build up static electricity. We always worried about too much water since the S1 experience, but we had not considered the situation when it was really dry. In working out this situation with Tom, we realized that the low water level was affecting the deposition rate of the SiO_2 seed layer target. Normally, with the panel being loaded with water vapor when it comes out of the system, the SiO_2 target is exposed to a lot of water vapor because it is the first station that the panel sees. When there is water available, oxygen is generated by breaking the water, and oxygen slows down the deposition rate of SiO_2 drastically. When the room became bone dry and the water vapor was not going into the system, the SiO_2 thickness was increasing dramatically, and this was affecting the way the magnetic film was growing. It was in effect becoming very exchange coupled, which reduced the coercivity. We saw other evidence of this as the signal-to-noise ratio of the media was suffering as well. So, here we have it. Too much water vapor, we get oxidation of the magnetic film, and too little water, we get exchange-coupled media by virtue of forming a too thick SiO_2 film.

We took the temporary measure of reducing the power of the SiO_2 target, but the longer term solution was to control the humidity of the room to a narrow range. We had the facility people install a humidity control system around the sputtering tool to maintain a ~ 40-50% humidity level at all times where the panels are exposed to air.

Transition to 800 Oe Media

By 1986, the industry was ready to push areal density from 10 Mb/in^2 to 20 Mb/in^2. For 5.25-inch media, the capacity was to increase from 10 MB to 20 MB/disk. It was easy enough to raise the coercivity of the media from 600 Oe to 800 Oe by increasing the Pt content of the alloy. I believe our competition had a much more difficult time. Plated media companies were having problems raising coercivity with their CoP plating chemistry. Those employing the CoNi/Cr structure had to change the composition of the CoNi alloy to CoNiCr. We were not free of issues, however. As media were starting to be tested at higher areal densities, we started to see higher defect problems. The defects were scattered all over the disk and at first it was quite difficult to figure out where they were coming from. This issue was identified by Steve Miura who determined that the defects were associated with the random radial scratches left on the disk surface from our polishing process. Our process imparted a random polishing pattern to the disk, and some of the small and large polishing scratches could occasionally line up with the recorded bits, and the scratch lines either caused a drop-off in the signal, or else caused "bit shift," where the transition signal was slightly delayed or moved up in time. Our customers' drives were experiencing this and there were enough defects to become a showstopper.

The only way to avoid the random scratches that came from our polishing machine was to add an additional "texture step" to achieve concentric polishing/texture. We used the word "texture" to signify the fact that we deliberately put roughness back onto the disk surface in order to prevent initial stiction. We had no hope of doing this with the polisher we had been using because it was impossible to achieve a concentric pattern using it. What we needed was a new machine that could impart a largely concentric scratch pattern on the disk. From my previous experience at Xerox PARC I knew that we could use a polishing tool made by Strasbaugh. I arranged to get their commercial machine to Komag right away and put Tom Yamashita to work to develop some preliminary data. This work was rather crude as we didn't have the facility and space to even place this tool inside the building. I ended up having to put this tool literally outside of the loading area of the building, plumbed water with a hose into the machine, and powered it with an extension cord. This was probably illegal, but we needed to generate some data very quickly. This type of work involved trying out a combination of polishing pads, alumina slurry and "texturing" conditions to obtain the smoothest possible concentric pattern that would polish away the random scratches that came from the polishing step. Once Tom established the approximate working parameters, I sent him back to sputtering and I organized a team with Jim Shir to complete the study for optimizing the process parameters. We quickly came up with the right level of texture to provide good defect characteristics, while providing just enough roughness to provide good stiction performance and glide clearance. This exercise was quite successful and we proceeded to invest in production manufacturing tools from Strasbaugh for the texturing step.

It turns out that our competitors were doing the same thing, but they were trying to develop the so-called oriented media process, where the texture roughness and its characteristics induced magnetic orientation of the media. For our isotropic media, the texturing process had zero impact on the magnetic performance because there was no "texture-induced" magnetic effect. I felt that we had a significant advantage over the competition that was practicing the Cr underlayer process. They were at a definite disadvantage in developing the texture process because they had to optimize the process for both tribology and magnetic performance. We only had to worry about the tribology. I believe that this advantage was quite significant for us, as we really excelled in the tribology and reliability performance of the media for quite some time.

2nd Manufacturing Building

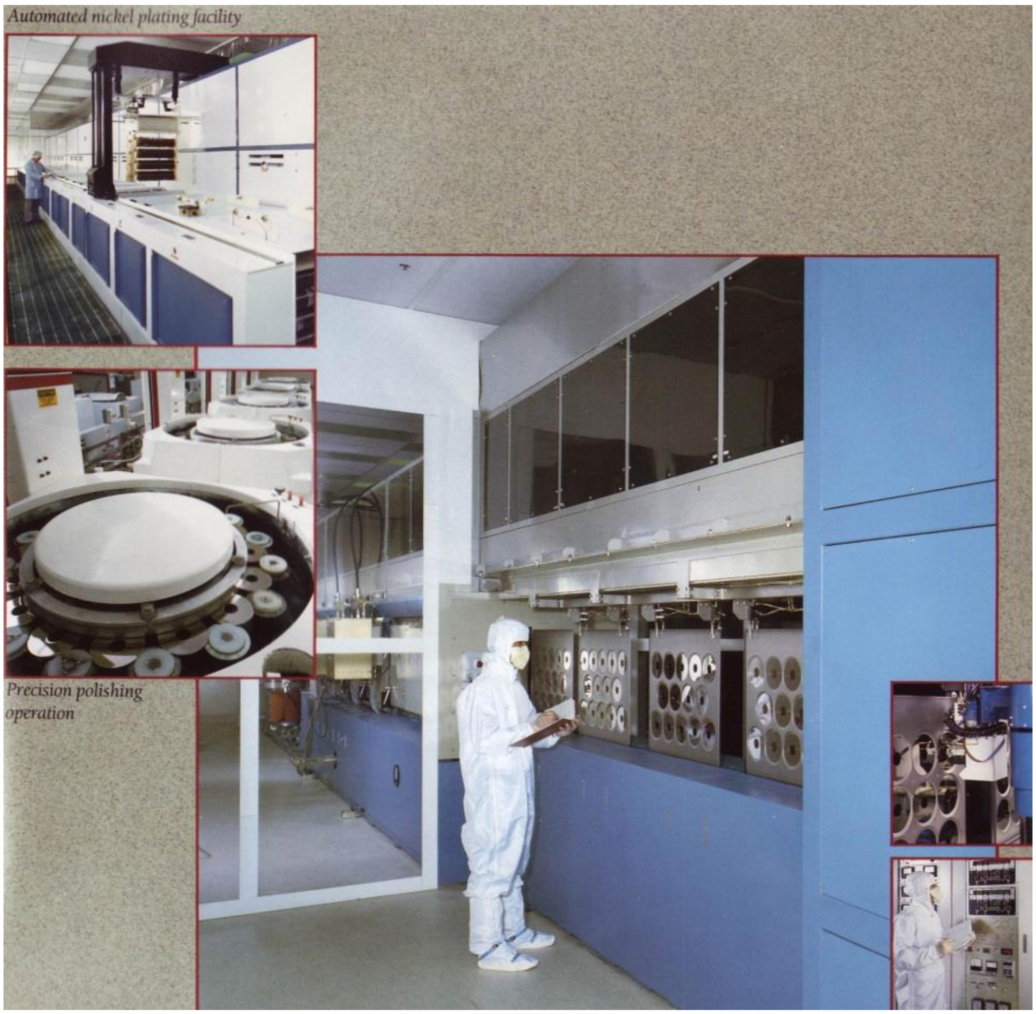
By the latter part of 1986, we undertook to expand production again and built a manufacturing facility nearby, on Hillview Street, in Milpitas. We installed the S3 and S4 systems and they were running smoothly by mid-1987. By late spring we held a company-wide party for all the employees, except for the people needed for a 24 hour production shift. During the party, I noticed that Keith Kadokura, who was a very good sputtering engineer working in the engineering group, did not attend the party. Sensing that perhaps he was having some problems with the line, I called him to ask why he was not at the party. He told me that he was helping a production engineer start a new sputter cycle for the S3 sputtering line, but he was having difficulty establishing a stable process. So before the party was over I rushed back to the company and went straight to S3 in Building Two to find out what was going on. After Keith explained the problem about the instability in controlling the RF reflected power, I started to check the common exciter to see if it had been changed by some operator. I found that the common exciter was set correctly, so the problem was somewhere else. I also checked the plasma in the sputtering chamber, through a viewing window on top of the chamber, and discovered that the plasma was flickering and jumping from one side of the target to another. My immediate conclusion was that there was something wrong with the grounding of the RF components and the chamber. The question was why this was happening then. During the development of S1, ULVAC had noticed the instability problem in the power supplies between the forward and reflected power. The instability was due to the interference of the radiating RF power from the different cathodes and the RF radiation that was absorbed by the metal chamber wall. As a result of this, something on the order of 500 V volts DC could develop on the chamber wall. Mr. Takei at ULVAC had developed considerable experience with RF before, and their solution was to thoroughly ground the chambers to grounding rods placed near the machine with several 5 cm wide silver coated copper strips together with the network boxes.

When we started Building One in Milpitas, we found that a grounding rod of 6 feet was long enough to ground the machine, and we never experienced any grounding problems in two years of production. Therefore, when I speculated that the plasma instability in S3 might be due to a grounding problem, I proceeded to check the grounding straps that were connected to the grounding rod, to make sure that it had not somehow gotten loose. When I touched the strips at the grounding rod I received a nasty shock. When I measured the voltage on the strips and the ground it showed about 500 volts DC and the contact between ground rod and the strips was fine. I also checked all the other grounding rods: they all showed the same results. So I concluded that somehow we had lost the grounding of the rods and telephoned Rick Austin, our facilities manager, right away to ask him what we might have done to the facility to cause this. Earlier in the year we had had a considerable water drainage issue with the building and water was getting under the vinyl flooring and the concrete, and the flooring was literally peeling off. He had installed a water barrier between the concrete and the vinyl flooring. This much I knew, but it didn't explain why the grounding rod was losing ground, it was 6 feet deep. This was when Rick told me that he had dug a drainage ditch all around the building as well, to drain the water away from the building. The Milpitas area where our building was built was once a swamp and drainage was quite poor in many areas.

When the drainage ditch was put in, apparently the water level in the ground was reduced sufficiently so that the entire grounding rod had lost conductance to ground and this was what was causing the plasma fluctuation. I was quite mad by then, of having this sort of action being taken on the building without any discussion as to its possible consequences on the process. It wasn't the first time the manufacturing group went ahead to do things without first discussing it with engineering and R&D, and we suffered the consequences. I called Phil Gahr in the middle of the night to make sure that he and Rick Austin got the electrical contractor lined up the following morning to install deeper grounding rods next to the sputter tool. The following morning, the contractor brought in several 20 foot copper rods to sink into the ground around the S3 and S4 sputtering systems. The rods were so long that they had to start on the roof top to drive in the rods into the ground. Once the new rods were in place and connected to the grounding strap, the system started up without any issue. The plasma was stable again.

This incident showed again how important it is for a manufacturing operation to work closely with R&D and engineering when making any modifications to the building, facilities and equipment. Our manufacturing people were not very willing to follow this simple rule even after so many incidents that caused big problems. In 1990, we invited Craig Barrett of Intel to join our board and tutor and mentor us on how to become a world-class manufacturer. He stressed the importance of manufacturing and R&D working closely together as a team, even before the process is implemented, and that manufacturing must copy exactly everything in minute detail when propagating manufacturing lines in all the locations, so it not only reduces manufacturing problems companywide but also makes it easier to trouble shoot the problems when they occur. I know that Intel practices this rule, and it is the reason why it became a world-class semiconductor manufacturing company.

In 1986, our sales reached $19.5 M, and we turned profitable by the third quarter. In 1987, we had $47M in sales, we had our IPO in May, and we raised close to $40M for further expansion. All the hard work that went into the media process starting in 1983 was starting to pay off big dividends by mid-1987.



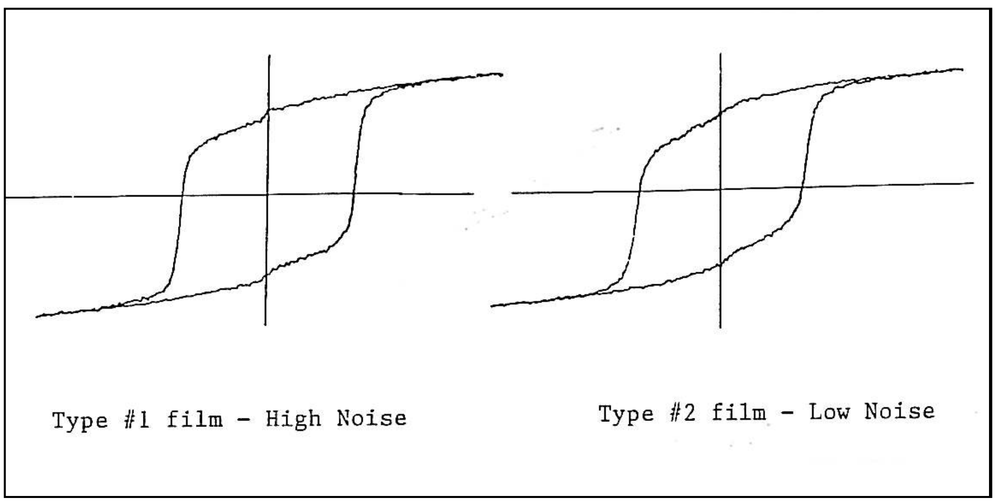
Top Left Inset:	NiP plating line made by HBS.
Bottom Left Inset:	Substrate Polishing machine made by SpeedFamCorporation.
Main photo:	S3 Sputter system in Hillview Street facility, panels loaded with 5.25-inch disks

(Figure 7-1)

1,200 Oe Media and the Signal-to-Noise Ratio Problem

By 1988, the areal density requirements set forth by customers had reached around ~100 Mb/in^2. We needed a new medium with a coercivity of around 1,200 Oe, higher than the 950 Oe that we had been shipping. Although we could easily have raised the coercivity of the media by increasing the Pt content, the signal-to-noise ratio performance was no longer adequate with the structure that we had been using, which was SiO_2/CoNiPt/Carbon. The figure of merit for the recording performance was actually based on error rate measurements using a Guzik spin stand. It was based on the bit shift value, extrapolated from error rate measurements taken at successively higher error rates. The media constructed using the SiO_2 seed were quite noisy and a new type of media was necessary in order to qualify the media into customer programs. Fundamentally, we knew that the microstructure of the magnetic media itself was at the heart of the problem, and the SiO_2 under-layer or seed clearly showed evidence of more inter-particle exchange as manifested by the very square hysteresis loop. There were basically two ways to go about improving the situation, the first of which was to somehow isolate the grains themselves in the magnetic film, or to use some other seed that would not couple the grains as much. We had already employed higher sputtering pressure and an additional increase in sputtering pressure did not provide much benefit. We either had to introduce a change in the magnetic alloy itself, or grow the film differently using another type of seed layer.

At the start of Komag, I had tried using a NiP target to do a glow discharge, which was a low-level plasma discharge, before sputtering the magnetic layer. This surface treatment caused the film to grow with a more perpendicular crystal orientation. But we knew how to suppress this tendency by then, using nitrogen doping and also by control of the residual water level in the system. It was worth reconsidering sputtered NiP as a seed layer for the magnetic layer. The substrate we used was made from an aluminum alloy, which was electroless plated with about 15 micrometers of NiP, with about 12 wt.% phosphorus. This film was put on to provide a hard and corrosion-resistant surface on the aluminum, which was polished to a mirror finish. The material properties of the film were excellent, and it had an amorphous structure, meaning that the atomic ordering of the NiP was random and did not have any crystalline structure. We could not obtain a good magnetic structure with good properties if we deposited the magnetic film directly on this surface, due to inhomogeneity that existed on the surface, caused by polishing and cleaning of the surface prior to sputtering. As was usually the case, the surface was probably oxidized as well, and it was difficult to determine just what sort of surface we actually had for this plated, polished and cleaned NiP layer. In order to study the effect of sputtered NiP on magnetic film growth I tasked Tom Yamashita to conduct a quick investigation of this problem, as I relied on him to do this type of exploratory work since the beginning of Komag. The S02 and S03 batch sputtering tools were used to deposit the NiP on the polished disk substrate before depositing the CoNiPt magnetic layer to see if one could improve the recording performance. When the medium was all made *in situ* (that is, both the NiP and CoNiPt were deposited together in the batch machine), its performance was not very good. By then we realized however, that the deposition conditions in the batch machine were quite different from what was being achieved in the in-line production tool. In the production tool we had the effect of dynamic deposition where the disks were made to slowly pass by the target as it was being deposited, and we also used nitrogen gas to adjust coercivity and crystallography. In addition, there was the issue of residual gas, especially the water vapor in the in-line tool, which was higher than in the batch system. However, we used the batch tool because we did not have the NiP target in the in-line tool. In order to simulate the in-line tool, Tom did the next best thing, which was to deposit the NiP in the batch tool, then transfer the partially processed disk into the S1 in-line tool, and deposit the magnetic layer and overcoat there. When this disk was tested, it showed the best signal-to-noise ratio performance we had ever seen. We were on to something. When we looked at the hysteresis loop by a VSM, it showed the telltale sign that the microstructure must have been less exchange coupled, as the squareness actually degraded somewhat. The micro-magnetic theory I had developed at Xerox PARC was still operative (Figure 7-2).



Comparison of the magnetic hysteresis loop between high noise (more square) on the left vs. low noise (less square) media on the right. The difference in hysteresis loop is quite subtle, but it is quite distinctive, and very recognizable from the micro-magnetic model that I developed at PARC. (Figure 7-2)

The next step was to reproduce the results in the S1 sputtering system. We first had to create a NiP target. The expedient method of creating the NiP target was to plate the NiP onto an aluminum plate the size of a regular target using our plating line, just as we did for the aluminum substrate. We could make the target this way very quickly but it would not be really practical because the amount of material we could plate was limited by the slow plating rate of the electroless plating method. We needed a target that was at least a quarter of an inch thick to run in production, but for the purpose of testing, we could make do with less than 1 mm thick plated NiP on an aluminum plate the exact size of the sputter target. When we finally got the target ready in S1, the sputtered disk using this target was not quite as good as the ones made from transferring the partially coated disk from the S02 batch tool, but it was still good enough to qualify for the customer's program. The effect was still there and we decided to proceed with it. In order to make the production target, we needed NiP powder so that the target vendor could fabricate a metal target using the powder, which could have been hot pressed into the form that we needed. The problem was that no vendor had NiP powder. It did not exist. I decided then that we had to make the powder ourselves and we decided to use the spent nickel-plating solution that we had in abundance in our plating line. All one had to do was to precipitate out the nickel and we would have the NiP sludge that could be used for making the targets. Soon, we had enough dried out sludge to provide to the target vendor so that they could ball mill it and create a fine powder to consolidate it and sinter it into a NiP target. When this target was tried we obtained media with a better signal-to-noise ratio than with the plated NiP target, and as good as disks made with the NiP sputtered in the batch system. This process then became a new process for creating the 1,200 Oe low noise media that was continuing to ramp up our business to new heights.

One key problem we encountered using the NiP targets made by this method was that the targets were not very consistent. Some batch of targets would work very well in terms of noise, while another batch would not. This was a big head ache in production, as we would have to test out each new target in order to see if it would work. Luckily, due to using RF sputtering, the target would last quite a long time, for at least a month or more, so that some extra time spent in checking the individual target was still workable in production. The performance of the target in terms of signal-to-noise ratio always followed the batch of powder that we generated. Somehow, each spent nickel solution was somewhat different. We could see this, as some powder batches would come out quite hard and lumpy while other powder batches came out quite malleable and crumbled easily. We could not figure out why this was happening even though we had two very capable Ph.D.-level engineers assigned to track this phenomenon. The difference in NiP targets influenced how much exchange decoupling the film conferred on the CoNiPt film. The ones with very good signal-to-noise ratios sometimes had too much exchange decoupling so that their hysteresis loop was quite non-square and sometimes OW suffered as a result. Something about the composition of the NiP was affecting the way the CoNiPt film was growing. We conducted the usual chemical analysis of the NiP powders and also of the target and sputtered film, but for a long time, we could not figure out what the key issue or difference was. This situation actually lasted many months before we figured out what was happening. It turned out that our target vendor, Cerac, who was converting our sludge into powder, was using alumina ball milling to grind the NiP sludge to convert it to the fine powder to make the target. Some sludge lots were so hard that Cerac had to ball mill the sludge for a long time in order to turn it into powder. We actually had been using the terminology of calling the NiP target "hard" or "soft" based on the degree of difficulty Cerac was having in creating the powder. We knew that a "hard" target always performed better in terms of signal-to-noise ratio compared to the so-called "soft" target. The difference, as it turned out, was due to the amount of alumina that got incorporated into the NiP powder during the ball milling. Because we used RF sputtering, we could sputter NiP as well as a ceramic phase, such as alumina, which was being incorporated into the film. Somehow, the base NiP layer was being affected micro-structurally so that the CoNiPt film nucleated more uniformly and the grains were more isolated with the presence of the alumina phase in the NiP film. Aluminum was always being detected by chemical analysis on the NiP powder, but we could not measure oxygen. We always had assumed that the aluminum was a contaminant that came with the spent NiP solution. The difference between the so-called "hard" and "soft" targets was not so much that we could notice that hard targets in fact contained slightly more aluminum (from the alumina).

Rather than using the spent plating solution, Cerac eventually figured out how to synthesize a pure NiP compound and adjust the composition of phosphorus from 12 to 20 wt. %. We eventually settled on around 15 wt. % phosphorus, and we deliberately added alumina powder into the target. We kept the sputtered NiP a secret for a long time, so that our competitors doing the Auger depth profile, for example, on our media would only see the magnetic layer followed by NiP, which had roughly the same composition as the plated NiP. So for a long time many people thought that we deposited the CoNiPt film directly on the substrate. It was not appreciated outside of Komag just how important the NiP "nucleation layer" was for the proper crystal growth of CoNiPt. The sputtered NiP underlayer process was worked on by many people at Komag for many years. Some of the best people that we had worked on various aspects of this process, from sputtering to target manufacturing, and fundamental work on the materials science and characterization of this film. Some of the key players were Tom Yamashita, Ga-Lane Chen, Keith Kadokura, Joseph Yuen, Lam Tran, Rajiv Ranjan, John Chen (my son), Miaogen Lu and Gerardo Bertero.

<u>Oxides to isolate the magnetic grains</u>

I previously described the hard fought battle to understand and control the amount of water vapor in the sputter chamber to control the magnetic properties of the CoNiPt film.

Initially we managed this by controlling the outside humidity of the sputtering system. Although this was a necessary step in manufacturing the media, we always thought that we could do better if we could directly introduce the oxide and control its formation inside the sputter chamber. There were two avenues for doing this, but both had many issues. Therefore, it took us several years to perfect the method.

The first step was to directly introduce water or oxygen into the plasma during magnetic film growth so that the media would incorporate the oxide as the film was growing. Practically speaking, this turned out to be quite difficult to do. One approach was to use oxygen directly but, as hard as we tried, this never worked. The difficulty was that the oxygen was very reactive, and it would immediately get consumed at the point of introduction into the plasma. We tried various manifolds and means to try to make the oxygen introduction uniform but we never achieved good results. The second method was to introduce water vapor directly into the plasma. This is the approach we finally succeeded putting into practice in manufacturing, but not without much pain and suffering. First of all, the water had to be in vapor form, and one required a specially constructed mass flow controller (MFC) designed to control water vapor delivery into the sputtering chamber. Once this was achieved, uniformity and reactivity were also critical factors in introducing water vapor into the plasma. Much of this work consisted of trial and error in designing the right manifolds, so that the right concentration of water vapor could be introduced into the plasma, and so that the uniformity of the effect would be even across the entire panel. Komag was helped by the fact that we had used an in-line deposition tool, where the panel with disks slowly passed by the plasma created by the target. The deposition rate was relatively slow, so that there was time enough to do reactive sputtering and allow the gas to distribute itself more evenly. Water vapor was much less reactive compared to oxygen, so that it had time to diffuse and even itself out before reacting. The third advantage we had was that we were using RF deposition. The plasma tends of be much more spread out compared to DC magnetron sputtering, so that it is more advantageous to do reactive sputtering using RF compared to a DC magnetron. We also did not have to fear target contamination and oxidation. RF can sputter metals and non-conductors just as easily. With DC magnetron sputtering, reactive sputtering can cause the target surface to become contaminated or to form an oxide, which then would not sputter and would eventually cause arcing and create spitting, resulting in defects on the disk.

We eventually used water (vapor) injection to control grain isolation in the magnetic layer as well as in the NiP seed/nucleation layer. We conducted extensive TEM analysis of our films. Controlled oxidation of the magnetic film played a very important role in controlling the grain size, uniformity and isolation. Grain size and uniformity were also controlled by having aluminum oxide in the nucleation layer. These two aspects of the film's microstructure control were critical in continuously advancing the areal density capability of the media.

The second means of introducing oxide into the film was through the target. We did this first with the NiP target, but with the magnetic target it was not so easy. The difficulty had to do with the way the target had to be manufactured for the magnetic layer. We needed the magnetic target to be of very high quality in order to ensure uniform composition and performance throughout the life of the target. For example, in a CoNiPt target, a composition difference of only 0.1 at. % Pt could result in Hc being different by as much as 100 Oe. Since we had to use the target for over a month, and it contained very precisely controlled amounts of each component, the target manufacturing method and composition control of the target were of utmost importance. In fact when we started Komag one of the biggest worries was target manufacturing. Being a metallurgist and materials scientist by training, I knew all about the phase diagram and what might happen to the target as it was being manufactured. The preferred method of fabrication was vacuum melting because it could create the highest theoretical density, but the cooling process after melting was a big concern. If the composition and phase diagram had complications, one could phase segregate different composition phases inside of the target. Because of this concern, I taught the vendors, such as Hitachi Metals, about using continuous rapid cooling as the molten target material was cast to prevent long-range segregation inside the target. Hitachi Metals then filed a patent on it without my knowledge or permission. Luckily, the CoNiPt target made by vacuum melting did not have such issues and we were able to go into production without having many problems. The very first set of CoNiPt targets were made by a small target manufacturing company in Morgan Hill, Demetron, and these targets were made by hot pressing or sintering. There was no segregation issue with this method, but the concern was with powder mixing and density. Hot pressing during this period was not very dense, and the target could have had a considerable amount of porosity, which was considered problematic in terms of purity and defects. Because of these concerns it was not so easy to consider radical changes in target composition, without some knowledge of the phase diagram. When one goes above 3 elements, the phase diagram data usually do not exist or are difficult to predict. Also, due to the fact that it had to be made by vacuum melting, one could not add alumina particles as we did for the NiP target. The high melting oxide particles would not mix well into the target. And the subsequent rolling and shaping that had to be done on the ingot usually caused the target to crack. All of these difficulties conspired to slow down the progress on magnetic target composition changes.

In order to introduce oxide and other elements into the target, one had to fundamentally change the way the target was manufactured. The right method to use for making the target was hot isostatic pressing (HIP). In mid-1980 when we started, the HIP method was reserved for forming high value added components such as titanium jet engine parts. HIP machines work by using gas to isostatically press parts into shape using pressures up to 20 K psi. The chamber needed to achieve such pressures was basically the size of large artillery gun barrel, and one needed to house this in a bunker to protect against accidental explosion. There were very few HIP facilities in the U.S., and no target vendor possessed such an expensive piece of machinery. We did however start to investigate how we might be able to HIP the magnetic target under the leadership of Jim Shir. We had several engineers look into the method, working with vendors that had the HIP machine. Eventually, a local vendor, Leybold Materials, which took over Demetron, decided to invest in the HIP tool and associated equipment in Gilroy, CA to fabricate the target. In order to HIP a target, one needs a starting powder that is appropriately sized. A powder mixing facility and know-how are needed to process the powder. The powder has to be placed into a "can" to hold the powder. The can has to be then placed into the HIP tool to be pressed. There are technology and know-how needed in fabrication of the can for the specific composition that is used. Once the can is pressed, the can's skin is peeled off from the ingot, and the ingot then has to be sliced into thin plates for the target. Electro-discharge machining (EDM) was developed to slice the large size ingot into plates. All of this took many years before it could be perfected for making targets. Once they were ready, we began to experiment with different compositions and addition of various oxides, as well as other elements, including very light elements such as boron. We were one of the first in the world to start using magnetic alloy compositions that incorporated oxide and boron into the target. This was a necessary step in continuing to improve grain isolation, as the oxide made its way into the grain boundaries to further isolate the grains, and boron aided the diffusion of segregants. None of this would have been possible without going to the powder metallurgy and HIP process to manufacture the targets. The reason why I say that we were way ahead of the game is that today, the perpendicular media that are used in 100% of the drives today, all have oxide as a segregant in the magnetic layer. It would not have been possible to convert the industry to perpendicular media without the oxide segregant. This will be described in more detail in Chapter 12.

Chapter 8

Komag Offshore Operations, Business Partners and Substrate Technology, Lubricant and Overcoat Improvements

AKCL Joint Venture with Asahi and VMC

After the IPO in June of 1987, we were very busy executing the expansion plan to capture greater market share. As I mentioned earlier, we had formed a joint venture with Asahi Glass and Vacuum Metallurgical Company (VMC) in Japan at the beginning of 1987, after our third round of fundraising in 1986. After forming the joint venture company, I went to Japan to work with Asahi Glass and Mr. Takei of ULVAC to look for a suitable location to set up a factory in Japan. According to the JV agreement, Asahi Glass was designated to manage the joint venture company, now named Asahi-Komag Company Ltd. (AKCL), so they organized a team from their employee pool to staff the new company. By the summer of 1987, a factory site was chosen in Yonezawa, Japan and the building was designed and construction started. By June of 1987, a team consisting of about 30 people from Asahi Glass recruited to lead AKCL arrived at Komag to receive training in manufacturing disks. They also had to work out the details of factory layout and order production equipment as well as the additional R&D tools needed to conduct their own development. The team was headed by Asahi Glass veteran Nishikori and Shinkai-san as head of the technology team. The team members were all quite young and had been with Asahi for only few years at most, but they were all quite well-educated and talented. Nori Shinkai had obtained his engineering degree from Tohoku University and Ph.D. from Penn State in ceramic engineering. He was instrumental in leading the young group of engineers and getting the technology transferred. The team was able to quickly absorb all the technology and know-how.

In the early 1980s, the Japanese government selected Yonezawa as one of several locations in Japan to develop what was called “the U-turn policy”. The policy was to encourage Japanese industrial companies to move their new factories away from crowded places near major cities such as Tokyo, Osaka and Nagoya for example, so that people from the countryside could find well-paying jobs near where they lived. The central government, working with regional governments, set up industrial estates with good tax incentives to build factories further away from the traditional industrial centers of Japan. At the time of the founding of AKCL in 1987, Asahi Glass had just finished a plant in Yonezawa to produce glass for various consumer products and they had extra land to build a new plant for AKCL. When I first visited the plant site in the summer of 1987 at Yonezawa with Mr. Uchino, the vice president of Asahi R&D and a senior director of Asahi Glass, I was overwhelmed by the beautiful scenery of lush green rice fields and hills. I was very impressed by the extremely clean air and green scenery and I decided right away to accept the proposal from Asahi to build the AKCL factory there. However, when I returned to Yonezawa in the middle of winter in January 1988, I was surprised to find that thick snow covered everything.

By January 1988, the AKCL factory was ready to start as planned. Since I had committed personally to see to it that the team would succeed, I went to live in Yonezawa for about 6 weeks to provide advice and training to the team on-site. I brought my wife, Nancy, along as well. AKCL had leased a brand new condominium apartment in Yonezawa city so that we could live comfortably there during our stay. The place was in downtown Yonezawa and there were piles of snow several feet high on each side of the street. It was rather difficult to walk around but Nancy bought special snow shoes to be able to walk around the town. She would go shopping every day to find interesting food to cook. I should mention that Nancy is a superb cook, and she made delicious meals for me every day and occasionally we would invite our friends from AKCL. Most of the young men recruited to work for AKCL were still bachelors. Older experienced engineers and managers came to work in Yonezawa, leaving their families at home near Tokyo, so they were also like bachelors; therefore I am sure they appreciated very much Nancy's superb cooking. They would always come despite my continuing lectures and haranguing them over details of things that they must pay attention to over dinner. Yonezawa is located in the northern part of Japan in Yamagata prefecture. The area is very famous for rice production and ranks at the top for the quality of rice and local sake in Japan. The area is also known for heavy snow. The countryside is beautiful and the area has a very rich history as well. After dinner, we would enjoy the famous Yonezawa cold sake together. Nancy would leave bottles of sake outside the window to chill and the cold sake was superb. Our time in Yonezawa was one of the best times Nancy and I had since starting Komag. After my 6 weeks in Yonezawa, I did a baton change with Tom Yamashita and he stayed at the condominium for a few weeks to continue with the training and start-up of the factory.

The first product that we made from AKCL was the 3.5-inch (95mm) media intended for IBM. The drive using our media was to be made in IBM's drive factory in Japan. The drive was one of the first small form factor drives IBM ever made, code named the Kazusa 2 drive, with a capacity of 120 MB and it was one of the earliest drives to use media made by an outside vendor to IBM. We were one of the few outside vendors to be qualified into an IBM drive. To qualify as a vendor to IBM was very difficult. IBM's drive engineering team demanded that the media pass very stringent corrosion resistance tests, which our standard media at the time had a very difficult time passing. Fundamentally, our disk used a CoNiPt alloy, which was weaker against corrosion compared to competitive media using alloys with chromium or with a chromium underlayer. However, IBM still wanted to use our disk despite the corrosion weakness, probably due to the superb tribology performance we had over our competitors. Nevertheless, we had to try to overcome our corrosion weakness. The initial approach to this issue was to insert a "passivation" layer of corrosion resistant metal such as chromium, titanium and tantalum between the carbon and magnetic layers. Because inserting this layer added to the spacing loss, one could not use too thick a layer. After many tries however, we simply could not pass the IBM corrosion tests without making the passivation layer too thick or affecting the tribology of the carbon layer. Somehow, inserting the passivation layer under the carbon actually made the tribology much worse. Tom Yamashita was the one working on this project and after few weeks of failures with this approach and becoming quite frustrated, he proposed that we try something else, which was to use a ceramic-based overcoat using stabilized zirconia ($ZrO_2 + Y_2O_3$). The technical term for this material is *yttria stabilized zirconia*. Both Jim Shir and I thought this idea was quite crazy. Nothing was known about using such a material as an overcoat and we insisted that Tom continue working on the carbon-based overcoat. As it turned out, Tom decided to work on this material secretly, and presented to Jim Shir media coated with zirconia for him to test after a few weeks. To everyone's amazement, the first disk made with this overcoat showed fantastically good corrosion performance, and with the ferrite head that was to be used in the IBM drive, the CSS performance exceeded 100K cycles without any degradation. We never saw anything like it. With these data, we decided to go with the zirconia overcoat, and submitted parts to IBM for qualification. We qualified the media in April of 1987 from parts made in the U.S. The reason why Tom was interested in trying stabilized zirconia was because it was something that he had worked on while at Stanford University and he was very impressed with its bulk characteristics. But making sputtered thin films from it had never been done to our knowledge, certainly not for a disk applications, and it was quite a stroke of luck that it worked as well as it did. It paid a big dividend for us.

However, as the saying goes, "good things don't come easy". Shortly after qualifying the U.S.-made disks, but before start of volume production, IBM Japan ran into two serious problems with the drive: 1. severe stiction problems caused by the disks, and 2. severe particle contamination caused by the ferrite heads made by Alps. These problems quickly became a serious crisis. The Kazusa 2 drive was the most advanced 3.5-inch drive in the industry at the time. It had been committed for use in one of IBM's most important PCs of that day. This PC product was expected to generate revenue of $3.5 billion. A delay of Kazusa 2 would delay the PC product and this was unacceptable.

When the media stiction problem surfaced for the US-made disks, we made the determination that it was caused by condensation inside the disk cassettes during air shipment from U.S. to Japan. This occurred because of large temperature and pressure changes in going from the cargo hold in an airplane to the hot and humid conditions in Japan around the time when volume shipment had started. Initially we did not have a problem because perhaps it was early in the season in 1987. The new zirconia overcoat was more sensitive to pick-up of environmental contaminants compared to the carbon overcoat and also more sensitive to condensation problems. To correct the condensation problem, we did change our packaging so that cassettes got a seal so that we can evacuate air, followed by back fill with nitrogen. We did tell IBM of this issue and the fix, but the explanation that zirconia overcoat is more sensitive to contamination might have left them with the impression that we had a problem with other contaminants inside our U.S. factory. Initially the media to the program was to be supplied from the U.S., but we also had production problems in meeting the IBM volume requirements, and told IBM of our problems. The ulterior motive was that we really wanted to ship out of AKCL because they needed to start shipping as soon as their factory was ready. However, this required a formal qualification of the AKCL disks by IBM and they were notoriously slow in such matters. We basically used the "contamination issue" and "production issue from the U.S." as a ploy to see if AKCL could be qualified sooner. IBM did not take this very well obviously. Our media issue and the Alps head issue were very quickly escalated to the office of John Akers, the CEO of IBM. He assigned Chris Bajorek to solve it. At that time Chris was managing the development of IBM's mid-range disk drives in Rochester, MN. I first met Chris when he visited Komag in Milpitas, California to review Komag's actions to diagnose and eliminate the stiction problem, and the production issue. Chris was justifiably very unhappy with our U.S. disk situation. We jointly decided the best option would be to switch the supply of the disks to AKCL. AKCL would have plenty of capacity and it was what we wanted. Also being in Japan, we would not have as serious a condensation problem in shipping by land to IBM Fujisawa. But we would have to mount a heroic effort in order to qualify the AKCL disk. IBM, under normal circumstances, could not have moved so quickly.

Unfortunately for Chris, he also faced a dire situation on the head supply as well. The Alps heads were shedding particles during temperature humidity tests. Alps used a very fast grinding process to make the heads. This process damaged the surfaces of the heads and particles were being generated. They had concluded that this was a stress-induced corrosion of the heads' surfaces. In reality in hindsight, the problem was most likely caused by our zirconia overcoat, which was composed of crystalline particles of zirconia which could be quite abrasive to the head if it was not processed properly. Grinding processes can leave a high level of damage on the slider surface and our zirconia overcoat was literally polishing off the surface of the head, thereby shedding the damaged layer of the slider. In any case, Alps would not have been able to modify its process in a timely manner, partly because of the required development time and partly because the solution required a slower, more gentle process, which would have limited Alps's supply capacity. Fortunately, Chris determined that TDK was able to offer an equivalent ferrite head, made via slower grinding, that did not shed particles. But TDK's heads did not use suspensions compatible with the head mounting requirements of the Kazusa 2 drive. The drive required suspensions supplied by Hutchinson Technologies, made in Hutchinson, MN. TDK had never used a Hutchinson suspension before then.

The long and short of it, Chris Bajorek ended up having to manage fast track qualification processes at multiple companies: a new disk from AKCL, a Hutchinson suspension and the ferrite head from TDK, and the ensemble of these new components at IBM Japan. Fortunately, the qualifications were completed in a record short time, in only two months. This was extraordinary for IBM to accomplish and it is when I started to appreciate Chris Bajorek's abilities. Chris had made six round robin trips between various parts of the U.S. and Japan during this period to manage the problems that covered the cities of Rochester, Milpitas, Tokyo, Fujisawa, Yonezawa and Nagano. Within IBM Japan (Fujisawa) and at the vendors, they had a secret codename for him, which was "Chris Typhoon" as he would come in to cause a major storm wherever he visited. Without Chris managing the drive program, AKCL would not have gotten off to a good start. AKCL was formally qualified into IBM in February 1988, and began shipping disks. From this time period, I started to think about hiring Chris away from IBM. It took me many years.

In April of 1987, Steve Johnson and I were on our road show to raise money at a bank in downtown Edinburgh, Scotland in preparation for our initial public offering. Steve got a call from California that we just received formal qualification from IBM to use our media on their 3.5-inch HDD product. That was huge for our road show because qualifying at IBM conferred instant credibility and a seal of approval on our product. IBM, and what they made and used, was still the standard against which everyone else was compared. IBM's blessing of our product made our story very compelling for our fundraising effort, and we had a very successful road show the rest of the way in Europe. Qualifying at IBM as an OEM supplier also reassured Asahi Glass that AKCL would have a very significant customer in Japan right out of the gate, as the new drives would be built by IBM's Japan division. It would also give immediate credibility to AKCL for expanding their customer base in Japan.

The zirconia overcoat process had many challenges before it became stable. Anything radically new always has issues, and we had our share of problems to solve, but we continued to work on it while we produced parts out of the U.S, until AKCL could start up and take over production. Having just one product to work on initially gave AKCL a lot of focus and they were able to start shipping products to IBM by April 1988. After IBM, they were able to qualify products into MKE (Matsushita Kotobuki Enterprise) who was contract manufacturer to Quantum and also to Toshiba. The zirconia overcoat was a great success for starting up AKCL, but it did not achieve widespread acceptance in the U.S. After about two generations of product with IBM, we started to observe significant lube build-up problems using the zirconia overcoat. This coincided with a change from a ferrite to a thin film head. We did not know this at the time, but it was due to what is called “catalytic lube breakdown”. This occurs when perfluoro-polyether-type lubricant is rubbed against an oxide material, such as alumina in the body of a thin film head. Much later in time, this issue became more understood, and the thin film head started to be coated with carbon film to prevent oxide in the head material interacting with the lubricant. We solved the problem of lubricant breakdown by reverting back to a carbon overcoat, but this time with high doping of hydrogen in the carbon. We realized by then that high electrical resistivity of the zirconia overcoat helped prevent corrosion of magnetic media and by doping the carbon with hydrogen, made the film very resistive. It was noticed by Tom, Charles Dong and other tribology engineers at Komag, that the water level in the sputter chambers seemed to be affecting the way the carbon would appear (its color) and affect its tribology and reliability performance. With a high initial level of water vapor at the start of the sputter runs, the carbon would take on a slight yellowish hue, and become very greyish as the system dried out. It turned out that this carbon with the yellowish hue had superior corrosion resistance and was also more durable. It was realized that hydrogen from the water dissociation was the key component to this effect and various experiments with hydrogen doping led to a new type of carbon, which was termed “yellow carbon” at Komag. This film had as much as 20% or more hydrogen in the carbon and corrosion resistance was excellent. The electrical resistivity was very high as well, which agreed with our theory about how the corrosion process occurred on the media. Higher resistance film is essential in corrosion protection. Once this new form of carbon film was discovered, it would replace the entire zirconia overcoat product and all the carbon took this new form. It led to a significant reduction in carbon overcoat thickness which helped with the spacing loss, and hence to increasing the areal density. In hindsight, the carbon that we first tried using pyrolytic graphite probably contained significant amounts of hydrogen and was probably performing much better. A pyrolitic graphite target was not manufacturable in large sizes however, and we had gone to the pressed graphite that is typically used for electrodes. This material is essentially pure graphite and produces carbon films without any hydrogen. The carbon films were worse for corrosion, unless some hydrogen got into them from water vapor breakdown. Had we realized this early on, we probably would not have taken the diversion of going with zirconia for overcoat. The zirconia overcoat was used in our MO disks however, because it would also serve as anti-reflective coating to enhance the MO effect and also to protect the super-sensitive TbFeCo magnetic film from corrosion in high humidity environments, as it was designed as removable data storage like a CD or DVD. Although the MO media was not a commercial success, the coating structure of the media was quite superb in terms of its performance. The MO disks that we made over 20 years ago still function in archival storage drives.

Improving Manufacturing Productivity

AKCL brought tremendous benefits to the overall business for Komag. AKCL employees worked very hard and systematically on process and manufacturing using the Kaizen methodology, so that by 1989, they started to generate significant profit and expanded their capacity quickly. Most impressive was that they achieved machine utilization near 90% and yield above 85% by the latter part of 1989. We were nowhere near such performance in our factory in the U.S. This was something that had been talked about in many industries--about the virtues of manufacturing prowess that Japanese companies possessed--and it was my first experience in seeing it firsthand. We had to get this know-how and culture back into our Komag operation in order for us to survive for the long-term.

Sometime around 1989, customers such as Western Digital and Quantum started to approach us about developing a future growth strategy as a strategic partner to them. Manufacturing key components such as media and heads took a great deal of technical know-how and capital. Unless you were a very large and well-capitalized company such as IBM, Hitachi or Fujitsu, vertical integration was not yet in the cards for most of the smaller drive companies at the time. At one of the Diskcon conferences, in either 1988 or 1989 in San Jose, Chuck Hagerty who was the CEO of Western Digital gave a keynote speech in which he said that Western Digital would only focus on its core competency of making hard disk drives and have "virtual" vertical integration with their key component suppliers; as opposed to Seagate which was clearly pursuing actual vertical integration at the time. Perhaps it was what a smaller company had to say to their investors, but we did listen carefully to his comments. Chuck Hagerty came to talk to us then and his key message to us was that in order to expand the market penetration of PCs, the price had to come down from two thousand dollars to less than a thousand dollars. Therefore, he said, the price of hard disk drives that were in the several hundred dollar range had to come down to a hundred dollars or less. Consequently Komag needed to develop a strategy of pricing the disk for less than $10 a disk by early 1990. At that time, we were selling disks for around $20 and we had a gross margin of around 20%. We could not quite envision a pathway to achieving anything like the $10 selling price for media that Chuck Haggerty was suggesting.

After the Western Digital message, we huddled many times to discuss how we could sell disks for $10 and still keep our margins. We held several offsite meetings with members of senior management. At the meeting, our operations team showed that labor costs accounted for about $3 out of $13 total cost, but based on 85% line utilization and 65% yield with recycles calculated in. It was argued after two days of discussion that we needed to go offshore in order to reduce our high labor cost in the U.S. Our COO, Will Kaufman who was previously the COO of Intel before joining Komag, suggested that from his experience in establishing and running Intel's Penang, Malaysia operation for several years, labor costs in

Penang were one tenth of what they were in the U.S. Our line operator cost us over $2,000/month while it would cost only $200/month for an operator in Penang. This seemed quite convincing to most of the senior management, therefore they were in favor of establishing a Penang production facility as soon as possible. I however, strongly objected to this reasoning. I argued that it is much more important to become a world-class manufacturer first, namely achieve very high line utilization and high yield (in the U.S.) before contemplating going offshore. If we simply move the existing low productivity and low yield process overseas, we would never achieve low cost. If anything, things would go much worse in overseas if we had poor productivity and yield. I believe that I had a good argument because we were already seeing AKCL achieve higher productivity yield and it served as good benchmark for what is possible in the US. We were already seeing lower cost per disk at AKCL than in the U.S. despite their high labor costs. They were achieving this through high productivity and higher yield, which was a hallmark of what Japanese industry and companies were doing to U.S. competition already across many different fields. I proposed that we should immediately develop a plan to learn the manufacturing know-how and methodology of AKCL and improve our manufacturing process first for several quarters before embarking on overseas expansion. Unfortunately only a minority of directors at the meeting accepted my proposal and the majority sided with the proposal to go to offshore manufacturing immediately.

After the offsite meeting, the entire company moved quickly to develop a plan for offshore manufacturing, which would start construction in 1990 and production in 1991. Will Kauffman took his facility director to visit several locations in Asia, which were Thailand, Taiwan, the Philippines and Penang, Malaysia to select the best offshore production site. The finance group started to put together a financial plan so that it could be presented to the board for the 1989 spring board meeting. This task was assigned to the finance department. Two weeks before the board meeting, they provided me a draft of the proposal and I started to review it while Nancy and I headed to Oregon for a week of vacation. As I started to look at the numbers, it became immediately obvious to me that the numbers were all wrong. The plan showed that we could achieve a 30% profit margin with two production lines in Penang, using the U.S. productivity and yields with a selling price of $10/disk. We weren't achieving anything like that sort of number, even with a $20/disk selling price in the U.S. After checking the assumptions used in the calculation, I quickly saw that a mistake was made. The number used in the plan for the output from a two line investment was actually the capacity for three lines, and the expenses were based on two lines. It really shocked me that the finance department could make such basic error and it was only two weeks before the board meeting. Basically the simple arithmetic said that if the correct numbers were used the Penang operation didn't make financial sense.

After coming back from the trip, I told the finance department about the mistake, and they re-ran the numbers and everyone was finally convinced that the offshore operation by itself with lower labor costs would not be sufficient to lower the overall cost of the disk unless we made drastic improvements in our productivity and yield. Fortunately we had the AKCL operation as our benchmark now so that I was able to use their financial results in the board meeting to convince the board that we needed to set a target for the Komag operations team to achieve the yield, utilization, and operator productivity comparable or higher than AKCL before we could start to consider moving offshore. The targets we set were 90% utilization and 85% yield or higher before the end of 1990. Only after we achieved such results should we set our sights on offshore operation. After I made this proposal, all board members unanimously approved it and told us to carry it out. Some of the board members were quite helpful in supporting our effort to achieve a first class manufacturing capability. Max Palevsky suggested that we invite Craig Barrett, then COO of Intel, to become a board member and help us become a world-class manufacturer like Intel. Masayoshi (Masa) Takebayashi of Kobe Steel and George Neil of Asahi Glass, who were on the board as observers, also offered to send a team of Kaizen experts from their companies to work with our manufacturing team to implement Kaizen practices in Komag. Masa Takebayashi personally led the Kaizen team to help us. Both Kobe and Asahi were very generous in that they covered all expenses for their employees from Japan who came to work with us. Even though Asahi and Kobe had invested an extra $20 million to buy one million shares of Komag after its IPO in 1987, they were still a minority shareholder and only had observer status on our board. Nevertheless, they were willing to help us as a partner in the business, with the expectation that a stronger Komag would also help with their investment and business relationship with us. I really appreciated this sort of business thinking and philosophy that a company like Kobe and Asahi had, which was typical of many Japanese businesses. American businesses rarely operated like that. Sadly to say however, I always felt that even within Komag, the value of such business practices and relationships were not appreciated enough.

Both Asahi and Kobe sent two experts each, and Masa Takebayashi personally led the entire team. Craig Barrett also visited us and made some rather pointed remarks about what he saw in our line. With their help, we were able to achieve the productivity and yield goals that we had set, and were then able to proceed with our dream of setting up an offshore manufacturing facility in Penang starting in 1991. We commenced production in 1992. This experience base allowed us to build our second media factory in Penang and a world-class aluminum and NiP plated substrate production facility in Sarawak, Malaysia, in 1995. The foundation for these achievements rests with having a world-class manufacturing capability based on Kaizen principles, which constantly strives for higher productivity and yield.

Developing Substrate Technology

In media manufacturing, the substrate and media operations are often separated as distinct operations. In fact, it was not unusual to have a completely separate business that made substrates and media. Some media companies bought 100% of their substrates from external substrate manufacturers for example. A substrate consists of two parts: Aluminum and subsequent NiP plating and polishing. Even on this second step, it was not unusual to have two separate businesses, one making an aluminum substrate and a separate business doing the NiP plating and polishing. The story of the technology surrounding the substrates goes back to my Xerox days when I was working with ADL to develop the plated disk for LCDM in mid-1970 that I mentioned in Chapter 4. During the time when we were developing the LCDM disk, the aluminum substrates that were bought commercially were designed to have high strength and stiffness specifically for an oxide media memory disk application. Based on classical metallurgical principles, aluminum alloy consisted of a high concentration of copper, zinc, magnesium and manganese to increase the Young's modulus (stiffness) and also the hardness. Therefore extra high concentrations of the additive elements in the aluminum alloy would form large diameter precipitated particles in the aluminum alloy. These particles made the alloy stiff and hard based on a metallurgical process called "precipitation hardening." After the disk was polished and cleaned, the precipitated particles on the disk surface would be etched away by a disk cleaning solution. Consequently the disk surface would have many pits. For use in oxide media, these pits did not affect the media performance because the substrate would be coated with a thick layer of magnetic oxide particles, so that defects would be invisible to the recording head. When this aluminum alloy was plated with a thin layer of NiP for thin film media applications, however, the pits could not be covered up sufficiently by the plated NiP. Thin film media itself was also much thinner than oxide coating, therefore the pits were not covered up or leveled and would show up as defects during recording. Therefore, when we made the disk for LCDM we needed to apply a thick layer of copper by electroplating followed by polishing the copper coating before plating the NiP layer. The substrate was then polished again to smooth out the NiP surface. This process was used for most thin film disk or magnetic drums initially and was very cumbersome and also expensive. Such a process was clearly not suitable for low cost thin film disk applications. Ampex, in their plated Alar media product, tried to alleviate the pitting issue by simply plating a very thick NiP layer without the copper layer, but it made the cost of NiP plating high and probably didn't solve the defect problem completely as the pits were quite large and deep. Plating thick NiP also caused other problems as well. Plated NiP film has built-in stress that increases its impact as plating thickness is increased. Uneven removal of NiP from each side of the disk during polishing could cause the aluminum substrate to warp. Clearly something had to be done about the aluminum substrate itself for thin film media applications.

At the time I started Komag, there were no aluminum substrates on the market that met the criteria I set for myself for the media application so I went to various substrate vendors in the U.S. to ask them to change the aluminum alloy composition specifically for thin film disk media applications. Unfortunately, none of the aluminum substrate manufacturers that I talked to in the U.S. was willing to put any extra effort into developing a suitable aluminum substrate. I think there were several problems in why I did not get much attention. First was the issue of my credibility with the vendors, and second was the market demand and importance of the aluminum substrate to the large aluminum manufacturer. I felt that I was probably alone in asking for this sort of change in aluminum alloy itself in those days, and for large metal companies like Alcoa, it was not trivial to change alloy composition. Aluminum ingots are made in very large batches and in immense industrial settings. It would take some doing to change alloy composition and adjust the process for subsequent operation for making thin foils that can be used to punch out disks. It would involve significant investments in plant time and their metallurgical engineering resources to make a new alloy available for new applications. During the late 1970s to early 1980s, the economy was not doing so well, and U.S. aluminum manufacturers were not keen to invest their resources into a business with an uncertain future like thin film magnetic media.

I was not getting any traction or attention from U.S. aluminum manufacturers, therefore I decided to try my luck with Japanese manufacturers. After some investigation, I found out that there were two companies in Japan already involved in making aluminum substrates for magnetic media applications, and they were Kobe Steel and Nippon Light Metal. On my first trip to Japan to see ULVAC for the design of the S1 sputtering system in September of 1983, I made arrangements to see Kobe and Nippon Light Metals. When I finished my work with ULVAC, I went to visit Nippon Light Metal in Tokyo the following day, and Kobe Steel the next day. The meeting with Kobe Steel turned out to be most productive. Kobe had already been very actively involved in making aluminum substrates for hard disk applications for several years, and they had managed to get all of IBM's business for aluminum substrates for oxide media, and were actively engaged in trying to develop new alloys for small form factor disks for thin film media applications. I got a sympathetic audience for what I wanted to say.

When 5.25-inch diameter oxide media emerged in late 1979, Kobe started to ship aluminum substrates to media manufacturers in the U.S. In early 1980, when the same form factor for thin film hard disk media started to show up, Kobe quickly modified their 5.25-inch aluminum substrate to cater to the thin film media market. Therefore, when I called on Kobe, they were interested in talking to me. On Friday morning, when I visited their headquarters in Tokyo, they had two persons from their sales office to meet me. I introduced myself and explained my credentials. I then proceeded to explain my expectations for an aluminum substrate that would be needed for thin film media applications. I explained to them that the substrates that were being used for oxide media would not be good enough for thin film media applications. Defects were too high, and they would not be viable for use as substrates for sputtered thin film media. I also explained to them what must be done to the aluminum alloy, based on my extensive metallurgical engineering background and my experience at PARC. It turned out that one person who was attending the meeting had a metallurgical engineering background, so that he understood what I was talking about. They decided that the Friday meeting was not enough, and asked me whether I would be willing to come back for one more day of discussions so that they could bring in a more senior level engineer, Mr. Usui, from their research group at their Moka Plant, which is located over 100 km north of Tokyo. I promised to meet Mr. Usui at my hotel on Saturday morning since my flight back to the U.S. was in the afternoon. Next morning, Mr. Usui showed up at my hotel (Kokusai Kanko), which was located near Tokyo Station. We had over three hours of intense discussions on aluminum substrates. I disclosed to him my idea of which alloying elements should be reduced to minimize the pits, but keeping enough of it to maintain adequate stiffness. I also suggested what combination of alloying elements would be appropriate to improve the plated NiP quality, namely how to obtain more uniform and smooth NiP on top of the aluminum. I provided these ideas freely to Mr. Usui, and I believe he was sufficiently impressed with my ideas that he went back to his laboratory and started to experiment with my suggestions. By November of that year, Kobe came to visit us in the U.S., and brought along some unique new substrates with new alloying compositions for discussion and evaluation. This was the start of the unique relationship between Komag and Kobe that was to continue for many years. The work that they did on the aluminum substrate was so successful that we used Kobe aluminum for a long time to come. They filed patents on their new substrates and came to dominate the market for aluminum substrates for sputtered media. From our first meeting in Tokyo, Kobe became a trusted partner for Komag and they not only became our key vendor but also a key investor. They also provided valuable assistance to Komag to improve our manufacturing capability and introduce Japanese Kaizen practices into our operation. I cannot emphasize enough the important role that substrates played for our technical success as well as business success by having a world-class capability in substrates. It was as important as sputtering technology in terms of establishing the quality of the media and cost advantages that came with the high productivity substrate operation that we developed. It could not have been accomplished without the Kobe relationship.

When Komag started in 1983, we did not plan to manufacture plated NiP substrates by ourselves. In fact, very few sputter thin film manufacturers tried to make substrates on their own. Our plan called for receiving plated NiP from an external vendor, which we would then polish ourselves. This was because it would have cost too much capital and resources to plate NiP ourselves and I knew there were several external vendors we could contract with to plate the NiP coating for us. Some of them were very small “garage shop” types of operations such as Burton Magnecoat, and larger metal finishing companies such as Allied-Kelite Corporation located in the Los Angeles area, and several metal plating companies in the San Francisco Bay Area as well. I had guessed that the electroless NiP plating process was already well-established, as this process was used to coat metal for many different industries, such as auto parts. Therefore, I assumed that all I needed to do was buy the right kind of aluminum substrate and qualify a well-established plating contractor to coat the NiP for us. From late 1983 to early 1984, we started to evaluate external plating companies by sending out our purchased aluminum substrates. This included experimental aluminum alloy substrates made by Kobe, as well as samples from Nippon Light Metals. We supplied parts to Burton Magnecoat and Allied-Kelite. Burton for example, also wanted to polish the NiP coated parts as well, and they were looking to become an "integrated" aluminum substrate supplier. At this period in time, there were no companies that had established themselves as a substrate manufacturer, to supply polished NiP substrates to media manufacturers. By February 1984, we had our own polisher within Komag Building One, and could compare our polishing with those from external vendors. After a few months of work, we settled on using only Allied-Kelite because Burton was too small and their quality was very inconsistent.

Initially when we were producing small quantities of evaluation parts from the batch sputter machines, we used NiP parts plated by Allied-Kelite and polished by ourselves. The quality of the NiP coating was adequate. However when we started to run the S1 system in October with a much higher volume, we suddenly received a batch of disks that had a very high level of pits on 80% of the parts. We were in a state of panic and Steve and I had to fly to Los Angeles to have a meeting with the president of Allied-Kelite. After a long discussion with them, it came to light that their de-ionized water system had been broken for a while, so that the filter they used for the reverse osmosis process was probably introducing a large amount of particulates into their plating line. They made many other plated parts from the same line and using compromised de-ionized water did not affect these parts so much, so that they kept producing our parts with the same bad water. This was an intolerable situation and I realized then that we could not rely on outside vendors with such sloppy operations. We picked Allied-Kelite because Burton Magnecoat was even worse. Their operation was literally a "garage shop operation". Realizing that this substrate situation would break us, I told Steve right there that NiP plating is much more demanding than I had expected and that we would not survive unless we plated our own NiP coating with much higher attention to detail and with improved technology.

After this incident, I quickly went out to find a company that could build a state-of-the-art NiP plating line for disk applications. The company we found was HBS Equipment Corporation, and the CEO was Donald Herzfeld. The company had been building plating lines for many years for other applications. Therefore when we called on them, they decided that they should invest in building plating lines specifically for thin film media industry applications. After I described to them what we needed, they quickly designed a line and quoted us a price tag of $400,000 for a plating line that could produce enough disks to cover the equivalent of four times the S1 output. Fortunately, in October 1984, we had just raised new funding of $7 million dollars so that when Steve and I presented the budget to the board in November for purchase of the plating line, it got approved immediately. I also asked for an extra $50,000 to build a manually operated prototype plating line to develop the plating process that we could use in the large production line. The prototype plating line was able to produce small quantities of disk substrates to supplement S1 needs before the production plating line could be started. The production plating line came on line in May of 1985. After all these experiences, we finally realized that the quality of the substrate is not only related to the aluminum substrate as I had originally thought naively, but also to plating process and its interaction with aluminum substrates. Even the interaction between NiP and the polishing process was quite important as well. With this realization, we decided that we had to control plating and polishing by ourselves, with careful attention to the aluminum substrate technology as well. We found that plating by ourselves was critical not only for defect control but for controlling substrate flatness or its outgassing characteristics during sputtering. We also learned that making our own substrate made big contributions to the overall cost of the media. We could optimize the plating process for high throughput through the plating line, as well as for improved properties through polishing, for example, to further reduce cost while improving quality at the same time.

For the aluminum substrate, we decided finally to use only Kobe-supplied parts. However, in order to ensure competitive pricing from Kobe and also to ensure supply, Will Kaufman decided to start using finished aluminum substrates from a local aluminum substrate finishing company called Disk Material Technology in Santa Rosa, California. They would still use Kobe's aluminum blanks, so that the plating process would remain the same for both Kobe and Disk Material parts. Unfortunately, their quality and cost were not able to compete with Kobe products; therefore Will Kaufman decided to buy the company (its assets) and assume its operations, believing that Komag would be able to run the operation better. Unfortunately, we could not turn around the operation; hence I approached Masa Takebayashi of Kobe, who was the president of newly formed Kobe Precision Inc. (KPI) in Hayward, California for help. KPI was setup by Kobe to make finished aluminum substrates for Komag and other companies in the Bay Area. Since Kobe was one of the major investors in Komag, he was able to get upper management permission to buy a 45% interest in this operation in Santa Rosa, and we re-named the joint venture Komag Materials Technology or KMT. It would make finished aluminum substrates for us starting in 1989. Kobe brought in a team of engineers under Mike Mizuno to station at KMT for several months at their own expense to help set KMT straight. They completely tore down the original floor plan and rearranged the factory layout to look like Kobe's manufacturing line, and they re-trained the personnel. After this infusion of capital and Kobe's manufacturing know-how, the KMT operation got better and started to become a very productive supplier of finished aluminum substrates to Komag (Reference 8-1).

To coincide with Komag's move to Penang, we asked Kobe to setup an aluminum substrate finishing company there. They accommodated us on this request as well and established Kobe Penang Technology, or KPTEC, in 1992, not too far from the Komag factory. We started the Penang operation in 1993 and KPTech was able to start supplying aluminum substrates to us from the beginning. They were truly an excellent partner for us. In 1994, when disk prices dropped to the $10 range, Komag decided to invest in a large substrate production facility in Sarawak, Malaysia. We leased a 50 acre plot on the outskirts of Sarawak at a newly developed High Technology Park. Komag was the first company to establish a factory there, so a year later the government of Sarawak awarded me a title of Dato', which is roughly equivalent to being knighted. When we started the plating and polishing operation of NiP substrates in Sarawak in 1995, we were able to reduce the cost of the substrate substantially because we were able to use the low price aluminum substrate from KPTEC. The move to offshore was a necessary step in achieving the $10/disk price target that customers demanded. The move to offshore operations was not easy because of the distance, and difficulties in solving problems when they occurred. I remember one time near the end of the year when we received a large batch of NiP substrates from Sarawak which had very high number of pit defects. We couldn't find out the reason why this was taking place by communication alone, so I decided to fly to Sarawak to investigate. After I got there, I reviewed all the procedures and process control methods. We took very systematic data and analyzed them using statistical process control methods. We finally found out the root cause. It turned out that new filters that we had started to use contained some polymer material that was leaching out when it was new, and this material was forming small microscopic micelles during NiP plating, which then caused pitting defects. The NiP plating would fail around the micelle when it attached to the disk surface during plating. Only when the NiP was polished did the pit manifested itself. The problem was solved when we asked the filter vendor to change one of the compounds they used for making the filter, and at the same time we implemented an extra step in the process to pre-condition the filter before it was used. This problem occurred only in the Sarawak factory, even though we used the same process and the same filters in our U.S. plant. Some things remained a mystery no matter how much we worked on them. The point I am trying to make here is that one cannot underestimate the complexity of substrate operations, such as plating, which on the surface appears mundane and old technology. When applied to demanding applications such as for a hard disk, it took all of my attention at times. Needless to say more than once, I had to work personally on matters related to substrate technology and process, and this was often just as hard and difficult as developing a sputtering process.

Sometime by 1997, when the disk drive market started to crash, the price of sputtered thin film disks dropped to well below $10 for 3.5-inch media. During this tough period, Komag decided to close the KMT operation in Santa Rosa and move the entire substrate operation to Sarawak. However, we still continued to use Kobe aluminum blanks. Only when the disk demand was high in 1995, did we purchase extra polished NiP substrates from outside substrate vendors such as Showa Denko to make up for the shortage. Otherwise we produced enough substrates for our own use. When the market crashed in mid-1998 to 1999 before my retirement, we had an overcapacity of substrates; therefore we sold substrates to others like Seagate and IBM. Even though our substrates had a much higher quality than their own product, it was very difficult to get good prices from customers. Typical behavior in the HDD business is to be very cut-throat and ruthless in one's treatment of vendors. The common strategy seemed to be that if one can drive a vendor to insolvency, it was a win for the materials group that managed the "commodity" purchase. Contracts, if signed, did not matter as they would be regularly broken. When there were only a few large customers left, the vendors were at their mercy. I saw this scenario played out over and over in the HDD business. Our own behavior toward the vendors was often not much better as we also had to struggle to survive, but I fought back as hard as I could to establish business relationships based on trust and mutual benefit, such as what we had with Kobe and ULVAC. To be fair, our customers were also subjected to the same sort of treatment further up the food chain. The whole electronics industry generally behaves very poorly in this regard in terms of business practice. One really had to fight very hard to protect what you had and provide a value-add proposition to your product so that customers have no choice but to buy from you. Alas, such situations are very difficult to maintain for long.

I found on many occasions that our senior management tried to undercut Kobe by expanding our own aluminum substrate capacity. When I was there, I tried hard to reduce this kind of short-term business strategy. I know that in Japan, business is carried out by building long-term relationships, which is unlike the typical U.S. situation. Because in Japan a senior manager works in a company for his whole life and they don't have the short-term view of quick gain like most of the senior management in the U.S. So in Japan, most of the companies like to build a long-term relationship with customers as well as with a vendor. I like to think that Komag tried to do the same, and because of this, we were able to get so much help from ULVAC and Kobe. By the same token, I tried to deal with our vendors for production equipment and consumable materials with a view towards establishing long-term relationships. Even after I left, Komag would turn the Sarawak Substrate Operation into a premier operation for making substrates not only for its own use but to supply top quality substrates to Seagate, HGST and MaxMedia. It contributed significantly to the turnaround that Komag enjoyed after emerging out of Chapter 11 bankruptcy. I think in some way, the foundation was set by the help we had received early on with Kobe, establishing excellent working relationships with key vendors to achieve a win-win situation for both sides. For a while, Komag was to become the largest producer of aluminum substrates in the industry with the highest quality and also the lowest cost before being acquired by Western Digital.

On the subject of substrates, some comment needs to be made about glass substrates. Initially when Komag started, the majority of substrates used for 5.25-inch and 3.5-inch media used aluminum with NiP coating. In order to make a smoother substrate that was also hard and stiff, it was always thought that glass would be superior to aluminum as a substrate for thin film media. Both large and small glass manufacturers attempted to enter this business but it was not going well. Even Asahi, whose goal was to eventually become a large supplier of glass substrates for media, setup a major effort to develop a process for making glass substrates. From the very beginning, cost was a major issue, as glass is very hard compared to aluminum substrates and a huge number of polishers would be required to polish glass compared to aluminum. Despite substantial effort, Asahi ultimately failed to commercialize glass until it was much too late. Hoya Glass can be credited with having persevered and finally succeeded in commercializing glass substrates. Today, glass substrates are used predominantly in 2.5- inch mobile drives while 100 % of desktop 3.5-inch media use aluminum/NiP substrates. The volumes are roughly split 40-60% or so for glass and aluminum. Mobile applications have gotten the main attention for glass due to its greater shock resistance. For desktop applications, the cost would be too large for converting to glass. Today, most of the volume for glass substrates is held by Hoya, followed by Konica Minolta. If there is any failure to be said for Komag in substrate development, it is that we were never able to play a significant part in glass substrates. Despite having spent significant effort with Asahi and also on its own to develop own glass substrate process, it never succeeded. It was always forced to purchase glass substrates from others to participate in the mobile market product space. The volume for media for mobile applications hardly grew for Komag until Western Digital acquired it. To this day, it still does not manufacture glass substrates on its own. The cost of purchasing glass substrates is significantly higher than for aluminum substrates; therefore the long polishing time still continues to be an issue. Also a factor is the very large capital investment necessary for glass substrate manufacturing. Even the vertically integrated companies would be hard pressed to justify the huge capital cost for setting up a glass substrate factory, which would run into several hundred million dollars.

Further Improvements to Lubrication and Overcoat

When the spray/buff method of applying the lubricant to the disk was developed, it provided significant advantages to Komag media in terms of tribology. But the process was difficult to control and manage. As the volume of media we produced started to increase, we were using an increasing amount of Freon that just evaporated and was released to the outside. We were using CFC solvents at first and it was recognized that this solvent caused ozone depletion and the Montreal Protocol was signed by most countries to eliminate its use. CFC was replaced by a PFC solvent, and although they solved the ozone depletion issue, they were strong greenhouse gas sources, which started to grab attention in the world. Further changes to the solvent were necessary to comply with environmental regulations. As major producers of these fluorinated solvents tried to comply with the new regulations, the cost of these solvents became very high. We could not expect to simply spray them away on each disk. In any case, from quite early on, we were not all that happy with the spray/buff method of applying the lubricant, and there was a concerted effort to convert the process to a dipping method, which was thought to be simpler, provide more uniform lubricant coverage, higher throughput, and be more economical in terms of solvent usage. Commercial dipping systems were available and our competitors were using them. When we compared the results with spray/buff however, the dipping process never performed as well on tribology and several years elapsed before we were finally able to use the process.

The reasons why we struggled with the dipping process were several, and this is with the benefit of hindsight. The initial type of dippers we tried to use were what is termed a "drain" type, which is basically a big tank with a drain at the bottom. The racks of disks would be placed inside, and the lubricant solution in Freon would be drained slowly. The concentration of lubricant and the speed at which the solvent drained controlled the thickness of the lubricant on the disk. The problem with this approach was that lubricant thickness was very sensitive to what was taking place at the meniscus between the solvent and the air. As the liquid drained away inside the tank, the meniscus condition changed and the lubricant thickness slowly varied across the disk surface. The alternative was to pull the disks out of a tank, with controlled meniscus. The difficulty with this method is that one needed extremely good mechanical design with the pulling mechanism to avoid even the slightest vibration so that the meniscus was not disturbed. We had to construct such equipment by ourselves which took some time and experience to develop.

The second issue was more subtle. Very fundamental to the tribology was the fact that there were chemical and van der Waal types of interactions taking place between the lubricant molecule and the carbon surface. The reason why the polar z-dol worked so well with the sputtered carbon was because the right balance of this surface interaction was achieved for the tribology to work well. When spray/buff process was used, we had the added effect of mechanical rubbing and heat that was generated by it that conspired to "bond" the z-dol to the carbon surface by just the right amount. There is an old adage that says that a lubricant is not a lubricant if it doesn't move. This means that oil that lubricates the interface between two rubbing surfaces works because the friction is reduced but also it replenishes itself. For the head/disk interface, there is apparently a necessity to have just enough lubricity for replenishment and low friction, but also some amount of bonding between the lubricant and the disk is necessary to fully protect the interface. When we applied only the dipping method, we had somehow lost some part of this equation and we could not obtain the good results that came from spray / buff process.

Some of the issues with applying the dipping process were probably exacerbated by having implemented a hydrogenated carbon process in the 1988 and 1989 time-frame. Again by hindsight, the hydrogenated carbon surface had significantly degraded bonding with even the z-dol lubricant, but we did not see significant degradation in terms of performance when we applied the spray/buff process. The spray/buff process was apparently helping to bond the lubricant to the surface. Of course, the situation was made worse for the dipping process. The lubricant was apparently not bonding very well to the hydrogenated carbon overcoat. A better understanding of what was taking place came when we hired Judy Lin from IBM starting in 1994. She had spent considerable effort to understand lubricants – carbon interfaces and how to go about measuring the effect. We also had started to get some ideas as to what was happening when Bruno Marchon et al at Seagate published an interesting paper describing what happens to carbon film when the surface is exposed to nitrogen in the absence of oxygen (Reference 8-2). They described a reduction in carbon wear or friction. They did not necessarily tie lubricant bonding changes with nitrogen, but we had interpreted the result as having a connection. The reason was that we always had varying levels of nitrogen in our carbon film, but it was not controlled and deliberate. We were introducing nitrogen to control the coercivity of the magnetic layer and we even had a relatively sophisticated way to vary the concentration over time by controlling the MFC (mass flow controllers) so as to obtain uniform Hc over the panel. Because of the way our sputter system was designed, there was always a down flow of gas going from the high pressure magnetic chamber to the carbon chamber through the slits. Because we were varying the nitrogen level in the magnetic layer all the time, and also the condition of the slit and pumping at the slit varied over time and from machine to machine, the carbon was getting varying levels of nitrogen in the film. This then explained the lack of consistency that we were experiencing with the dipping process. With Judy Lin on board and with a better understanding of what might be taking place with the overcoat, we started to deliberately add nitrogen to the topmost part of the carbon in order to manage the lubricant bonding. This, combined with the improvements that we made in the dipping equipment, allowed us to finally be able to migrate to a dipping process in 1995.

Lubricant-overcoat interactions continue to play an important part in media design. In 1996, we migrated to thermally bonding the lubricant, but it was short lived. We started to introduce carbon with an even higher level of hydrogen later, and for this type of carbon we had to resort to laying down a fresh carbon layer with only the concentrated nitrogen on the top most layers in a separate step. UV bonding of lubricant was introduced in 2004 at Komag to help bond the lube more aggressively to the surface. Today, adjustments are being made on the lubricant itself to change the nature of the bonding. This area remains a very active area of research and development in media development.

(Reference 8-1)
http://www.fundinguniverse.com/company-histories/komag-inc-history/

(Reference 8-2)
"Evidence for Tribochemical Wear on Amorphous Carbon Thin Films"
Bruno Marchon, Neil Heiman, and Mahbub R. Khan
IEEE Transactions on Magnetics
Vol. 26, No 1, pp. 168-170, Jan. 1990

Chapter 9

Foray into the Head Business: Dastek and Headway

From the very start of Komag, I had thought that we would have to become a diversified company so that supplying sputtered thin film media to OEM customers would not be our only business. In our original business plan for Komag, we planned to diversify into the consumer-oriented removable media market by developing magneto-optical (MO) media. Therefore, when David Treves told me that he was retiring from his teaching post at the Weizmann Institute in Israel in 1987, after our IPO, I obtained board approval to invite him to join Komag to start up a MO media development effort. As I described earlier, David formed a small group to work on the project, and he was able to successfully develop a commercial MO product by 1990. We managed get up to $2M in sales from the MO disks, but by this time write-once CDs came onto the market and basically started to kill everything else. The price of CDs started to drop so fast that it came to completely dominate the removable storage business. The MO disk and the drive needed to read/write this type of media was just too expensive to compete with CDs and their drives. Consequently, after only one year of production, we had to shut down the MO disk operation. The cost of developing MO media was over $12M, and we paid an additional $2M license fee to Philips for the basic patent that they owned for optical recording. The failure of our MO effort taught me that it is very difficult to create a new business with a standard that has not been accepted by everyone, even if you have great technology. Our MO media technology was truly great. It was far superior to any other MO media that existed at that time, but it did not matter.

By 1990, when we had achieved some level of success in the media business, I became even more convinced that we would not survive for long solely as a hard disk media supplier. I saw that in the head business, thin film head technology would eventually replace the older ferrite heads, so I started to think that there might be an opportunity for Komag to participate. If we could control both head and media technologies, we would command the two most important components inside the HDD. The integration of these two components into an optimized design so that they would work together, would provide significant value-add and turn Komag into a powerhouse in the HDD components business. I also saw this as a path for us to eventually become a vertically integrated HDD manufacturer. That dream was a stretch, but still a realistic goal. I felt that if we did not do this, we would eventually see our margin pushed down by our customers as they consolidated into fewer companies, and our competitors became more capable. Many of our customers, such as IBM, HP, CDC and Seagate, already had their own media operations, and they would have eventually caught up with us and they would have had no more need for us. Making heads as part of our business really made sense. Getting the head and media to work together was always the key issue in any HDD design. This integration activity was always in the hands of the HDD customers, and I frankly thought that we could do better if we owned the media and the head, and designed both from the ground up to work together. I always thought that we knew much more about the media than the customers, and the head vendors usually felt the same way about their product. If we could put the expertise of both together, we would have a significant edge in being able to design-in performance from the ground up. This applied not only to the magnetic performance but also for tribology. It was natural to believe this as a business opportunity, as we would be often blamed for issues that occurred with the heads and vice versa. Customers often played us against the head vendors in order to solve problems which the HDD customers were more often than not, clueless to solve. They frequently had relatively less understanding of the issues that they were seeing, whether it was in the heads or in the media.

In 1990, when Jim Shir was taking an extended sick leave due to exhaustion caused by several years of hard work to start up Komag, we hired Tracy Scott as vice president of R&D to cover for Jim. In the early spring of 1991, Tracy heard that our customer Siemens, had decided to get out the disk drive business and intended to close down their thin film head research facility in Germany, and dispose or sell-off their equipment and IP. Tracy had considerable experience in running a head operation at IBM. Therefore, Tracy and I went to Siemens's facility in Stuttgart, Germany to take a look at what they had. We were quite impressed with what they had developed for thin film head technology, and we made a proposal to our board to acquire the technology and all the development equipment, with the intention of eventually entering the thin film head business. The price tag was $2M, plus some expenses to cover the cost of transferring the equipment to our facility and to pay for Siemens engineers to setup the equipment and process at our site. Our board approved the proposal and we went ahead with the purchase. I believed at the time that the thin film head was riding the start of new “S curve” as it would replace all the ferrite heads. Eventually, the new MR heads would be coming along and this technology required thin film heads as the starting base. Even though there were already existing independent thin film head manufacturers, such as Read-Rite and Applied Magnetics, along with in-house operations at large companies such as IBM, Seagate, Fujitsu and so on, the thin film head and the expected MR technologies represented enough of a technology change that a new entrant into the business would still have had opportunity to succeed. Therefore, I was able to convince Steve Johnson and the board that getting involved in heads made sense and they trusted my judgment. We asked Tracy to take charge of the thin film head development effort, and he hired Jerry Lopatin from IBM to lead the group. Jerry hired several experienced engineers for the effort, including C.C. Han from Applied Magnetics Corporation.

The Siemens process used 3-inch wafers and it was an “all dry” process, meaning that the entire stack of soft magnetic layers of the poles was deposited by vacuum deposition, instead of the more common method of plating. This process was more expensive than plating, but we thought that eventually all heads would be made by vacuum deposition as it would have much more flexibility and capability, as was the case with media. The Siemens equipment and process were successfully transferred to our Milpitas facility, and by end of 1991, we were able to reproduce the process that Siemens had in Germany. Armed with this good news, we went back to the board to start discussing the investment necessary to start production of thin film heads. During this time, Tony Sun, who was on our board from Venrock Venture, brought out the idea that we should consider buying Dastek, a small thin film head manufacturer located in south San Jose, instead of starting fresh all on our own. At that time Dastek was struggling to raise more capital, and needed some help. Being acquired was one way, and Venrock was one of the investors in Dastek. Buying an existing manufacturer made sense since we would have been able to quickly enter the head business and would have had an existing customer base to work with. In December of 1991, we acquired Dastek by issuing 2.6 million new Komag shares. We merged our Head R&D into Dastek and created a subsidiary company. In order to provide new cash into Dastek, we formed a joint venture with Asahi Glass in February of 1992, and Asahi invested $36M cash, and $24M in loans for a total investment of $60M, for 40% share of the joint venture. In 1991, Komag raised another $42M from a second public offering of 2.3M shares. The Dastek purchase was worth about $50M and we were riding high with success in the media business. AKCL started out successfully as well, and Asahi saw no reason to doubt that a $60M investment into Dastek would be a good bet. In 1991, Dastek had sales of around $50M. Even though they took a loss of around $10M for the year, we saw no reason why we could not turn around the business with a fresh infusion of cash from Asahi, and new R&D muscle that we would provide through our Siemens technology. We also had a pathway for developing the new MR head technology.

In June of 1992, we signed an agreement with Hewlett Packard to jointly develop an MR head based on the dual stripe magnetoresistive head (DSMR) that had been in development at HP Labs for several years. Prior to working with us, HP had worked with the Applied Magnetics Corporation to develop the DSMR head, but this was not going very well for HP. Applied Magnetics was in severe financial difficulties as well, due to tough competition from metal in gap (MiG) ferrite heads which were cheaper to produce. MiG heads were based on ferrite technology that had been around for a long time, but by introducing a metallic soft magnetic core at the gap, provided an extra boost in writability and thereby greatly extended the life of ferrite heads. Komag presented a much better opportunity for HP to develop their DSMR head technology. We had the financial strength, we had just acquired Dastek and we were the leading supplier of media to them. We also had an excellent reputation as being very competent in developing new technology. We were a natural choice for HP to pick as partner for their DSMR head development effort. HP's DSMR head was quite unique and it provided some significant advantages over other designs, which were being used by IBM and others. Because of the several years of effort that HP had spent on the DSMR design, we stood to gain significantly in accelerating their MR head development so that it could be made available to Dastek quickly. Tracy's R&D head development team was now directed to work on DSMR with HP, incorporating some of the know-how that we had obtained through the Siemens technology.

As 1992 progressed, we were having a tough time at Dastek. They were making mostly mini-sliders that had to compete with MiG heads of the same size. There was no way to compete with MiG heads. They had to switch to smaller micro-sliders that would target high-end drives with better margins, where only the thin film head would be used. By switching to micro-sliders, one could put many more heads on each wafer, so that the output could be increased and cost could be reduced. As the Dastek team started to switch to micro slider heads, new problems started to emerge. The biggest issue was that the noise in the output signal increased, which was associated with the Barkhausen noise in the pole. This is typically caused by domain wall motion in the poles, and it was severe enough to disqualify the head from customer programs. Each head had to be tested for this problem, and testing could be done only after the head was completed. The yield hit was very high, and the cost was out of control. We were in a desperate situation, and I pulled in Tracy's R&D team to help solve the problem. I also recruited Kochan Ju from IBM to work on the problem. He was already well regarded as a technologist at IBM with many years of experience in MR head technology development. In order to avoid creating issues with IBM, we assigned him to head the mask design group and also help in solving the noise problem. This was an area where Kochan had not had direct involvement at IBM so that we felt that we would avoid issues with hiring someone of his stature. Another outstanding engineer we hired from IBM was Mao-Min Chen. In order to hire him, I had to visit Mao-Min's house to convince not only Mao-Min, but also his wife that joining Dastek would be worthwhile. This was not easy but I was able to entice him to join. Also, to avoid issues with IBM, I asked Mao-Min to start working on slider process development, which he was never involved in at IBM. Such considerations were important then, since earlier in the year, IBM and Seagate were involved in rather nasty litigation over the hiring of Peter Bonyhard from IBM by Seagate, to manage their MR head development effort. Peter Bonyhard was an acknowledged expert on MR heads and IBM was very aggressive in protecting its MR head technology IP and know-how. We did not want such entanglement with IBM. We could not afford to have a fight with IBM. They were also a very important customer for AKCL in Japan.

We had assembled quite a talented group of people for Dastek and for the advanced head development effort with HP. We eventually solved the Barkhausen noise issue and by the end of 1992, we were getting qualified with micro-slider heads, and started to ship in volume in 1993. However, the struggles at Dastek took its toll on our 1992 earnings. The loss at Dastek amounted to $25M in10 months, 60% of which had to be booked by Komag. We also had expended a large portion of Asahi's $60M cash infusion on capital improvements and conversion to micro-sliders. There had also been a considerable amount of wasted money spent on the "look and feel" of Dastek, to make it look more like Komag in terms of culture and appearance, even down to its furniture. Perhaps I should have fought such spending more vigorously, but at the end of the day, it was the large loss from operations that doomed the venture. Head manufacturing, like media, is very capital intensive and difficult. When things do not go well, it becomes a black hole for money. As it turned out, other independent head manufacturers were not faring much better. In 1991, Applied Magnetics had discussions with us about being acquired and we had a meeting with them in Goleta, California where they are located. This discussion did not lead to anything useful, which had led us to purchase Dastek instead. In the fall of 1992, Cyril Yansouni, CEO of Read-Rite called up Steve Johnson about the possibility of a merger between Komag and Read-Rite. We had a meeting at the Holiday Inn in Milpitas to discuss the proposal. Cyril, like me, also thought that it would be difficult to survive as an independent OEM component supplier. Therefore, it would have been more advantageous to join forces and control a bigger piece of the component business. I think the reality was that his business was hurting badly as well, and needed some way of getting through the difficult business environment. One interesting outcome of the discussion was that he threw out an idea that a three-way merger between Western Digital, Komag and Read-Rite ought to be discussed with Chuck Haggerty, then head of Western Digital. We thought that this proposal has some merit, and we actually had a meeting with Chuck Haggerty in Las Vegas, during the Comdex Show. The meeting was held at his hotel room in the early morning, he was still in his bathrobe when we knocked on the door. Perhaps because it was still too early in the morning, Chuck Haggerty was not so enthused about the idea, and he did not feel any urgent need for vertical integration at that time. As things turned out, Applied Magnetics went into Chapter 11 bankruptcy in 2000, while Read-Rite went into Chapter 7 liquidation in June, 2003. It was Western Digital that picked up the pieces of Read-Rite, and they also acquired Komag in 2007. Perhaps Cyril had a premonition of what was to come, but it actually happened many years later. However, our Dastek head venture was not to last much longer either.

It would be interesting to think about how the world might have turned out if we had merged with Read-Rite in 1992, but in 1993 we went about trying to compete with them as best we could for the available market. Unfortunately our cost was too high due to smaller volume and poorer yield. We did not have the operational experience that Read-Rite had, and we continued to lose a significant amount of money at Dastek. By the end of 1993, we had spent all of the $60M funding that came from Asahi, and yet we still needed to continue pumping even more money if we were to make it operate better. The troubles at Dastek also affected our Komag financial numbers, and our stock price was getting a big hit. By the end of the year, our board had seen enough, and told us to get out of the head business completely by early 1994. For 1993, we booked restructuring charges of $38M related to the closure of Dastek, and Komag had a loss of $10M for the year. The saving grace was that media was still very profitable and generated over $110M in cash. The effect on Asahi Glass was even larger as they had invested a lot of cash in the business, not to mention taking a 40% share of its losses on their books. Faced with the Dastek closure in late 1993, Tracy Scott, Kochan Ju, an Asahi Glass representative and I met to figure out what we could salvage from the wreckage. One of the key issues for me was that I owed a future for the many people I had recruited into Dastek. Many of them were working so hard to develop the new DSMR head technology, and I needed to find a way to save what we could and make something out of it. The outcome was that we decided to continue operating the R&D operation in Milpitas while we negotiated a future with HP. HP was still interested in completing the work on DSMR head development, and they wanted to use it for their drives. We had set aside over 60 people from Dastek to continue the R&D effort, and it was vital to find a way to keep the activity going. On July 16, 1994 we came to an agreement with HP and Asahi Glass to start a new head venture called Headway, with cash investments from HP and AKCL of approximately $20M each, while Komag and Asahi Glass would contribute the assets of the R&D equipment for a small minority stake.

A total of 61 employees from Dastek joined the Headway venture. The first president was Ralph Patterson from HP's printer division. He was a seasoned executive that is credited with turning HP's printer division into a powerhouse. He also brought several HP engineers from their drive division to coordinate the project. They wanted to use the DSMR head in their "Tomcat" drive program, which would be the first drive to use MR heads at HP. I should mention here the reason why Asahi Glass made investments in thin film media and also in the head business. Asahi Glass was one of the largest glass and ceramic manufacturers in Japan, if not the world. Therefore, they had an interest in using their core competencies in glass and ceramic technologies to develop new opportunities in high tech businesses. Glass substrates would have been an opportunity in the media business, and in the head business, they could have developed high performance Alumina-Titanium-Carbide (AlTiC) substrates for use in thin film heads. The reason for funding Headway through AKCL was so that they could provide AKCL personnel into Headway to develop the know-how necessary to engage in the head business, just as they did with Komag on the media.

As a minority investor into Headway, I continued to work with Headway people on technical matters, and meeting with them on a weekly basis to track their progress as a Komag representative. Between 1994 and 1996, Headway engineers under Kochan Ju, Mao-Min Chen, C.C. Han and others were able to solve many difficult technical problems in developing DSMR heads for HP's HDD requirement. With their intense efforts, they were able to meet several funding milestones and demonstrated not only technological feasibility of the DSMR, but also produced many prototype heads for HP's Tomcat drive development. During the initial part of the development effort, the Headway team used the 3-inch wafer equipment that was inherited from the Siemens acquisition. In addition, Tracy Scott took some of the wafer processing equipment, as well as slider and HGA (head-gimbal assembly) processing equipment from Dastek's San Jose facility to Headway's Milpitas facility to support the prototype development. Therefore, Headway was able to produce wafers and process them into small quantities of sliders and HGAs for customer qualification. Headway could not have produced larger quantities of sliders and HGAs on a manufacturing scale. Therefore, they had to look for an outside contractor to make sliders and HGAs from the wafers. There was an intense negotiation that took place between HP and Seagate for Seagate to become a contractor for these services. Finally, HP succeeded and Seagate agreed to process Headway wafers into sliders and HGAs. In order to make this work however, Headway had to convert their line from 3-inch round wafers to 4.5-inch square wafers which was the standard that Seagate was setup for. This was no small feat; it was a very difficult undertaking. With intense effort, the Headway team was able to do the conversion successfully on time and sent new wafers to Seagate for processing. When Seagate tested the DSMR head for HP, they realized that the head was performing much better than their own design, and showed the superiority of the DSMR technology. As a result, Seagate decided to use Headway's DSMR heads (as wafers) and process them into sliders and HGAs so that they could be used in Seagate's 10K and 15K high performance server drives, thereby employing MR heads for the first time. In return however, they demanded and got a license for the DSMR technology and know-how to make them for their own use. They paid around $2M for the license. Headway needed this money to keep the operation going. The deal that was negotiated by Tracy was a positive for Headway, since it gave them extra money to keep going and also lent some credibility to Headway, as acceptance from Seagate was worth a lot for other business opportunities. Also, the conversion to 4.5-inch square wafers turned out to be a blessing as Headway's business took off between 1997 and 2000.

Unfortunately, by mid-1996 HP decided to exit the HDD business altogether, and HP wanted to get out of the Headway venture as well. Headway needed another investor to continue the business. One person that came to mind was Ta-Lin Hsu, who had just raised a lot of money to invest for his Hambrecht & Quest (H&Q) Pacific Venture Fund. I asked Kochan Ju to contact Ta-Lin Hsu about investing in Headway. Ta-Lin Hsu called me back right away to ask me questions about what I thought about Headway's potential. I told him what I believed and told him that Headway could be the first independent head company to deliver commercially viable MR heads into the market. However, Ta-Lin Hsu hesitated, and he did not come through with his decision. During this time period, Seagate was trying to recruit Kochan Ju from Headway, and as they found out from him that Headway was in play, they offered to purchase the entire operation. The price they offered was very low, at the level of a fire sale. I heard about the offer from Kochan Ju when I was visiting my father in

Taiwan at the time. Therefore, from a public phone located at a National Park in Taiwan where I was visiting, I called Steve Johnson to tell him that we should not accept Seagate's offer. I told him that Kochan and I were talking to Ta-Lin Hsu and that we could get a better offer. Of course, we didn't have anything yet at the time, but I was not going to let go of Headway for cheap. My recollection of the events was that I was very angry with Seagate's tactics, first of all in trying to raid Headway personnel, then to offer the fire sale price for what I considered to be very valuable property. It was quite predatory.

After I called Steve Johnson, I called Kochan Ju to ask him to talk to Ta-Lin Hsu about the impending offer from Seagate. Finally, after this, Ta-Lin Hsu decided to act. When I got back from Taiwan, Ta-Lin Hsu called to inform me that he was prepared to invest in Headway. He would put in $20M into Headway if I would also personally invest $2M myself, as good faith money into the venture. I agreed to his terms without any hesitation as I truly believed that we had an excellent market opportunity and great technology to meet the market need. Most importantly, we had assembled a superb team at Headway. When I presented this new reorganization plan for Headway, with H&Q as the lead investor, at the Komag board meeting, Irwin Federman became quite excited about it. He was also relieved to hear about our plan, as he really cared about fostering good technology and he didn't want it to go to waste. Irwin Federman was a managing director of U.S. Venture Fund, and he had a lot of knowledge regarding the storage technology business. His investment goal was not only to make money, but also to advance the technological frontier for the benefit of society. He was the first outside board member for Komag, and he helped Komag and me personally ever since we got organized in June of 1983. From my past experience with Irwin, he truly walked the talk. He really cared, and gave me a few lectures (when I needed to hear it) and advice through the course of our interaction with him on the Komag board. After the board meeting, he came to my office to ask me more detailed questions regarding Headway and its position in technology and in the market. After this, he offered to invest $4M into the new Headway venture. I invited him to be a board member of Headway and he gladly accepted. Erwin went on to provide invaluable advice to Headway, ranging from management, marketing and IPO strategy until they were acquired by TDK. I respected Irwin Federman's business acumen, and he also understood the importance of Headway's technology for the future of HDDs. I was very happy to see that he would put in his money into the venture. It was a big stamp of approval, and his continuing support for my opinions despite having seen Dastek as a big failure for Komag. Irwin's support meant a lot to me.

There was one thing that did not come out in my favor with Irwin Federman investing in Headway. Ta-Lin Hsu became so bullish with the Headway investment once Irwin Federman came on board, that he sold the deal to more of his friends as payback for some past favors he owed. He over-subscribed the deal, and he took it out of my $2M that he originally asked me to invest. My investment into Headway ended up being only $200K and

I was basically cheated out of a great opportunity. The $2M investment which I was prepared to make, would have netted me very nice gains for all the effort that I had put into the success of Headway. With Ta-Lin Hsu's and Irwin Federman's new investments into Headway, the earlier investments by HP, AKCL and a little bit from Komag were basically wiped out. Asahi, through AKCL, had lost out twice now in the head investment, although 50% of AKCL's investment was also Komag's. It was again, from the financial standpoint, a completely new start for Headway. The new financing deal was completed at the end of 1996.

With the new Headway, I could not be connected into Headway as a Komag person, but rather as private investor. I promised Ta-Lin Hsu that I would help run the company and he asked me to be the chairman of Headway. However, I told him I was chairman of Komag already and I could not do both. Ta-Lin Hsu became the chairman, and I became vice-chairman. Since I could not spend too much time in being involved with Headway, I conducted weekly half-day meetings with the key technical people about their progress, and also got involved in some management matters. It was quite stimulating intellectually to work with the Headway people on their development of DSMR technology. Having concluded that Headway also needed a change in management, I went to Hong Kong with Kochan Ju to recruit Mike Chang from SAE to run Headway. Mike was a long time drive veteran, having held key positions in IBM from the very beginning. He was recruited by SAE, which was acquired by TDK. We had to wait for Mike to complete his obligations to SAE before he could join Headway. He came on board in July of 1997. He also brought one of his top lieutenants, Po-Kang Wang. As it turned out, Mike Chang's management and leadership was very crucial to the success of Headway.

By the end of 1997, under Mike Chang's leadership, Headway was qualified into Seagate's 10K rpm Cheetah drive, with an areal density of 800 Mb/in^2, for the server application. Toshiba also qualified Headway's DSMR head for their mobile drive with an areal density of 1.8 Gb/in^2, using SAE's back end operation to support fabrication of sliders and HGAs. These drives were leading edge drives using the first MR heads, and aside from IBM, it allowed companies like Seagate and Toshiba to announce their own drives using MR heads. This was quite an achievement from such a small start-up with only a handful of people producing the head. Recruiting Mike Cheng not only brought in his management skill to the company, but through his earlier relationship with SAE/TDK, Headway was able to obtain SAE support for back end slider and HGA fabrication, and it was one of the key factors that made Headway profitable very quickly.

From 1997 to 2000, Headway continued to make progress in improving the reader and writer technologies in order to expand the customer base. The objective was to achieve an IPO. In order to qualify, Headway had to become profitable. With their hard work, they achieved 9 consecutive quarters of profitability; between 1997 and 1999 delivered 4 generations of DSMR technology; developed GMR reader technology; and qualified it into a Toshiba drive with areal density of 10 Gb/in^2 in 1999. With this kind of achievement, it should have been possible to go to IPO. By early 1999, an effort was made on going for an IPO by the end of 1999 or early 2000. This was vital in order to raise money for expansion of Headway's manufacturing facility. Tom Surran, the CFO of Headway, and Mike Chang put together the plan for the investment community, and with Irwin Federman's help, they were able to line up several investment banks to prepare for the IPO. Unfortunately, by 1999 the HDD industry was in large turmoil due to over-capacity. The stock price of public companies involved in storage at that time was also being hit very hard.

The investment bankers advised the IPO would not be possible in such conditions, and that they would have to wait. However, Headway could not afford to wait. Without additional investment, Headway could not have increased the capacity to meet customer demand and they would not have been able to survive standing still. Therefore, Mike Chang had to work on an alternative solution. Again, thanks to the connection that Mike had with TDK, he was able to convince the upper management of TDK to look into investing in Headway. Consequently, a deal was worked out with TDK to purchase Headway and turn it into a TDK subsidiary. The purchase price was $120M, out of which $30M was distributed to the employees. It provided handsome returns to the investors, and also to the employees who worked very hard to make it into a success. My own regret was that Komag was not part of this success. I also regret the huge amount of money that Asahi had invested that came to nothing and also Komag's own investment into Dastek that returned no gain to its stock-holders. The fact that TDK was willing to spend this sort of money for Headway is also a testament to the high level of technology that Headway was able to develop. One should not forget that what Headway represents now is the culmination of a much longer effort and investment by many parties, many of them losing everything before the last group of investors finally reaped the benefit. This is the fact of life for many new technologies. Xerox's large investment into the office of the future did not return any benefit for them. It was everyone else that followed that reaped the benefit. In return for TDK's investment Headway gained a valuable owner that continued to invest in the future. The sale was also attractive in that Headway was already using SAE facilities to turn the heads into sliders and HGAs. It was a natural fit. Having Mike Chang at the helm also helped to get the deal done with TDK. Headway was formally merged into TDK's Head Division in March of 2000.

In response to the HDD industry's demands, Headway also adopted the single stripe MR head, the design pioneered by IBM. This design became the industry standard. Later, this design was extended by the industry as well as Headway to capitalize on the GMR and TMR effects. Headway was first to market with TMR heads.

Today, Headway continues to operate as a TDK subsidiary at the same location that were originally Building Two and Eight for Komag in Milpitas, where Komag started. I was told that they produce about half of the wafers needed for the approximately 600 million HGA's that TDK produces in a year. The people that I originally recruited into Dastek and Headway, such as Mao-Min Chen, CC Han, and Po-Kang Wang continue to run the operation, delivering the heads that enable the latest areal density points for TDK's customers. The areal density being achieved by Headway's state-of-art TMR heads is around 750 Gb/in^2. This is amazing progress, around 1,000 times higher than the 800 Mb/in^2 at the start of DSMR head development effort. I believe that TDK, like many other Japanese companies, have the long-term vision and patience needed to invest in a company like Headway, and they have definitely made the right bet in purchasing Headway. I also believe that if Headway had gone the way of an IPO, they probably would not have been ultimately successful. This is because in order for them to compete, they would have had to invest heavily in the infrastructure needed to perform the back-end operation for the head to stay cost competitive. This is very difficult even for large captives such as Seagate, Western Digital and Hitachi GST. The marriage of Headway with TDK was truly a win-win combination for both.

I also believe that because of Headway, the HDD industry was able to adopt MR head technology much sooner than they would otherwise have been able to do. Only IBM possessed the MR technology at the beginning and everyone else was struggling, including Seagate. TDK also benefited considerably by having Headway, and they are now the only independent head manufacturer left in the industry. I believe that they would not have been competitive against the large internal head operations, such as IBM's or Seagate's, if they had not acquired Headway.

Chapter 10

Competition with Oriented Media

I have talked about the competing media design based on a CoCrTa magnetic layer deposited on a chromium base that was also oriented in Chapter 5. By the time sputtered thin film took over the media market, there were basically two types of media being produced. One was what Komag produced, which consisted of a sputtered NiP underlayer and a CoNiPt based film, which was crystallographically isotropic; the other, which is what everyone else made, was oriented media. There were a few exceptions, but for all practical purposes, these two media types dominated the market. For Komag's isotropic media, crystallographic orientation of the cobalt grains was randomly oriented (in 3D), but the easy direction of magnetization of the film was random in the plane of the media due to thinness of the media (in 2D). Oriented media, mostly based on a CoCrTa alloy with Cr or Cr alloy base underlayer, had its cobalt c-axis crystallographic orientation lying in the plane of the media, and its easy direction of magnetization oriented along the circumferential direction of the media. There was some controversy as to whether c-axis crystallographic orientation was also along the circumferential direction or not. When I was starting up Komag in 1983 and 1984, oriented media consisted of CoNi or CoNiCr alloy on Cr film. I think it is fair to say that initially I had the upper hand with my isotropic media because oriented media was much more difficult to make. Depositing a Cr underlayer and a CoNi or CoCrNi magnetic layer epitaxially on top of the Cr underlayer required good vacuum, and also concentric texture on the aluminum substrates, neither of which were well-controlled initially.

As a matter of fact, the epitaxial growth of CoCr alloy on top of a Cr underlayer to create useful media was demonstrated by J. T. Maloney (Reference 10-1) in late 1970, but obtaining high Hc with this structure was not well-understood. Therefore when Lin Data started to use the CPA in-line system with this structure, they seemed to be having a difficult time making good media. I believe it was at least partly due to the fact that the vacuum level in the CPA system was probably not good enough. Furthermore, the texture induced preferred orientation of easy magnetization was not well-understood either; therefore the initial production disks shipped by Lin Data and Trimedia showed amplitude modulation along the circumferential direction of the disk. We saw this on some of their media that we had obtained "surreptitiously" and we heard the complaints about amplitude modulation from our customers. It has been claimed in recent recounting of history from this period that Trimedia was in fact initially trying to avoid getting the orientation and had been trying to ship isotropic media because the orientation effect was hard to control and it would cause modulation problems. However, what I saw on the media that we had obtained back in early 1985 was something that was clearly intended to be oriented media.

J.T. Maloney did not know about the orientation effect due to concentric texture, and he was depositing the media directly on as-polished NiP plated aluminum substrates. As such, substrates would have many random polishing patterns on their surface and the Hc would have been all over the place, as orientation could have varied from location to location. Even after the concentric texture effect was discovered, the quality of substrate polish was still not very good, therefore concentric texture still might leave enough polishing scratches if one did not texture deep enough to remove them. Simply speaking, the substrate technology, texture process, and vacuum equipment were not quite ready to deliver oriented media with good consistency. Our magnetic film process did not depend on the substrate condition; therefore we could work on them independently. I believe that this conferred a significant advantage to Komag at the start of thin film media being introduced into the market.

In Chapter 7, I mentioned the problem we had at the start of 800 Oe media, where the random polishing mark would cause defects in the media. For this reason, we had to introduce concentric texture to eliminate the polishing marks. But this action would cause us to consider the tribology consequence of the texture, not to mention its effect on corrosion performance. But the texture had no effect whatsoever on magnetic properties of our isotropic media, therefore we could concentrate all of our efforts on non-magnetic related concerns. Again, I believe that this gave us considerable advantages compared to oriented media where the texture had significant impact on the magnetic film properties. It is this separation of magnetic development from the effect on tribology and reliability that allowed us to excel in tribology, and also in being able to develop a new generation of media very quickly. This provided us with a competitive edge. We also promoted this idea to our customers. In order for companies like Lin Data and Trimedia to successfully make oriented media, they needed to have a substrate with a nearly pure concentrically textured surface to induce uniform magnetic properties. Additionally, they needed a mass production sputtering system with extremely good vacuum. At the time of their start-up in 1983 and 1984, I do not believe that there were commercially available texturing machines that would do an adequate job of texturing NiP substrates. There were fixed abrasive buffing machines that were used for the oxide disks, but these tools and the process were not ideal. Furthermore, the sputtering machine vendors were not aware of the need to have extremely good vacuum with low residual gas, especially low water vapor. By my reckoning, it took several years for the vacuum system vendors to develop sufficiently good vacuum in their systems to be truly useable for making good oriented media.

For the 800 Oe media product defect issue, we employed a tool made by Strasbaugh, which applied a rotating polishing pad on one side of the disk while the disk was held on a spindle which also rotated. By changing the rotation speed of the pad and the spindle, a variety of patterns could be put on the disk, although they were generally of the rose petal type, and still roughly concentric. The deficiency of this tool and method was that the disk could be processed only one side at a time. Also, initially the operation was heavily manual, in that the operator had to flip the disk to process each side. Even though this process was called "texturing" because of the fact that it had to leave a certain amount of roughness on the disk for tribology and stiction requirements, the process and materials were taken from basic polishing technology. We had a variety of pad materials and slurry formulations available to us. I believe that we were one of the first to apply this type of process, namely a slurry-based texturing process, on the disk. What we did not appreciate at the time was how superior this process was compared to doing it with fixed abrasives. The fixed abrasive method used tape with an abrasive coating on it. Basically, it was sandpaper on a roll. This arrangement had some advantages in that both sides of the disk would be processed together, and it was more amenable to automation. The important point about this discussion is that if one were after oriented media, one would much prefer the fixed abrasive path, because one could obtain the same texture everywhere on the disk surface. The Strasbaugh-based method created different textures at the ID and OD (inner and outer radius) of the disk, thereby potentially creating different magnetic orientation effects in these zones, which would not be very attractive. Of course for us, we did not care. For this reason, we pushed the Strasbaugh-based free abrasive texturing method, and this produced excellent surface quality and very good removal of the polishing scratches left over from polishing. As mentioned earlier, we did not truly appreciate what we had until we attempted to switch to fixed abrasive tape texturing many years later. The discussion of oriented media involves texturing of the NiP surfaces, therefore we will come back to this point later. There is significant interplay between substrates, texture and magnetic orientation with this discussion.

With regard to what happens between the Cr film and the Co-based magnetic films during deposition, this interplay is something that had been observed quite a while ago. There is a well-known work done in 1967 by J.P. Lazzari, I. Melnick and D. Randet (Reference 10-2) which showed that Hc of up to 400 Oe could be obtained by first growing Cr with a preferred crystal orientation of (001) crystal plane lying on the plane of the film, followed by deposition of cobalt film with its $(11\bar{2}0)$ crystal plane lying on top of the chromium (001) surface. This effect is called "epitaxy," in which the lattice of one film (Cr) promotes the specific lattice orientation of the second film (Co) to match up to the Cr layer. One can refer to the crystallographic notation in Chapter 5, page 101 to see this relationship. To make the crystallography discussion easier to understand, this notation means that cobalt's easy direction of magnetization lies in the plane of the film, making it appropriate for longitudinal recording. Lazzari et al used vacuum evaporation to grow the Cr and Co films. Later W.T. Maloney, using a sputtering method in 1979, reported similar findings. In the early 1980s, it was difficult to develop this technology for mass production because growing high quality cobalt alloy film epitaxially on Cr film requires a high quality vacuum. Typical vacuum systems always have residual gases, containing oxygen, nitrogen and water vapor, inside the system during film deposition. Chromium films are quite easy to oxidize, and this would quickly ruin the epitaxy of the cobalt film. At Xerox PARC, I had experimented with this concept on my sputter tool, and I found that I could not get good results, meaning that I could not obtain good epitaxy between Cr and Co films. I realized that Cr films were difficult to grow cleanly using the conventional vacuum tools of the time. What was mystifying to me was that concentric texture would cause an additional degree of magnetic anisotropy to be manifested. This effect is something I was totally unaware of and had never heard of before 1983. As such, it was not something that I would risk my start-up venture over, as mentioned before.

As to how the oriented media evolved initially, what I do know is that Lin Data and Trimedia were the first ones to start to produce this type of media. As I mentioned earlier, it had been clearly invented at IBM. One mystery to me is why IBM never patented this process. Instead, a very unlikely person in Virgle Hedgcoth ended up owning a patent on oriented media. My knowledge of Hedgcoth's development plan for thin film media was based on sputtering of a magnetic layer directly onto glass substrates instead of onto textured NiP/Al substrates. I learned this from him when I interviewed with him. Therefore I do not know how and when he switched to textured aluminum substrates and developed his own oriented media. I personally suspect that by late 1984, there was oriented media from Lin Data & Trimedia showing up in the market, and he was able to copy it and file a patent. His application was filed in late 1985 (Reference 10-3). Many years later, Hedgcoth teamed up with a lawyer to start collecting money from anybody that used the oriented media process and they got quite rich doing this. It is a side note to the history of thin film media, which to this day I am not sure just what happened. I eventually adopted the oriented media process at Komag too, but not until 1996.

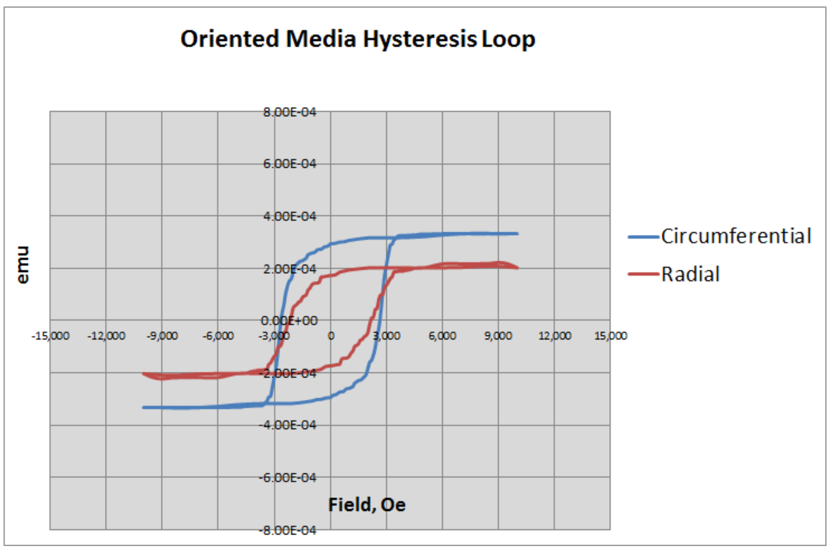
While we were busily expanding our business, the oriented media camp, which was just about everyone else in the business, was perfecting the process. There were probably two key breakthroughs that allowed the media to become truly excellent in terms of magnetic performance and orientation. First was development of a CoCrTa alloy, which was reported by Bob Fisher and co-workers in 1986 (Reference 10-4). He reported it when he was at Seagate, but I believe he had worked on it while he was at AIM. He must have been working on CoCr-based alloys, which was basically the alloy of choice for perpendicular media, which AIM was developing. Bob Fisher tried many different combinations of additive elements to CoCr, and Ta gave the best results in terms of high Hc and signal-to-noise ratio performance. Another crucial addition to the process was the use of very high temperatures. Something on the order of 200 to 250^{o}C was needed in order to obtain high coercivity. 1200 Oe was possible by using this combination and the alloy contained very high Cr content, in the neighborhood of around 20 atomic %. This was the sort of range of composition for Cr that perpendicular media was also using. I had always assumed that this combination would not really work because the magnetization would decrease very rapidly as more Cr is added. Therefore, for longitudinal media applications, I had assumed that a high Cr alloy would not be very attractive. However this composition really started to work because at the high temperature and with the addition of Ta in the CoCr alloy, Cr begins to segregate into the grain boundaries and magnetically isolates the grains, thereby providing excellent noise performance. Magnetization in the cobalt grain was higher due to being depleted of Cr. The high Cr content also provided superior corrosion resistance to the media. It is not so appreciated that oriented media owes some of its success to the CoCr alloy that was developed for perpendicular media applications. It is also possible that Bob Fisher had read my paper on annealing of CoCr films and the Cr segregation effects that I observed. It may have been the reason why he decided to use high substrate heating for his CoCrTa alloy.

The second breakthrough had to do with improvements in the Cr underlayer. Initially the Cr grains were very large, and CoCrTa film growing on top of it followed the contours of the large Cr grains underneath. The cobalt grains broke-up into smaller subgrains on top of the large Cr grains, but isolation within this subgrain was never quite as good or as large as it could be between the Cr grains. Many different Cr alloys were tried and various tricks put into the Cr process to try to break up the grains into a smaller size. But when this was done, the coercivity dropped also. I believe that finally the combination of Cr alloys and much better vacuums finally started to improve the Cr grains so that thinner Cr could be used to get good cobalt alloy structure. The advent of static sputter tools made by companies such as Anelva, Intevac and later by Balzers provided better vacuum and higher substrate temperatures with a high degree of uniformity. It was these improvements in the deposition systems that started to turn the tide for oriented media. This entire process took many years.

As was the case for us using isotropic media, many things had to happen before oriented media really became competitive. In the early 1980s, the entire industry suffered from a lack of good sputtering tools for making a breakout product. We had our share of problems in getting the first ULVAC sputtering tool to work well, and it took ULVAC about 3 design iterations before things started to get better. The CPA sputtering tool was originally made for single-sided sputtering, since it was really intended for semiconductor applications. It meant that one side had to be sputtered first and be re-sent through the machine a second time in order to coat the other side. Just as we suffered from having to take the parts out to coat the other side on our batch S02 and S03 tool, this was a poor way to make the media. The two sides of the media would never come out the same and the second side usually suffered from a large increase in defects. Apparently, Trimedia had to deal with this type of issue at first. Another company that made sputtering tools was Varian, which made a model called the VDP 1000. Several companies made their bet using this tool to start up the company. It was not particularly a good tool either, but the problem was that Varian gave up the business and discontinued the line, leaving many of its customers literally "holding the bag." We were fortunate in having picked ULVAC because at least they stayed in the game and kept trying to improve the tool. Having the ULVAC tool by itself was not the only key to success, as Cyberdisk had purchased the ULVAC in-line tool and failed to deliver. IBM had purchased a very expensive version of the ULVAC in-line tool about the same time as us, but they did not make much progress with it until three to four years after it was installed. Others, out of desperation or misplaced judgment, tried to design and build a new sputtering tool by themselves. The problem with this strategy is that it usually takes much more time than first imagined, and it ties up a significant amount of money and resources. Trimedia and Domain tried to go this route and both failed to deliver in time or failed altogether. Domain tried to design and build their own sputter machine, and it really did not start to work until Conner Peripherals purchased what remained of Domain. Later Seagate bought Conner Peripherals and they continued to use this sputter tool and made many improvements to it. Given enough time and resources, most machines can be made to work eventually. For a start-up company, there is not enough time and not enough money to do such a thing. You only have one chance to succeed, and with a fairly limited amount of time.

The key feature for oriented media using the CoCrTa alloy is that it required very high temperatures. Considerable work had to go into both the aluminum substrates and the NiP plating process in order for the substrate to stand up to high temperature processing, and this took some time to accomplish as well. Aluminum substrate suppliers had to use high purity alloys and introduce multiple annealing steps in order to stabilize the substrate. The NiP plating chemistry had to be adjusted for high temperatures and the annealing step had to be introduced after plating in order to prevent disk warping during sputtering, and also reduce outgassing. A high degree of control in polishing removal had to be developed to make the NiP thickness the same on both sides of the disk after polishing. This prevented uneven stresses from developing, which could warp the disk when heated. These are just some of the difficulties that had to be overcome for oriented media to become manufacturable. Sputtering tools also had to be designed for high substrate temperatures as well. This is not so easy, especially since there is very limited time available to heat the disk in a sputtering tool, and it has to be done uniformly across the entire disk surface. Sputter system vendors had some work to do before this was done well.

While we enjoyed a great deal of success using our isotropic media process deposited at ambient temperatures, we put some effort into understanding the oriented media process through research and keeping an eye on current developments in the field. We assigned Tom Yamashita to lead the effort and he purchased one of the early models of the Intevac MDP sputtering tool, designed for making oriented media, and he hired some capable scientists to study the process. We also funded a research project at Stanford University to conduct microstructural characterization of oriented media. This was not the only purpose for the project but at least we did not care whether Stanford published the work on oriented media structure. There was a considerable lack of understanding about how the orientation effect developed, and it was useful to study this from a more fundamental perspective and approach. For example, the characteristic hysteresis loops for the media, as shown in the figure on the next page, are quite different along the texture vs. across the texture. The orientation effect provides significant overwrite advantage along the texture direction, and also has some improvement in signal-to-noise ratio. The effect was clearly coming from the chromium underlayer but what was it due to? Was it the crystallographic orientation of chromium along the texture that caused the orientation to take place for the magnetic layer? We had basically concluded that stress was the main cause, in which the texture line somehow imparts differential stress along the texture line on chromium that is also transferred to the magnetic layer. Therefore we believed that the effect was predominantly due to the magnetostriction effect in the magnetic layer. A considerable amount of x-ray diffraction and sophisticated TEM analysis was done to determine whether there was a contribution from a crystallographic component as well, but the results were never quite conclusive. There were about as many opinions as there were x-ray and TEM experts looking at them, and we had our own internal disagreements that took place over the interpretation of our data. The magnetostriction effect was one piece of data that was very clear and I tended to believe this data more than anything else. One paper that does a good job of explaining various observations is by K.E. Johnson et al (Reference 10-5).



Hysteresis loop of oriented media along the texture (Blue) and across (Red) showing the difference in squareness of the loop. (Figure 10-1)

I had mentioned that we had used a free abrasive process for texturing, using a tool from Strasbaugh. Although this had been serving us well, the tool was not very amenable to automation, and use of free abrasive was quite messy on the production floor. In setting up AKCL, they were much more sensitive to manpower issues and were not very happy with the process and equipment we had been using. In order to improve, we set up a team headed by Fuyuki Habu, a superb engineer and a manager at AKCL, to come up with an alternative and invited him over to Komag in the U.S. to work on process development. The process he developed was a fixed abrasive texture process that would match up favorably with the free abrasive process that we had been using. More importantly, because the process was done on both sides of the disk simultaneously, it would be amenable to automation and it would be more "elegant" compared to the messy slurry based process that we had. After over a year of effort, the process was finally developed and used on a media model that we called "Pegasus." This product was wildly successful, but it had some difficulties in terms of how it behaved with heads in the case where flying height was reduced. By then, the Strasbaugh process we had been using was being spread around by the people that had left Komag, and competitors were starting to use them, as they adapted the process for their oriented media processes. One of the more flagrant cases was when I visited Trace on a discussion about a merger with them. What they set up for their texture process looked exactly like ours. Needless to say, I was quite angry. In any case, we had thought that fixed abrasives were more attractive and we moved away from free abrasives, while our competitor was going toward our free abrasive process instead. The point of this is that the free abrasive process was superior for obtaining a better surface for tribology, flyability and defects, and it was possible to tune this process for oriented media as well. We had gone the wrong way with the fixed abrasives and after about two generations of products with fixed abrasives, we reverted back to the free abrasive process. By then, a number of manufacturers were making texturing tools that used tapes made with polishing cloth material and the tool could be adapted to using free abrasives. This finally allowed for a more concentric and uniform texture pattern over the entire disk surface, and also provided a pathway for easy automation. Also it did both sides of the disk simultaneously. At this point the advantage that we had over the oriented media in terms of texture flexibility started to disappear.

One more "ploy" that we tried to use as an advantage of isotropic media against oriented media was a concept of zone textured media. This idea was put to customers around 1994 and it was as follows: only the ID region of the disk that needed the contact start-stop would have the texture; the rest of the surface would be left alone in an as-polished state. We believed that this would be a great selling point for our isotropic media as the magnetic properties would be the same between the textured area and the as-polished region. With the use of a new texturing tool that used tapes, we could position the texture region fairly precisely over the CSS (contact start stop) region, while the smooth area would afford closer flying by virtue of the fact that head would never land there. In fact we had been practicing a sort of graded form of texture where the ID was somewhat rougher than the outer region of the disk for better flyability but it did not have a precise transition between landing zone at the inner radius of the disk to the data area. When we tried to produce the zone-textured media, things actually did not go as well as we had hoped. It turns out that the as-polished surface was not good enough in terms of defects and smoothness as we had assumed. When we put the media on top of the as-polished surface, the corrosion resistance of the media actually degraded. Because we did not have a Cr underlayer, our media was always somewhat more prone or sensitive to corrosion compared to oriented media, and we had to be more careful. The source of corrosion in the smooth surface was due to small microscopic polishing scratches that were left there from the polishing step. We had been removing some of them by the texture process but even in this case, not so completely. It was clear that we had to do a much better job in polishing the substrates.

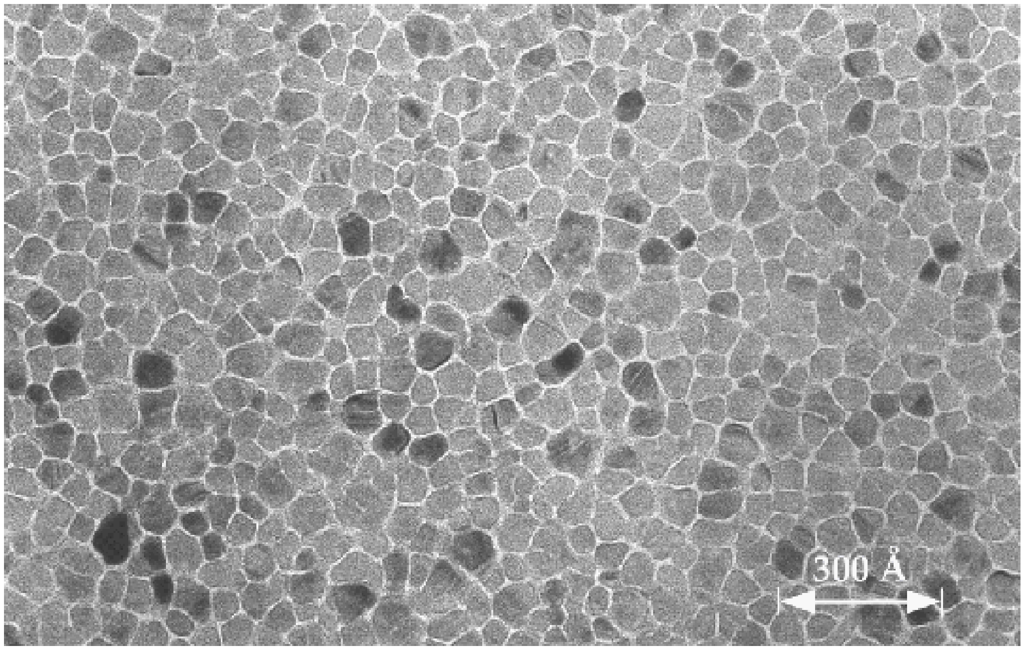
Instead of the one-step polishing that we had been practicing, we had to move toward a two-step polishing process, so that the first step can planarize the as-plated NiP surface quickly and efficiently. The second step would use finer abrasives to provide smoother and finer polishing of the NiP. By 1997, we had developed an entirely new type of polishing process for the second step that used colloidal silica slurry, which had been in use in semiconductor polishing for a while. We adapted it using colloidal silica particles with a new chemistry appropriate for NiP so that we could obtain a nearly perfect surface finish, down to only a few angstroms of roughness, with virtually no polishing marks left on the surface. The kind of surface that we were able to obtain was actually superior to the Si surface that was being used in semiconductor processing. This development in NiP polishing turned out to be very critical not only for isotropic media but also for oriented media as well. We needed to start out with a superior polished surface in order to advance the texture process in the case of oriented media, isotropic media and ultimately for perpendicular media which did away with the texture altogether. I believe that we were the first to develop colloidal silica slurry for NiP polishing, but like most other things that we had developed, eventually the vendors for slurry started to sell such formulations to our competitors and as we lost our engineers to competitors, our trade secrets were disseminated as well.

The idea for a zone texture process took a different turn as Seagate decided to use what is called laser texture to place an array of microscopic laser created bumps in the landing zone, instead of a mechanically induced texture as we had proposed. Seagate had patented a method to form laser heated bumps on a NiP surface many years earlier. One of the inventors of this process was Rajiv Ranjan, who had come over to Komag and worked on developing new magnetic alloys for us for many years (Reference 10-6). Seagate had sat on this invention for many years, not taking much of an interest, until the idea of zone texture came along. IBM was first to commercialize laser texture. Seagate's initial results using this method were quite promising and they had decided to use this on all future products.

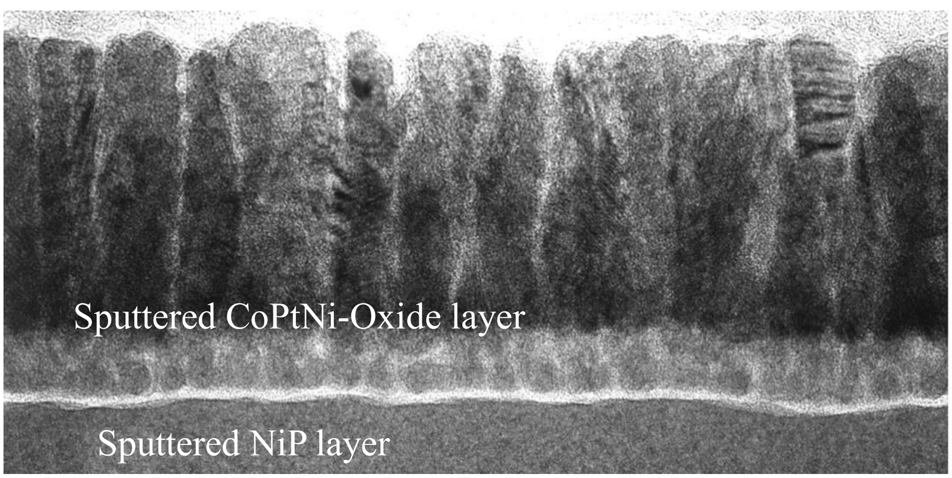
Seagate, being one of our customers, told us to do the same, or else they would not buy our product any more. The problem was that they had developed a laser tool to do this, but they were not willing to provide the tool to us. I believe the fact of the matter was that they had not fully developed the tool for themselves either, and were probably not in any position to support us. Faced with this dilemma, I quickly mobilized David Treves to construct a tool to do this function. In a very short time, he produced a manufacturing level tool that was very precise and reasonably priced, which could do this laser texture process on the disk. We were able to continue delivering media to Seagate with the laser texture process on it, without any help from them or anyone else for that matter. Eventually the rest of the industry followed suit, but capable machines made by outside manufacturers lagged for a few years and prevented our competitors from fully utilizing this new way of texturing. For this process to work well, it turns out that having a very high quality polished surface helps with uniformity of the laser texture. Here again, our substrate technology helped in a way that offered a competitive advantage to us. Laser texturing was even sensitive to the plating process and the plating chemistry. Even the type of soap we used on NiP substrates to clean them after polishing had some effect on laser texture. When you have your own plating line and control the substrate yourself, the laser texture is much more consistent. Our competitors who were not controlling their substrates well enough should have had great deal of problems. If you were purchasing different substrates from different vendors, you would have encountered different laser texture responses, and that would have been a nightmare to manage and control.

Where texture was required for providing a better surface than what the polished surface could provide (for isotropic media), and for providing good magnetic orientation for the case of oriented media, one of the key developments was in the types of abrasives that were available. Around 1995 or so, fine synthetic diamond slurry started to become available in large quantities and at very reasonable prices. There were two ways to go about making such fine diamond powder. One was a hydraulic press method that had been developed by GE many years before, which became more refined and commercially viable. The second method was by using high explosives to create fine diamond particles in an enclosed vessel. This second method apparently used military grade high explosives, such as C4 or the Russian counterpart such as hexogen. Not everyone had access to such explosives of course, and it was due to the ending of the Cold War that Russian and Chinese Military organizations began a side business of producing diamond powders for profit. I believe that availability of this type of diamond abrasive revolutionized many industries where cutting and polishing were required. We started to use such abrasives for texturing the disk, and the results were quite remarkable. The diamond powders are extremely fine, but they still can cut extremely well. They can create very fine cutting lines on a NiP surface that are nearly at atomic scale, without causing jagged edges, which were prevalent with the alumina-based slurry used previously. Of course, there were also very important advances in the coolant and suspension agents that were used with these diamond slurries, but the effect on the quality of the surface that one can obtain was very substantial. What was remarkable was that even with such microscopic texture lines, that are near atomic scale, an orientation effect still takes place with Cr and CoCrTa-based alloys. With such texture, the past disadvantage that oriented media had with respect to isotropic media in terms of being forced to use rougher texture disappeared. We were on an even footing as far as texture was concerned for media. In addition, the use of laser texture did away with having to worry about controlling the texture for tribology and CSS. The need for an ultra-high smoothness polished surface remained and became even more important, as these ultra-fine diamond slurries did not remove much material, and the starting surface had to be very good to begin with.

The key and primary reason why we had to change from isotropic to oriented media was, because around 1995, the industry started to use MR heads (soon followed by GMR heads). The gain in signal output from these new heads allowed drive makers to use narrower track widths to gain track density. Unfortunately when the track width decreased, the contribution from the track edge to the signal-to-noise ratio increased. There is always some field leakage from the write pole so that the amount of signal present at the edge of the track contributes to how closely the track can be packed together. The portion of media written by the edge field became extra noise to the signal because the state of magnetization in this area is different from regions within the track. It turned out that our isotropic media had a greater amount of track edge noise compared to the oriented media. In effect, we were writing slightly wider tracks compared to the oriented media. Additionally, the use of MR heads allowed thinner magnetic layers to be used due to their greater sensitivity and our isotropic media did not do so well when made thinner compared to oriented media. We started to lose squareness (because of non-uniformity in grain size development when the film is thin) and it affected overwrite, PW50 and even signal-to-noise ratio. This put our media at a significant disadvantage now against oriented media. We were headed towards a crisis and I decided that we needed to change all of our production to oriented media for future products.



TEM micrograph of the Komag isotropic magnetic film around 1996 timeframe. White regions at the grain boundaries are filled with segregated oxide phases, containing oxides of Co, Ti, Ta, and Cr. (Figure 10-2)



Cross-section TEM micrograph of Komag isotropic media showing its structure.
(Figure 10-3)

By 1996, when we hired Chris Bajorek to become CTO, I committed to him that I would see to it that we had a pathway to convert all our in-line sputtering systems to making oriented media. This had to be accomplished since we did not have enough time or money to completely change our sputter line to static machines (such as an Intevac or Anelva machine) to replace all our capacity that we had in our in-line sputter systems. So this task became my responsibility. This task was actually much more difficult than the "February Crisis" we had in 1985. In the 1985 crisis, at least we had proven already that a machine could produce the desired product, we just had to figure out what had gone wrong and what was causing the problem. For us, making oriented media in the in-line system had not been done before and we did not know what it would take. We knew that the sputtering machine had to have a better vacuum and that we needed to heat the disk to relatively high temperatures of over 200°C or more. Whether we could achieve the necessary vacuum and whether the RF sputtering that we had in the system was compatible with the oriented media process were things that we did not know. What we could go on was the fact that some of our competitors had managed to make oriented media using the in-line sputtering system, namely the ULVAC system used by IBM and Fuji Electric, and that Seagate was using their in-house developed MINT system. IBM also used Leybold in-line systems as well. So I sought out some advice from a few acquaintances in the field to find out about vacuum requirements for making oriented media. Of course, I was not able to get a straight answer, but I was able to confirm that they were able to make the machine work with only a small modification to its original configuration from the system vendors. Since we already knew that the majority of the water vapor in the system was carried in by the panel, we organized a task force under Ron Allen to come up with a design with ULVAC to pump on the panel more. Toward the end of 1996, we had proven that our modification worked and we modified all our in-line systems across the company by 1997. By 2000, we had an oriented process running on most of our tools. We also started to install Anelva sputtering tools to complement the output from the in-line system. We worked together with the AKCL team to develop the process for oriented media in the Anelva system and we started to purchase more Anelva machines for new capacity.

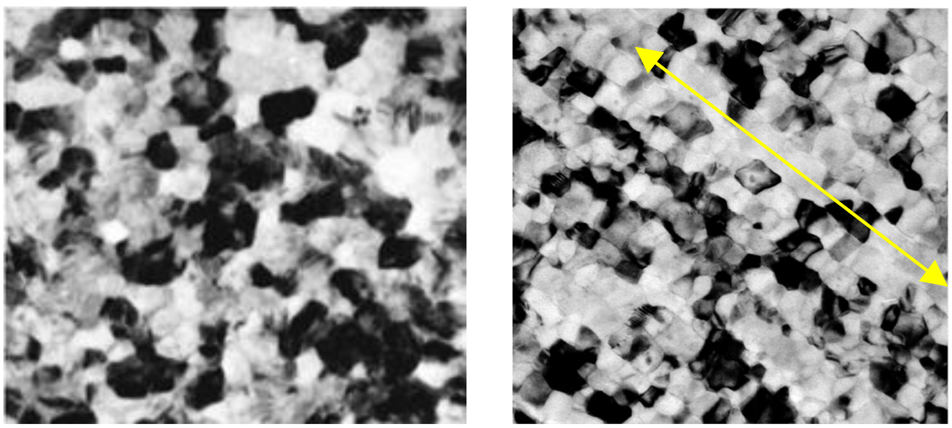
Even though we had been enjoying a great deal of success up until around 1995 and 1996, the introduction of MR heads took the industry into a trajectory of increasing areal density by 100% per year for several years. Unfortunately, the consumer needs for storage capacity did not grow at this rate, so that drives started to use fewer platters per drive. At the peak, the average platter count across all drives had dropped to about 1.2 platters per drive, and we saw the demand for media fall off to about half of what we had earlier. Starting from around 1997, and for about the next five years, we probably had the worst business conditions we had ever faced. The drive companies themselves were not spared either. So we were facing a classic "double-whammy" of falling demand and having to switch the critical sputtering process in the middle of this business crisis.

For the selection of static sputtering tools for the future, there were really only two choices, the Intevac tool and the Japanese Anelva tool. There was also a Balzers system but this tool did not have the user base that Intevac and Anelva had. These tools were really developed around making oriented media from the ground up. We had been using the Intevac R&D tool in the U.S. so that the U.S. team was inclined to go with the Intevac machine. Our AKCL Japanese subsidiary on the other hand was leaning toward the Anelva tool. Someone had to give the final go ahead for the choice and I overruled the U.S. team in favor of choosing the Anelva. I think my familiarity with Japanese vendors, especially the good result that I had with ULVAC was a big factor. Also I just thought the Anelva tool was built better and was more flexible as more chambers could be added later if needed. So this was the machine to go forward with for making the most advanced oriented media for the future.

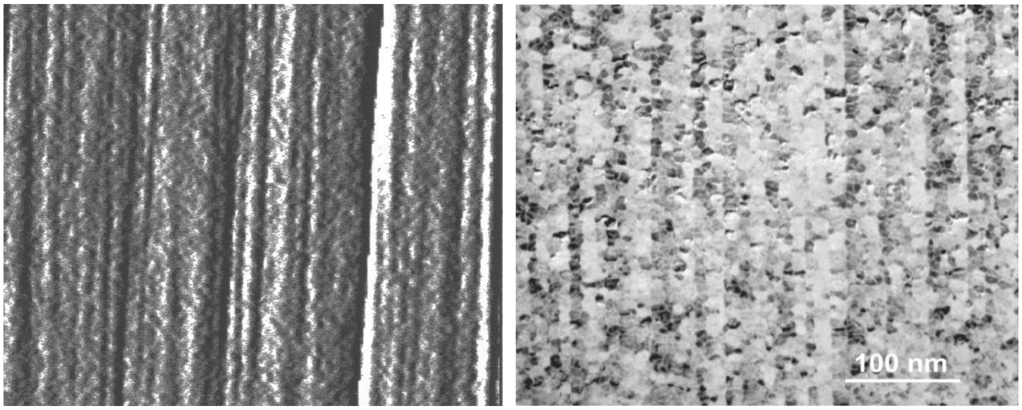
Developing the oriented media process for the ULVAC tool was very stressful and demanding. However, we knew what needed to be done from many years of watching developments in this field. One key issue was that the need for higher Hc was forcing everyone to start adding platinum to the target and this had the tendency of reducing the epitaxy and orientation effects. Our competition had already figured out how to overcome these issues, and it took some time and effort for us to figure this out for ourselves. I credit Gerardo Bertero for leading this effort under my direction and for getting this difficult job done. He was another student from Bob Sinclair at Stanford University's materials science department who we were fortunate to hire into Komag.

The key thing then for oriented media was that one needed a very good vacuum in order to achieve good Cr film growth, and good epitaxy between the Cr alloy and the magnetic film. The smaller size "static" or single disk sputtering tool is more ideal and easier to control as a process. For the in-line system, however, we had to put in extra pumps and try to reduce the water intake into the system. One lucky break was that we could continue to use RF sputtering for the magnetic layer so that target utilization continued to be good using the in-line system, while for the chromium alloy underlayer, it turned out that we had to use a DC magnetron. Apparently RF deposition knocked too much water off the panel as it sees the first sputtering cathode, which was the Cr target, so the decomposed water easily oxidized the Cr target and film. The net effect was that it created a massive amount of particles at the Cr station. The transition to 100% oriented media took several years to accomplish, longer than I stayed at Komag actually, because isotropic media was still accepted by some customers. Oriented media was made in both the new Anelva sputter tool and also in the heavily modified ULVAC sputter tool. The oriented media made by Komag quickly became very competitive with the rest of the industry's media, and great advances were made in its microstructure and areal density capability. Shown below are some of the TEM micrographs of the media, both the Cr alloy underlayer and the magnetic media, taken in 2003. This media incorporates the SAF (Synthetic Antiferromagnetic) coupling in the magnetic structure to enhance writability of the media to extend the areal density and thermal stability.

The ULVAC tool continued to make longitudinal oriented media at Western Digital after Komag was bought by them in 2007, and it continued to produce media until around 2010, the final product being a 320 GB/platter, 95mm media at a recording density of 260 Gb/in^2. The ULVAC tool had a much longer life than even I had expected. The static tools were still better in terms of producing superior product because each disk was made individually and with better consistency. For both oriented media and perpendicular media that followed, the static sputtering systems became the dominant tool in the industry, and today it is the only type of tool that is used by everyone.



TEM image of $CrMo_{15}$ underlayer of media taken in 2003. Left photo is for un-oriented $CrMo_{15}$ layer on a smooth Aluminum/NiP substrate. Right photo is on a textured substrate showing the alignment of Cr film grains along the texture direction shown by the blue arrow. Also substantial grain refinement as well as shape change can be seen. This change in CrMo grain morphology is part of the phenomenon that leads to magnetic orientation in the media. Grain size is approximately 8 nm, and the film is about 15 nm thick. (Figure 10-4)



Left photograph is AFM (Atomic Force Microscope) image of textured Aluminum/NiP surface. Texture is created by microscopic diamond slurry and scratch separation is of the order of one nanometer. The contrast is greatly exaggerated and the surface is much smoother than indicated by the AFM image. Right photograph is a TEM micrograph of the magnetic layer formed on ~~a~~ the textured substrate, showing the extremely fine nature of the grains, which are all aligned morphologically along the texture direction, and at scale of individual grains which are only ~ 8nm wide. The micrograph shows the refinements that ultra-small diamond slurry provided in obtaining an extremely fine orientation effect in the media. (Figure 10-5)

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Chapter 11

Seeds of Industry Chaos Starting in 1997 and Exit from Komag

By the end of 1996, the task of converting the in-line sputtering system from isotropic to oriented media was practically finished. After very hard work to understand the key elements that were needed for the process and equipment, we went ahead to implement the new processes and converted the in-line sputtering systems one by one across the company in both the U.S. and Penang, Malaysia. The conversion proceeded very smoothly and qualification of the new oriented media product was successful. Also, our new CTO, Chris Bajorek, started to settle down and was starting to take charge of R&D and setting technical strategies that had often been laid on my shoulders previously. I felt that my burden was relieved and I was looking forward to my retirement from Komag, and doing something else that might be more interesting for me. By this time I found that running a public company with shareholders to keep happy was not my cup of tea, and it did not give me the satisfaction like the challenge I experienced in doing research and development work. However, several board members who were with Komag from the very beginning convinced me that I was still needed and asked me not to retire. Therefore, I continued to work at Komag and work with engineers in both the U.S. and Malaysia. As it turned out, the decision to stay was one of the biggest mistakes in my life. For the next three years I had to endure the biggest chaos to hit the disk drive industry.

The root of the chaos started back in the 1994 to 1995 time period. During this time, the PC industry started to grow spectacularly, and Microsoft Corporation became the biggest success story in the world. Microsoft's operating system became the standard and started to become more powerful. The software for many applications demanded more storage capacity. Sometime in late 1994 or early 1995, Bill Gates, in one of his keynote speeches at the Comdex show in Las Vegas, said that the future software for PCs would be so powerful that it would require a data storage capacity of one terabyte. As Bill Gates's pronouncements carried so much weight, the drive industry responded. Everyone started to plan for a dramatic increase in storage needs, and changed their forecast for demand from 15% annual growth to 20-25% growth for 1995. Komag also got on the bandwagon, and started a frantic capacity expansion in the summer of 1995. As evidence, the demand for disk drives was very high in 1994 to 1995 due to rapid growth in PC sales, and media manufacturers were all having trouble keeping up with the demand. By 1995, we enjoyed our best financial performance to date, with historically high profits. Our stock price hit $50 in early summer of 1995, also a historic high. In the early spring of 1995, we held our annual offsite meeting with the senior staff and devoted most of our time to discussing our options for dealing with this growth opportunity. We even hired an outside consultant to mediate the discussion. He was supposed to be a guru on setting up a strategy for growth. During the discussion we generated three distinct plans labeled as A, B and C. Plan A was the most aggressive one, which was to take the full opportunity of the expected increase in demand for media by doubling capacity within the next 3 years. The mediator at the time suggested that if we were to grow our capacity according to Plan A we would become "monopsony" in the media business. This was a term that none of us had heard before, but I believe he meant the situation where there was only one major supplier for the key component, namely Komag. In hindsight, the advisor used the term incorrectly. Monopsony refers to a situation where the demand comes from one customer. At the meeting however, everyone understood that Plan A called for Komag to become the main supplier of media to everyone.

Plan B was actually my proposal and it called for only increasing capacity by the historical trend, which was 15% per year. It included using capital for developing an alternative business, which was to develop MR heads to complement media. It was what I had in mind when we bought technology from Siemens and the Dastek thin film head operation. I proposed this despite the failure of turning Dastek into a viable business. At that time, the MR head was on the horizon already, and Headway, which had been funded by HP and AKCL/Asahi, had shown good results by the spring of 1995 and expected to ship products by late 1996. I had in mind to acquire Headway in Plan B. From there I thought that it would lead us to becoming a vertically integrated company. Plan C was a proposal to sell the company to a drive company that was looking to acquire the capability of making the critical components for a disk drive, the heads and media, to become a vertically integrated drive company like Seagate. Seagate was the leading independent drive maker at that time. IBM also had HDDs and made their own components but they were part of a much bigger computer company. Since other independent HDD companies such as Western Digital, Maxtor and Quantum probably aspired to become vertically integrated, we felt that there was a possibility of selling Komag with a good premium to one of those companies at that time.

After two days of intensive debate at the offsite meeting the majority of the senior staff was convinced that Plan A would be doable and they voted for the plan. I think everyone was enamored by the prospect of becoming a "monopsony", that is like Intel or Microsoft as the single most powerful supplier of media to the industry. The success of Komag at that time also contributed to everyone’s thinking. It is often the case that at such times, everyone becomes arrogant and thinks that the whole world can be conquered. Plan B got no support because of the bad experience with the Dastek venture just a year earlier. The reason that I had more confidence in Plan B is that after dissolving Dastek, key employees had reorganized the company with refinancing from HP and AKCL to form Headway Inc. in the spring of 1994. HP invested into Headway because they had their own production capacity for making HDDs used by their own internal PC business. They wanted to take advantage of the emerging MR head technology to create the most advanced HDD products for their own PC business. Therefore, the new company's goal was solely to develop the MR head under the direction of HP. I was also involved in managing the company as a board member due to partial ownership of the original Dastek asset. I knew that the team I had assembled was quite talented. This team was able to produce a prototype MR head by the spring of 1995. Therefore, I felt that Plan B could have been a viable path to make Komag more diversified by integrating the head business with the media business on our own terms. The idea of selling out was something that no one wanted. In hindsight, we might have gotten the most value out of the company if we had chosen to sell out at that time, when we were valued so highly, even though many of us probably would have lost our jobs.

After Komag's management team decided to adopt Plan A, Steve Johnson prepared a presentation for a 5-year business plan, and presented it to the Komag board in April of 1995. The critical slides that he used are shown in the following table. After the board approved the plan, the entire company was mobilized to achieve the goal of doubling production capacity by 1998. Steve and the CFO, Bill Potts, went out to get financing for the plan in July of 1995. With very positive market perception of the disk drive industry at that time, and the good story that Komag told, we were able to issue 1,750,000 new shares, with the option to issue an additional 262,500 shares to investors. We were able to raise over $200 M at the offering price of over $70 per share. (Reference 11-1)

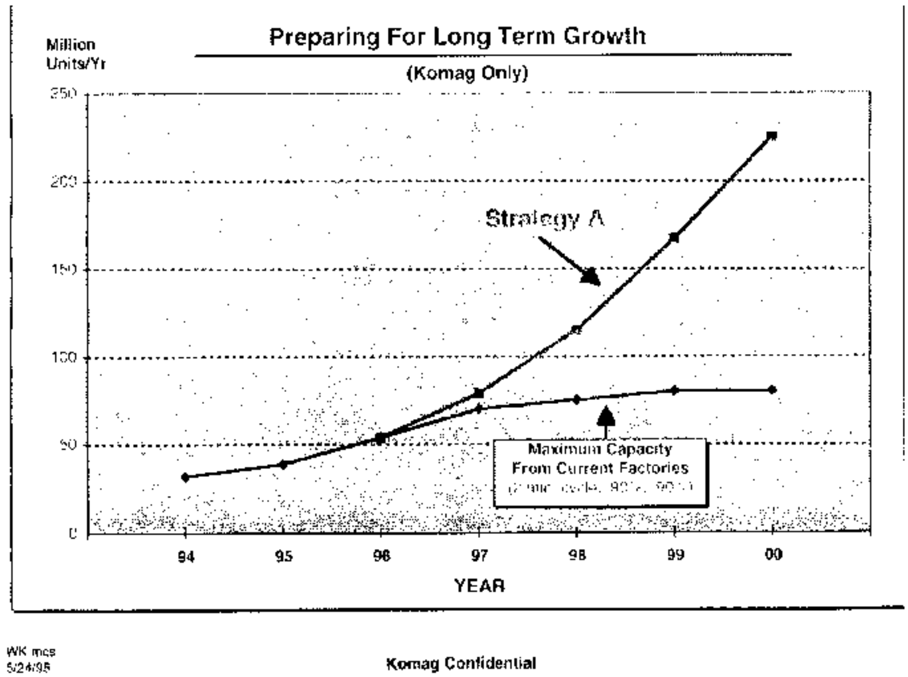
The Plan A as we presented to Komag board is shown below:

WHAT WE LEARNED AT OUR OFFSITE

	1994 FIVE YEAR PLAN ENVIRONMENT	1995 FIVE YEAR PLAN ENVIRONMENT
DRIVE MARKET GROWTH EXPECTED	15% FOR THE NEXT TWO YEARS	20% TO 25% UNTIL THE END OF THE CENTURY
TECHNICAL LEADERSHIP	SHOWA DENKO MEDIA BETTER MAJOR PROCESS CHANGES EXPECTED AND UNKNOWN	KOMAGCLEARLY NUMBER ONE PROCESS PROVEN AND EXTENSIBLE TO NEXT TWO GENERATIONS
MARKET SHARE	24%	24%
ACTION	RETRENCH	GO FOR IT!

...WE ARE IN A MUCH IMPROVED POSITION

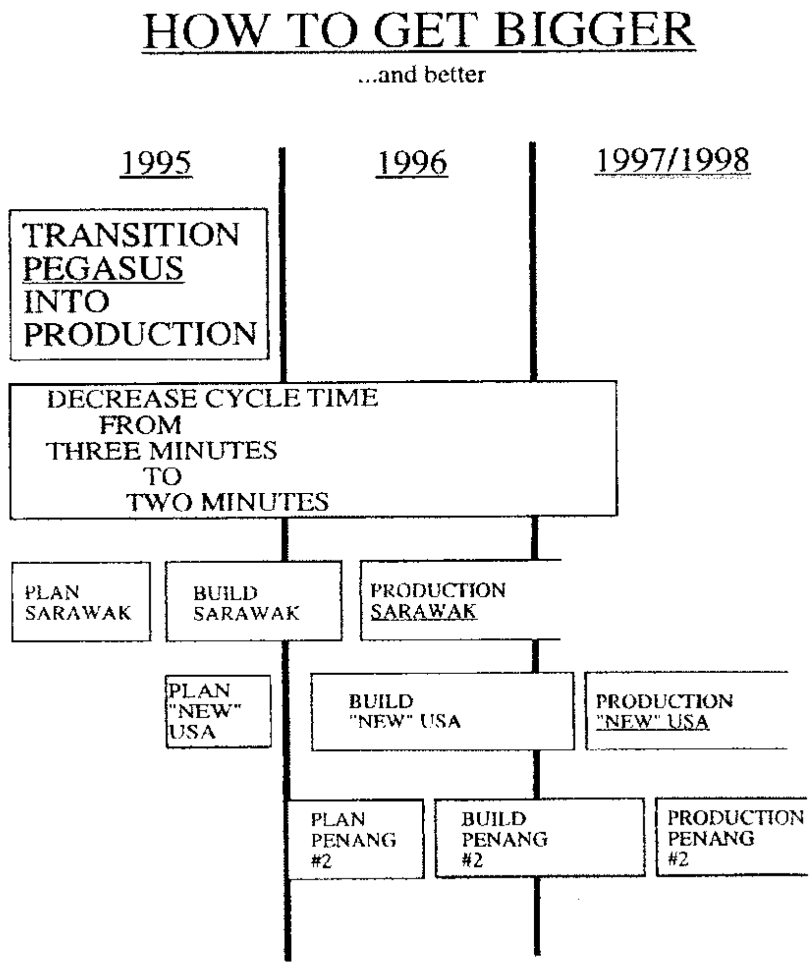
Komag internal document, outcome of off-site meeting to plan the future of Komag in 1995. (Figure 11-1)



Material prepared for Komag board meeting 1995. (Figure 11-2)

After Plan A was approved by the board in the April, 1995 meeting, Will Kaufman and the financial team created a 5-year operating plan for the expansion called the "Long Term Capacity Growth" plan as shown in the following figure.

With $200 M from the new stock issue, existing cash on hand of about $400 M, and a new bank line of $202 M, we had the war chest to try to execute Plan A. We then proceeded to build a giant new manufacturing facility in San Jose (Building 11) and a second large manufacturing facility in Penang, Malaysia. The capacity from these two new buildings would be enough to double our production capacity once they went into operation by late 1997.



Material prepared for the Komag board meeting, 1995 (Figure 11-3)

Unfortunately, among all this hype everyone, including myself, did not see the impact of the coming MR head technology on disk performance requirements. Additionally, the MR head would start to dramatically increase the areal density capability of the drive, so much so that the industry achieved an incredible 100% increase in areal density, compounded every year for several years. That was doubling the recording density year after year. Unfortunately, the consumer did not need the storage capacity that this increase would provide, contrary to Bill Gates's prediction about needing 1 terabyte of storage. The net effect was that the number of disks used in the drive plummeted, so that the average number of disks per drive quickly approached just one. Not even the HDD manufacturers saw this coming. HDD prices also plummeted and now we had a glut of media and heads. The industry really out did itself spectacularly with the new technology, but everyone got badly hurt in the process.

When the industry was still using inductive thin film heads, in 1995 to 1996 we were able to qualify successfully into customer programs using our 1,800 Oe products (called Pegasus) using isotropic media. While in some areas of performance, such as in overwrite, we were not as good as oriented media, our disk's noise performance was still better. For the following generation products, from 1996 to 1997, our products with a coercivity of 2,000 to 2,200 Oe continued to do well without major issues using the same isotropic media process and equipment. Therefore, the decision was made to adopt the same equipment and processes for our capacity expansion. To reduce cost, I even suggested to the manufacturing department enlarging the sputtering machine to double the throughput of disks relative to the first generation ULVAC sputtering machines. The new machine was called the ND machine (for New Design) and the objective was to decrease the footprint for a given capacity, thereby reducing the overall facility cost and increasing productivity. We implemented the first of the new ND machines in 1996. We found that the machine was able to double throughput, and the product was as good as or better than we had expected because the machine was designed to have better vacuum and a dryer condition than all previous machines. Therefore, it was easier to control the ambient conditions in the chamber. The reactive gas sputtering process that we had been using seemed to work better in the ND machines. Unfortunately, the excessively large capacity became a big liability when the demand for the disk softened. Komag had to discard the ND sputtering systems when we downsized. ND was just too big. We ordered a total of four ND tools from ULVAC, but we had to scrap two of them without even installing them (Reference 11-2).

In 1996, when the MR head started to appear on market, we noticed that isotropic media were inferior to oriented media in overwrite and pulse width or PW_{50} characteristics. The main reason was due to the narrower tracks that an MR head was able to use. Drive makers started to take advantage of the high signal strength generated from the MR head to increase the recording density, favoring narrower tracks. We should have seen this coming since we were working closely with HP on development of advanced MR heads by 1994. What the narrow track recording did to isotropic media was something that caught us by surprise. Thinking about it now, the reason why one tends to neglect the most important technologically critical aspect of our business, at a most inopportune moment, is because we became enamored with our own success. When we put Plan A together, things were going very well indeed, and as human nature had it, one started to believe that it could keep going for a long time by doing the same thing. The main concern was over losing market share while there was a fundamental paradigm shift starting to take place with MR head technology. We became too complacent as a result of our continuous success for the first ten years. This could have happened to any publicly traded company that became too preoccupied with growth and forgot about the possibility of a paradigm shift in the business that could change the game completely.

As I said earlier in chapter 8, the introduction of MR heads in 1996 forced us to change our sputtering process from making isotropic to making oriented media in the middle of our capacity expansion. Unfortunately, after we did all the modifications needed to make the in-line sputtering machines capable of producing oriented media, we found ourselves with tremendous overcapacity for media in both the U.S. and Penang. There were some extraneous circumstances as well that caused the oversupply:

1) The HDD forecast for 1995 was never realized due to the financial crash in Asia and slow growth in the U.S. and in Europe.

2) The areal density increase due to MR heads started to reduce platters in the drive. Originally drives had around 4 disks/drive, which quickly dropped to 3 disks, and later to only one disk.

These two events caused big chaos in the disk drive industry and severely hurt many drive makers. The ripple effect was felt by the component suppliers. The drive makers themselves had also bought into the hype of the huge demand for drives predicted in 1995, and they also expanded their production capacity across the board. Fearing shortage of key components, particularly media, many of them started their own media operations. This was the case for Western Digital and Maxtor. Korean companies were enjoying boom times in the early 1990s, and Hyundai in particular, went on a buying spree. They took control of Maxtor and formed MaxMedia as a joint venture with Maxtor to make disks. Seagate merged Connor's media operations with their own, and Trace in Taiwan was able to get sizable funding from the Koo Family to expand their operations. In Japan, Showa Denko and Fuji Electric started to invest heavily into media as well.

During this time period, media manufacturing was becoming more of a turnkey operation as many vendors started to not only supply equipment but also the process to go with it. This was particularly the case for the oriented media process with Balzers, Intevac and Anelva, all supplying static sputtering tools with a process that could be started relatively easily. There was also a lot of movement of talent from one company to another, which disseminated information about disk making secrets to operations that wanted to start up. We lost many people during this period to competitors. Komag was called "Komag College" in those days as we had the reputation of having trained many technical professionals that went on to other companies to start up various competitors' operations. In addition, drive companies could quickly hire away engineers from independent disk makers such as Komag and HMT to start up their own media operations. Unfortunately, by 1997, when media supply became too high, the vertically integrated companies started to pressure the independents for price cuts and the independents had to undercut each other for the little business that was left. By 1998, the disk price dropped to around $5 from the $10 that we enjoyed in 1995. In order to reduce capacity and cost we eventually had to stop U.S. production (Reference 11-3). By 1999, we shut down our newest and most advanced factory in San Jose, and took most of the production equipment from the U.S. to Penang (Reference 11-4).

The market crash in 1998 was also painful for the drive manufacturers. They were losing money on making the drive, and their fledgling media operations were also losing money. In fact, their cost for making the media was actually higher than buying it. This was due to the smaller scale of their operation, and they had not yet achieved the high productivity and yields that took us many years to achieve. They often did not possess their own substrate operations either, and they had to buy them. This also added to their cost. Additionally, they had started their operations in the U.S., and their labor and operational costs were much higher than for us. As for Komag in this difficult environment, we were slightly better off because we had invested early in the Malaysia manufacturing facilities for finished disks and substrates, and our volume was the largest, so that our cost was still the lowest in the industry if we had not had the excess capacity at that time due to Plan A.

In 1998, all the disk media manufacturers were struggling to figure out how to survive. There was talk of mergers or buyouts. I was invited to visit Taiwan by the investors in Trace to visit the company and discuss a possible merger with Komag. There was also a proposal to merge with HMT. HMT was in even more serious difficulty because they could not raise capital by a stock offering in 1996, and they raised the money for expansion by essentially borrowing money in the bond market with a convertible debenture. They used this money to expand their capacity in the U.S. This might have been ok when the market was good and the price for the disk was high, and the stock price was on the rise. However, by 1998 everything had collapsed including the stock price. They would have had to pay back the bond with cash, which they did not have (Reference 11-5).

Since everyone had excess capacity, the price was lowered in order to maintain market share. In late 1998, Western Digital was in serious difficulties as they were late to MR head technology and losing market share very fast. They had to unload their media operations in order to raise some cash. Western Digital then offered the media operations to HMT and Komag, with the proviso that the buyer would have been able to obtain a volume purchase agreement (VPA) to get the majority of the media business with Western Digital.

At that time Komag and HMT were the two major suppliers to Western Digital and Showa Denko was a distant third. However, since Showa Denko was a big Japanese chemical company that produced many other products besides thin film disks, they were probably not in such dire straits as Komag and HMT. Therefore, Wetern Digital figured that only the two independent public companies hungry enough to kill each other would buy their media operations. It was a life or death situation. Whoever would have been able to buy Western Digital's media operations would have had a chance to survive. We bought the Western Digital media operations for $80M in February of 1999. We figured that this would effectively kill off HMT.

The MaxMedia operation was also in trouble during this time period. The Hyundai empire was in trouble itself due to the economic crash in Korea and it was looking to get rid of MaxMedia. MaxMedia's president at the time was Nick Pigniati, who had worked for us at Dastek. He approached us about merging MaxMedia with Komag. We had discussions but unfortunately we could not figure out how to do it. Actually, there was no financial or strategic reason for doing the merger with MaxMedia at the time. On the other hand, we had to buy Western Digital's media operations. Western Digital was a major customer and losing them would have meant a severe problem for us. Our offer had the most credibility since we had the overseas production capability with the largest volume. We could ultimately offer the lowest cost media to Western Digital compared to HMT. Our technical capability was also viewed as superior to HMT's with our much longer history and reputation (Reference 11-6).

After the acquisition of Western Digital's media operations, Western Digital became the largest Komag shareholder. Unfortunately, the merger did not bring anything useful except a committed customer's business. Komag had to close down the entire Western Digital media operations and lay off most of the employees to stay alive in April of 1999. In effect, we took the entire cost of closing down Western Digital's Media operations from Western Digital. Their equipment was quite incompatible with ours in any case. They had Balzers sputtering tools, which were different than our in-line and Anelva systems. We had relatively little interest or need for keeping the Balzers tools (Reference 11-7).

By this time, I was so tired and frustrated, particularly with the negotiation to try to change the covenant for the $202M bank loan that did not go anywhere. I was finally ready to retire. After over 16 years of hard work, sometimes 24 hours a day and most of the time more than 10 hours each day, I was just spent. In the end, it was my failure to see the impact of an emerging new technology (MR heads) that resulted in bringing down the company that I put my heart and soul into. I also felt that I would not be able to protect the employees from the coming storm, particularly those who often worked with me day and night and often into the weekend. I could not bear it. It was time for someone else to lead that company. I found that the business world is cold and uncaring. By the spring of 1999, when Craig Barrett resigned from Komag's board to become CEO of Intel, I decided not to participate as a candidate for the board for the 1999 to 2000 term. When Craig resigned, he recommended Michael Splinter to replace him. I interviewed Mike in April and encouraged him to become a Komag board member, and he agreed. Due to this change, I had to stay. I joined the roster of the board candidates and was re-elected as the chairman of the board again. I remember at the April board meeting after the board elected me to continue as chairman, I told them openly that I was now prepared to retire and that I saw the chairmanship as a temporary situation. When Mike Splinter heard this, he was quite angry with me as he had expected that I would continue. When I expressed my desire to retire, Steve Johnson also announced that he also would like to retire as well, which did not help the situation. This made the entire board very angry. In the end, I had to promise to stay for a while to help out and see through the difficulties ahead.

After we bought the Western Digital media operations, one of the Komag board members was approached by HMT to act as middleman in a proposal to merge with Komag. Steve and I tried to come up with some good reason to go through with the merger. In the end, we concluded that it made no sense. We had just wasted a big chunk of company shares to buy the Western Digital media operations only to write it all off, and we did it partly to kill HMT. We were succeeding as they were desperately trying to merge with us now. In addition, HMT had an even larger bank loan on their books, as a convertible debenture which would have come due in a couple of years. This was a poison pill, which we did not have to take. Additionally, HMT did not have any low cost offshore manufacturing operation and their substrate facility was in Portland, Oregon. They could not possibly compete with our Sarawak, Malaysia substrate operation. Therefore, I felt that if we worked hard, we would beat them at the end and they would disappear. Unfortunately, after I retired in August of 1999, the new board and T.H. Tan, who took over as CEO of Komag, decided to buy HMT. This move, in my opinion, drove Komag into bankruptcy and in the process wiped out any value that employees and investors had in Komag. I also held the majority of my original shares of Komag to the end and all of it was wiped out (Reference 11-8, 11-9).

By the summer of 1999 we decided to move all the manufacturing operations to Malaysia except for the R&D building (Building Ten). The plan for the R&D building was also proposed in Plan A in the spring of 1995. In the previous ten years, since Komag first installed S1, R&D never had any dedicated research equipment for developing production processes except for some analytical instruments and simple polishing equipment. For development of new products and processes, R&D had to share time on manufacturing tools. When it came to very expensive sputtering machines, the time for R&D development was often the lowest priority, especially when we were short of capacity. During the first ten years, we were usually capacity constrained. The benefit of using production tools for development is that transfer of processes from R&D to production would be faster and smoother. We had this mode of development ever since the S1 sputtering tool, so that we were quite effective and timely in developing a manufacturing process from R&D development. Working with isotropic media technology allowed us to operate this way as we could change Hc by platinum adjustments and gas addition, for example. When more substantial materials development was needed, for example for developing a better nucleation layer underneath the magnetic layer, then it took more time on the sputtering machine. We were able to get away with operating in this manner for over ten years, and we became complacent. Particularly to those that did not appreciate how difficult technology development can be sometimes, when things seem to be going smoothly on the surface, it is hard to convince them that there may be troubles ahead if you are not investing in basic work with dedicated tools. When we decided to go for Plan A, we pushed the management for dedicated resources to build an R&D facility, which would be also capable of building new prototypes for new products. Since all the upper management was frantically at work on pushing production capacity for manufacturing, they did not necessarily help R&D create a comprehensive plan to present to the board. We had lost a critical voice for R&D when Jim Shir took a long leave of absence for health reasons in early 1990. He, more than anyone else, including me, could have helped Steve formulate a comprehensive R&D facility and funding plan to present to the board. When we showed our Plan A to the Board in the spring of 1995, the management team only gave a simple outline for a R&D facility budget with hardly any detail or justification. Consequently, several board members were not happy about the R&D plan and they specifically did not approve the part of the plan dealing with the R&D building. Consequently, I had to step in to write a detailed explanation and plan to Irwin Federman as to why we needed the building and how we intended to use it. Irwin Federman was the first outside board member of Komag; he was someone that I always relied on for good advice. He is a financial guy by training, but he is very smart and had experience in running a high tech semiconductor company. He was the president and CEO of Monolithic Semiconductor Corporation. Therefore, he was very familiar with the need for having an excellent R&D facility and capability. So I wrote him a 5-page letter to explain the whole concept and the reasons why we needed the facility, so that we could count on him to help sell the idea to the rest of the board. In the end, he helped us to get the project approved. As fate would have it, this R&D facility became the sole surviving facility for Komag in the U.S. We couldn't have developed the oriented media process without the line that we had for this sole purpose in the R&D facility. The same can be said of the development of perpendicular media later, in the 2005 timeframe. At the time that it was built, it was by far the best R&D facility for development of media technology that anyone had. Today the same facility is the core of Western Digital's media operations for development of new products and technology.

By the board meeting in August of 1999, I finally told the board that I would be turning 65 and that I needed to retire from Komag. What I said was that I did not want the responsibility of running the company anymore, but I would be willing to stay on a little longer as a consultant and a board member. Steve told the board that he wanted to resign as well. When the board heard what we had decided, they told both of us to go outside so that they could discuss it. About 20 minutes later they asked us to come back in and told us that they would like both of us out of the company that afternoon. They also rejected my offer to stay on for help. Surprisingly, they also decided to appoint T.H. Tan as the new CEO. This was not the outcome that I had expected, but the board was angry with us and it was their prerogative. So, just like that, in one afternoon, both Steve and I were out of the company that we helped to build for 16 years. As I was cleaning up my office to get out, T.H. Tan rushed into my office and told me that he needs my help for six months to start his new job. TH was as surprised as the rest of us for being appointed to the CEO position and he was totally unprepared for it. I told him that the board did not want that and he would have to ask the board himself for permission to have me stay on. Later that afternoon, he came back to my office and told me that board had approved his request. Consequently, I stayed with Komag for six more months to help him with the R&D and train people in Malaysia as a consultant, before I formally retired from Komag in 2000.

As for Headway, I continued to serve on their board as a vice chairman and actively participated in the management on a part time basis in their weekly staff meeting to see their progress in developing the MR head process, all the way up to the time they sold the company to TDK in April of 2000. The company became stronger and more viable after I asked Mike Chang to become CEO of the company. They also hired Tom Surran as CFO. Under this combination and with excellent technical leadership, the company started to make a profit in 1998, while most of the HDD related companies were losing money. By late 1998, Headway was preparing to raise money through an IPO. Unfortunately, the financial market for the HDD industry was in a very poor situation so that Headway was not able to do an IPO. They really needed money to expand however, because the MR head was just starting to take off and the business opportunity was excellent, much like where Komag was in 1985. In the later part of 1999, Mike Chang went to TDK to negotiate for funding. TDK knew Headway quite well, because TDK acted as contract manufacturer for Headway by building HGAs from Headway's wafers, and they also bought the processed wafers for their own heads. TDK realized that Headway could build the MR head better than itself, and TDK decided to buy the whole company for $120 M dollars in April of 2000 (Reference 11-10).

Today Headway became not only the source of MR, GMR & TMR technologies, but also the cash machine for TDK. They continue to produce leading edge technology for new heads from the same facility in Milpitas that they started with. I often wonder if Komag had a good management team and vision of following the S curve of emerging technology and had taken Headway under its wing, it might have been Komag that would have bought out the drive company instead of being bought out by Western Digital. Such are the thoughts of a retired engineer with too much time to ponder "what ifs".

As for AKCL, our Japanese operation, their fate was quite sad, as they started to lose Japanese customers for their media. Their main customers were IBM, MKE (Matsushita Kotobuki), Fujitsu and Toshiba. IBM's business was the first to go, as their internal media operation ramped up and forced all of their operations, including Fujisawa, to use the internal media. Fujitsu also started to ramp up their own media production and the business disappeared. MKE became the biggest customer for a while, but MKE built the drive for Quantum. When Quantum sold their HDD business to Maxtor, Maxtor did not see a need for MKE as they had their own drive manufacturing capability. AKCL's operating cost was also becoming a problem as they had to compete with the overseas' cost advantage that we had, and others such as Fuji and Showa had started to move some of their operations overseas. AKCL was prevented from doing so as we had territorial agreements as to where we could sell and produce. Komag was in a world of hurt trying to survive, and we could not have figured out for them how they could have survived. If they had succeeded in commercializing their glass substrate and media, they might have had a chance to survive by selling them to Toshiba and others. Komag did not have a glass media product. But this did not happen and this business was taken away by Hoya and Showa Denko. At about the same time that Komag was facing Chapter 11, AKCL closed the door completely on March 31, 2001, and Asahi had to write off about $120 M. Between the investment in Komag, AKCL, Dastek and their efforts on glass substrates, Asahi probably had invested close to $500 M as a guess, and at the end, had little to show for their efforts. Many of the people that worked on this adventure continue to work for Asahi, and they generally have fond memories of the time they spent with us. Many people left to join other companies, and a few joined Komag.

(Reference 11-1)
http://www.thefreelibrary.com/KOMAG,+INCORPORATED+FILES+REGISTRATION+STATEMENT+FOR+1,750,000+SHARES...-a017293048

(Reference 11-2)
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Chapter 12

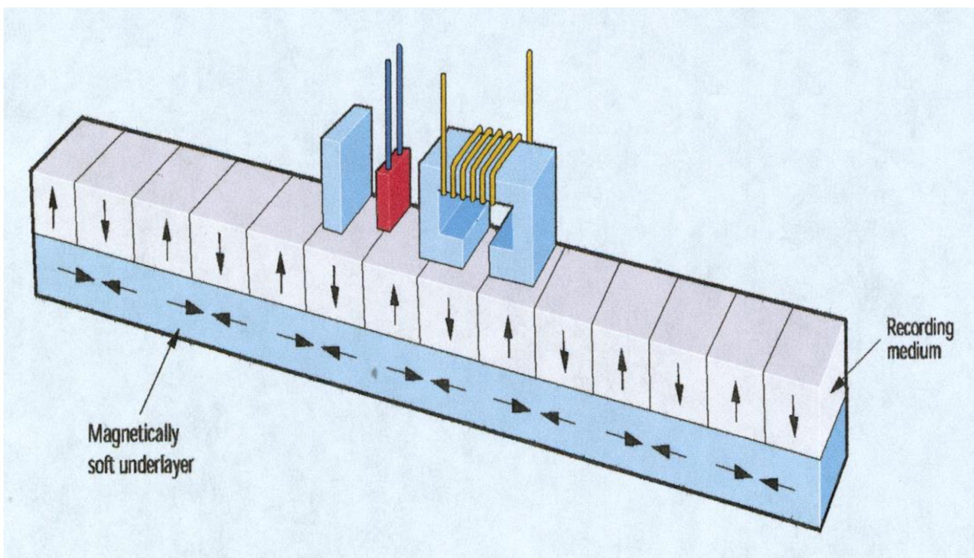
Perpendicular Media

Change to a new recording paradigm, thirty years in the making

The development of perpendicular recording and how it became the dominant form of magnetic recording for HDDs is an interesting story in its own right. As mentioned in Chapter 5, Professor Iwasaki at Tohoku University in Japan first proposed it in 1977 and caused quite a stir. Before I started Komag, there were already several start-ups in the U.S. attempting to use a perpendicular approach for high-density recording. Japan also invested heavily into research and development of products that could use this concept for many years. Combined together, well over a billion dollars in investment was probably poured into this effort, but by around 2000, it practically ceased to exist. The only place the technology was used might have been in tapes and in very limited use at that. It was a complete bust.

Conceptually, perpendicular recording is very elegant and compelling. One of the biggest factors that limit recording density for longitudinal recording is the demagnetization effect due to two magnetic poles that oppose each other. Perpendicular recording eliminates this issue altogether by arranging the poles to support the magnetization transitions rather than to oppose them.

The original and most popular manifestation of this idea was in using a CoCr magnetic layer, and later by incorporating a soft underlayer to keeper the magnetic recording layer with the image field of the head. There were significant efforts in both the U.S. and in Japan to commercialize the concept, but for various reasons it never happened until recently. Perhaps part of the reason for it having taken so long to be adopted was because of the success and extendibility of longitudinal recording. Oxide media were able to stretch into overlapping the introduction of thin film media and, reluctance by IBM to adopt thin film technology due to the fact that they had thin film heads. They were able to continue increasing the areal density even with oxide media well into late 1980s. The last major large size disk drive from IBM, the IBM 3390-2 introduced in 1989 had an areal density of 62.6 Mb/in^2. This was much higher than anyone could have imagined for oxide media, certainly in 1983 when I started Komag. The introduction of thin film media was also not very easy, with plating and sputtering competing for a while until sputtering, by 1989, became pre-eminent. Extending existing technology is quite powerful and not to be overlooked or underestimated. At that time, small form factor disk drives enjoyed spectacular success with the rise of the PC and using thin film disks. MR and GMR heads also dramatically raised the areal density, so much so that it drove many companies out of business. The introduction of AFC (Anti Ferromagnetic Coupled) media extended longitudinal recording by at least another two years. Therefore, it could be argued that PMR (Perpendicular Magnetic Recording) technology was not needed or that the industry wasn't ready to tackle it until it ran out of ideas that could extend LMR (Longitudinal Magnetic Recording) technology as far as it could.



Schematic representation of a perpendicular magnetic recording arrangement (Figure 12-1).

It could also be said that the R&D activities for perpendicular media never completely disappeared. There were some low level activities taking place in large organizations. Also, both in the U.S. and in Japan, Japanese storage companies formed consortia that funded university research into magnetic recording technology, and perpendicular recording always had some level of interest. The money being funneled into universities was never very much but at least it had the effect of periodically bringing industry and research people together a few times a year for discussion. The U.S. group was called NSIC (National Storage Industry Consortium) and the Japanese counterpart was called

SRC (Storage Research Consortium). One of the key exercises that the consortia did every year was to come up with a roadmap for areal density increases and what technology could be considered for achieving that recording density. I believe it might have been around 1997 or so that a small subgroup working on the roadmap at the NSIC meeting came back to report to the entire group, that perpendicular recording would be the technology that could achieve the next generation of areal density increase. The biggest issue that everyone was starting to see was that the thermal stability of the LMR media was going to be a major issue. As the grain size was being reduced, the problem was getting worse. The drives had to resort to re-writing data (refresh) in order to make sure that the data did not evaporate. Going to perpendicular recording was one way to alleviate this issue, since the volume of the media would be increased in perpendicular recording, and higher coercivity media could be used as it would be easier to write them in the perpendicular recording configuration than in longitudinal recording. Nevertheless, the recommendation for considering perpendicular recording was still surprising, as most people considered it long dead. Not everyone wanted to hear this as many people had tried and got burned in the past by invoking perpendicular recording. Remarkably, a majority of the members came out thinking that this was a viable path to consider. We cannot be certain that this was the actual seminal event that leads the industry to reconsider perpendicular recording, but the NSIC meeting perhaps strongly influenced everyone's thinking.

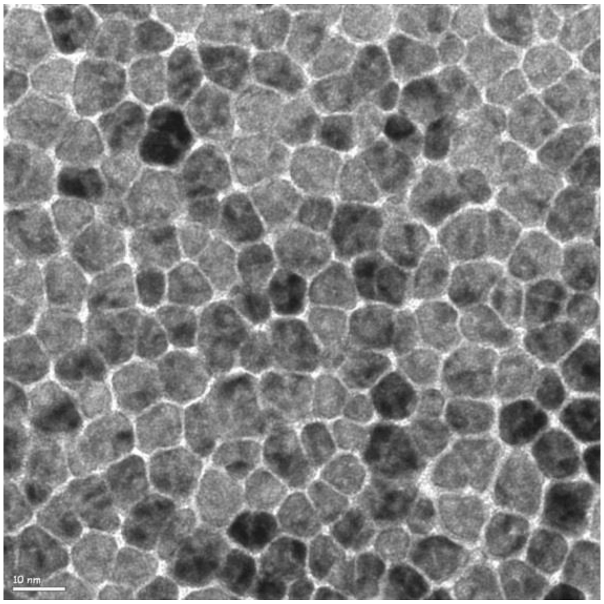
In the 2004 to 2005 timeframe, there were very active and concerted efforts taking place in media companies and by sputtering equipment manufacturers to develop a perpendicular recording media and process. The race was really starting to heat up. Significant areal density demonstrations of 400 Gb/in^2 were reported by 2006. When perpendicular recording was resurrected, there were several competing approaches. First was a CoPt or CoPd multi-layer approach where one could obtain very high perpendicular anisotropy by stacking multiple layers of Co with Pt. The advantage was that one obtained very high Hc, and a square hysteresis loop, which was recognized as being critical for a good signal-to-noise ratio. Earlier PMR media using CoCr had a sheared hysteresis loop with positive nucleation fields, which turns out to cause severe noise and thermal decay problems. A high negative Hn, or reverse nucleation field, was essential. The disadvantage with CoPt or CoPd multilayers is that this would be difficult to construct using conventional sputtering tools. However, companies such as Anelva valiantly constructed working cathode assemblies that could produce multi-layer stacks in an existing tool. The second problem with multi-layer construction was that there was no easy way to isolate the grains. The very act of trying to isolate the grains led to breakdown of the multi-layer structure. The process required relatively high temperatures as well. The second approach was to try to use the existing LMR media structure to grow perpendicular media. Most magnetic media alloys have a natural tendency to grow with perpendicular crystallographic orientation. It seemed that it should be possible to try to enhance it, by changing the Cr underlayer with some other material that would naturally tend to orient the film perpendicularly instead of horizontally like the Cr. One could imagine using a hexagonal crystal structure seed such as Ti for example. Considerable work had been done over 20 years ago with underlayers to orient the CoCr magnetic film that one could draw upon. I believe that most media companies attempted this path, as it was the most convenient approach, but all ran into difficulties. The main issue was that Hn, the reverse nucleation field, could not be raised to a high enough level. Hence, the noise performance was quite poor. There were other ideas or hybrid structures that combined the two, called CGC (coupled granular/continuous) media (Reference 12-1) that had the elements of both. This process still required the same complicated deposition cathode to lay down the multi-layer film so that it did not seem to hold much advantage either.

One of the key breakthroughs in PMR media came from Toshiba Labs in 2000 (Reference 12-2). They combined certain elements of existing LMR technology with some unique added enhancements. They used a pure Ruthenium (Ru) seed layer to set up the perpendicular growth of the magnetic layer, followed by a magnetic layer containing a heavy doping of oxide. A fairly thick layer of up to 100 nm of Ru was required but it appeared that the oxide magnetic phase grew beautifully on Ru with good perpendicular orientation. The critical benefit was that the reverse nucleation field Hn was quite high with this arrangement. The rest was a matter of bringing everything else together. A fairly thick magnetic soft underlayer was needed as keeper for the recording. The magnetic soft underlayers had to be quite thick so that in order to efficiently sputter these materials, one had to develop a specially constructed cathode with an extremely high field to penetrate the high Ms, soft magnetic alloys. The high thickness requirement was a big problem also. Because many stations would be required, it became more advantageous to have a system such as the Anelva sputter tool that allowed additional chambers to be added so that PMR media could be made. Intevac also by then had a modular system that could add many new stations suitable for PMR media fabrication. Toshiba was the first to announce an actual drive in May 2005 using the perpendicular recording technology, and it was in a 1.8-inch form factor drive. The areal density was not particularly high compared to the longitudinal drive available at the time, but it set the tone for the future. It had a capacity of 40 GB using single platters for an areal density of 133 Gb/in^2.

The use of an oxide doped magnetic layer was something that we used for our isotropic longitudinal media. Toshiba at various times in our history was a Komag or AKCL customer; therefore they were also familiar with what we did for the magnetic alloy design. In fact, in one meeting that Komag had with Toshiba in Japan that included Yoichiro Tanaka, who was for a long time a key technical leader at Toshiba, and Komag personnel that included Gerardo Bertero, David Wachenschwanz and Tom Yamashita. Toshiba gave credit to Komag and AKCL for developing the magnetic media with oxide doping and serving as a template and inspiration for Toshiba's perpendicular media design. Komag initially started to sample to customers perpendicular media based on a conventional LMR alloy and also using the oxide alloy. Oxide based perpendicular media was superior in having a high reverse nucleation field, Hn, and better noise performance. Our customers on the other hand were initially headed in very different directions. HGST was focused on a CoPt and CoPd based multilayer structure and invested in very expensive hardware with Anelva to achieve such structures. Seagate was focused on a FePt $L1_0$ structure alloy, which required very high processing temperatures and invoked ultrahigh temperature substrates such as silicon and as of yet experimental high temperature glass substrates. They went so far as to declare aluminum/NiP substrates dead, and that they would no longer work because they would not be able to tolerate the high temperature processing. With Toshiba's PMR drive introduction and also Komag's sampling of oxide PMR media, by 2004 to 2005, everyone had adopted the oxide based PMR media.

As various media organizations in Japan and the U.S. attempted to ramp PMR media using oxide doping, one of the biggest problems became the fabrication of oxide targets. Due to the fact that everyone, including ourselves, had switched to oriented media design with Cr metal based segregation, all the target manufacturing had switched to a vacuum induction melting (VIM) process in order to manufacture the magnetic targets. Even though the composition was quite complex, the VIM method somehow kept pace. This was despite the fact that the target manufacturing process required rolling, and the target often cracked. With oxide addition, there was no way to use VIM anymore, and target manufacturing had to revert to powder metallurgy and hot press. Companies such as Leybold had given up on the business of making targets using hot isostatic press (HIP) and had left the business. Luckily, many of the Japanese target vendors had been developing new hot press methods for consolidating the powder, which still achieved very high theoretical density. They also worked out the complex powder metallurgy method for making the powder for optimum microstructure in the target. This process was quite important because with the static sputter system, the preferred deposition method is DC magnetron and the oxide phase has to be very small and evenly distributed over the entire target so as to prevent spitting and defect generation. At Komag, we did attempt to return to RF sputtering in the Anelva static tool, but we still had a considerable amount of defect generation and had to revert back to DC magnetron. This situation caused a severe premium to be put on target manufacturing and there was some shakeout of sorts in this industry as to who could make the targets and who could not.

An interesting side note on the path to perpendicular recording was the use of ruthenium (Ru) metal. As is sometimes the case with high tech products, ruthenium seems to be a unique metal, best at orienting the magnetic layer. As mentioned, the initial required thickness for ruthenium film to achieve the good orientation in the cobalt-platinum oxide alloy film was up to 100 nm. This is quite thick, and due to relatively poor target utilization of DC magnetron sputtering, everyone started to buy considerable quantities of ruthenium targets.



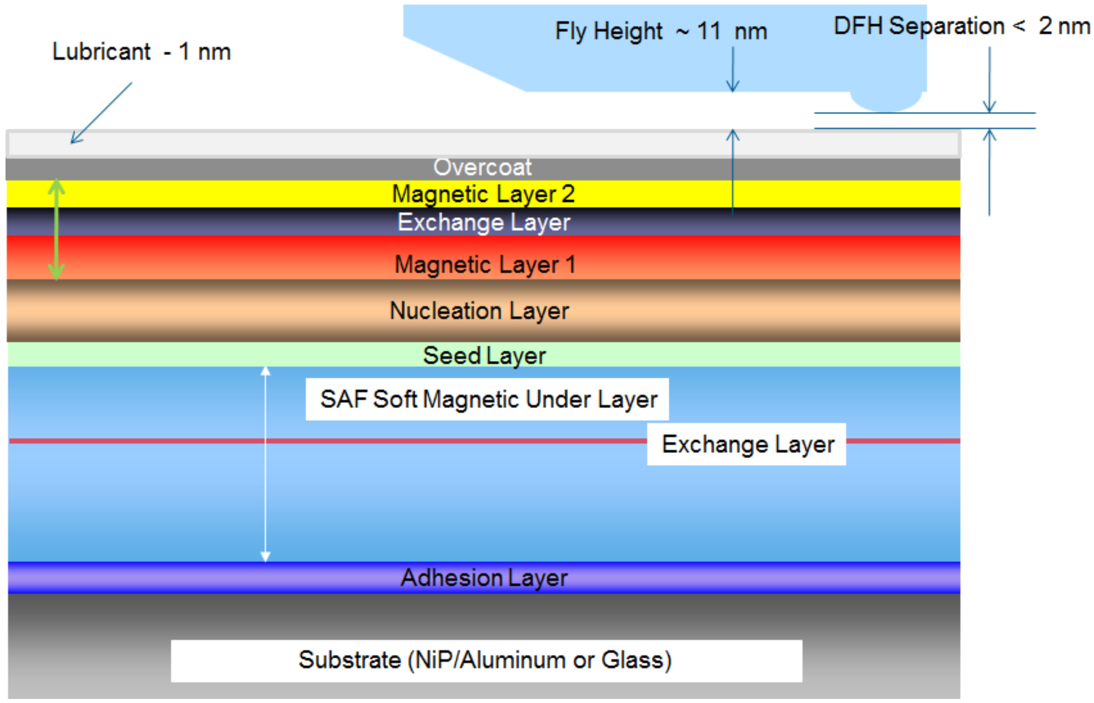
TEM micrograph of the PMR magnetic layer. Alloy consists of Co, Pt, Cr, and variety of oxides of Co, Cr, and Ti. The white area between the grains is an oxide phase that separates the grains. Each grain is about 7-8 nm diameter. This type of film supports areal density of ~ 400 Gb/in^2 density.

(Figure 12-2)

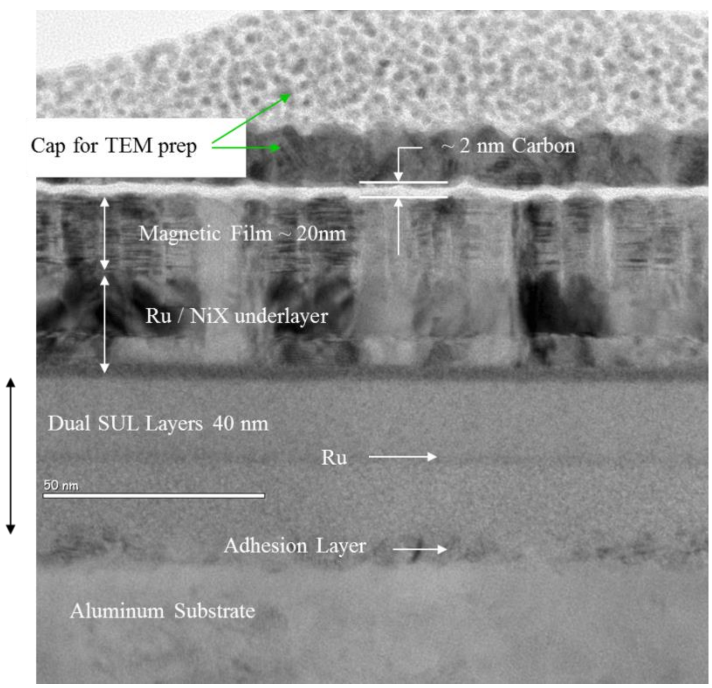
Ruthenium is one of the rare precious metals in the same category as platinum and palladium in terms of rarity, but due to the fact that its industrial use was quite limited, the price was only around $80/oz. Compare this to platinum, which at the time cost around $1,200/oz. No one really thought much about having an issue with this metal. However, it turns out that annual production of this metal is only about 20 ton/year. This is about a cubic meter of the stuff per year. Because the production is only a by-product of platinum production, the quantity available was very limited, and there was no stockpile of this material either. As we started to buy up ruthenium targets, to our shock the price started to zoom up. Soon some speculators got into the market and made things even worse. It hit a price of $900/oz. by 2007 and we faced a very dire situation. Soon, we had to layout upwards of $20M in cash just to secure enough material to keep the factory running. It seems that just overnight, we had become the major consumer of ruthenium metal in the world. Everyone then had to go into a crash program for reducing the usage of ruthenium metal, by reducing the thickness required for the stack. Eventually the amount has been reduced to about a third of where it was originally, and the industry has become a bit smarter about how it deals with the market for ruthenium. Some recycling methods have also been developed by the metal industry and are starting to be applied. Today the price of ruthenium is back down to around $200/oz. It was a hard but interesting lesson in how we choose the material to use in the media. One has to be aware of availability and consequences that our choices have on the materials market.

Changing to PMR media also was quite gut-wrenching for the media manufacturers. Thick soft underlayers were required and this meant that new cathodes capable of sputtering this material had to be created and installed. Additional chambers were needed for the sputtering system, and major upgrades had to be done to the sputtering system and the facility that housed it. Considerable capital outlays were required. The old in-line ULVACs were finally no longer capable of producing the PMR media and had to be retired. Seagate faced similar issues with the old MINT tool that was originally designed at Domain Technologies. They too were also retired finally after many years of service. Some of the older generations of Intevac 250B as well as Balzers Circulus tools with limited number of chambers continue to produce some lower capacity PMR media, but their days are numbered and they are in the process of being retired. The industry is dominated by Anelva and Intevac Lean systems for making the latest generation of PMR media, as these tools have the flexibility to add new chambers as needed. These tools have anywhere from 20 to 24 separate chambers for processing the media. This is a far cry from the days when we had total of 4 cathode stations in the original ULVAC S1 and S2 sputter systems in Komag.

Left, a photograph of modern Anelva 3040 sputter system at the time of installation ceremony at Komag and at right, an Intevac Lean System (courtesy of Intevac Inc.). (Figure 12-3)



Construction of PMR media (in 2011) showing the complexity and the number of layers that are used. This type of construction was used for media up to around 400 Gb/in^2 areal density. (Figure 12-4)



Cross-section TEM micrograph of typical PMR media used in the industry, showing various layers. (Figure 12-5)

Within about four years of introduction then, the entire world production went from LMR to PMR, and today 100% of drives being produced are using PMR technology. In 2010, a conference was held in Sendai, Japan for the PMRC or Perpendicular Magnetic Recording Conference. It was the ninth conference held on this topic, and it was in large measure used to honor Professor Iwasaki for his work done in 1977. He was also awarded the 2010 Japan Prize, which is given for the highest contributions to science and technology. The award is the most prestigious science award given in Japan, and the Emperor of Japan himself bestows the award every year. This was quite a turnaround for something that was really given up for dead only ten years ago. To be fair however, the construction of perpendicular media today is considerably different than what it was when it was first introduced. Much of that knowledge was gained only recently as the media industry re-tackled the problem in much more earnest. Today the highest recording densities being achieved using perpendicular recording stand at around 1 Tb/in^2, and it achieves that density in combination with the latest in TMR (Tunnel Magneto Resistance) head technology. The write and read elements are very close to the disk surface at around 2 nm. Carbon overcoat is still being used but is considerably better than when we started. It is also less than 2nm thick (about 10 to 15 atoms thick). The disk continues to use a polar PTPF based lubricant but with considerable design enhancements since the days of z-dol.

The ability to squeeze more performance out of PMR media is becoming quite difficult now. Areal density increases have slowed down considerably. Something new has to come along soon, but as of this writing, it is not so clear yet what that would be. Companies are working on heat-assisted recording and bit patterned media, but turning them into actual products still seems very difficult indeed.

(Reference 12-1)
"Coupled granular/continuous medium for thermally stable perpendicular magnetic recording"
Y. Sonobe, D. Weller, Y. Ikeda, K. Takano, M.E. Schabes, G. Zeltzer, H. Do, B.K Yen, and M.E. Best.
Journal of Magnetism and Magnetic Materials, Vol. 235 Issues 1-3, pp. 424-428, (October 2001)

(Reference 12-2)
"A new design to suppress recording demagnetization for perpendicular recording"
A. Takeo, S. Oikawa, T. Hikosaka, and Y. Tanaka
IEEE Transactions on Magnetics
Vol. 36, No. 5, pp. 2378-2380, Sept. 2000

Chapter 13

Final Thoughts

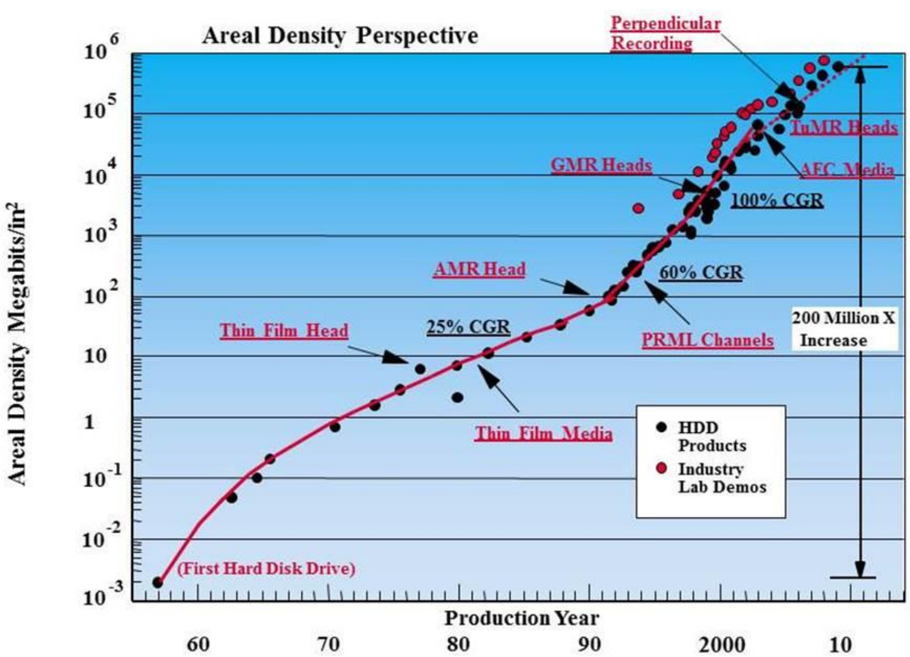
I began working on magnetic materials in 1957 and on magnetic recording materials starting in 1967 at Northrop, and kept at it till August, 1999, when I retired from Komag. People that I worked with and trained continued the work that I started and they continued to advance thin film media technology, including the transition to perpendicular magnetic recording. In late 2000, after I had left, Komag merged with HMT Technology, who was our main rival in the business at the time. As I discussed in Chapter 11, both Steve Johnson and I opposed it when it was proposed to us first. As background to this story, Komag's $202 M bank loan was in violation of the loan covenant at the time. In theory, the bank could have called in the loan at any time. If they did, it would have forced us into Chapter 11 immediately and the bank would not have been able to collect the money. In 2000, the banks seemed to be content to keep collecting interest on the loan, and perhaps wait and see what might happen to us. Both Steve and I thought that we could wait out HMT, and hopefully the media market would recover in the meantime so that we could refinance the loan with one less competitor pushing the margins down. With this logic, we had rejected HMT's proposal for a merger. Therefore, it was with considerable dismay that we received the news that T.H. Tan went ahead with the HMT merger shortly after we had left Komag. It was like hearing the death sentence for Komag, and surely as we had expected, Komag went into Chapter 11 in August of 2002. I do not know all that transpired with the HMT negotiation after we left, but to this day, I still wonder what might have happened if the merger had not happened. I still believe that it was a mistake to merge with HMT.

As it is often the case with companies going into dire financial situations, the vultures start to circle the dying. Loans and bonds that were held by Komag and HMT's bond holders and banks were bought on the cheap by Cerberus Capital Management before 2002. Cerberus Capital in effect took Komag through Chapter 11, put in their people on the board and recapitalized the company. To be fair, companies like Cerberus Capital and Bain Capital that were being talked about with Mitt Romney serve a function in business to turnaround distressed companies. They will force companies to take actions that perhaps they would not take on their own, or lack resources or have the stomach to do so. In late 2002, Komag emerged out of bankruptcy and was relisted in Nasdaq in 2003. T.H. Tan led the company through this tough period and rebuilt the company to health. Business continued to grow afterwards but the company could not fight the forces of consolidation that were continuing to take place in the disk drive business. I think T.H. Tan fought it as long as he could, but at the end, he was replaced by Tim Harris in 2006, and within a year, Komag was sold to Western Digital in 2007 for about $1.2B, having reached annual sales of around $940M. Through this process, Cerberus Capital and their investors made a handsome return for their efforts. Komag people that suffered through the entire process and hanged on did quite well as well, but many people were let go in the process of merging with HMT, again through Chapter 11 and through the subsequent purchase by Western Digital. The human cost of the process should not be overlooked. Hundreds of millions of dollars were also lost by investors that owned shares of Komag and HMT, as well as by banks and their bond investors. I was also an investor in Komag and lost a substantial amount of money when Komag shares lost all of their value. I suppose that it was not unlike the situation with Headway which went through two rounds of new financing to stay alive. Only the last group of investors made any money.

The Komag media operation continues to survive today as Western Digital's primary media operation, along with HGST's, which was also bought out by Western Digital in 2011. HGST is what remains of the original IBM HDD operation as it was bought out by Hitachi in 2002 and renamed Hitachi Global Storage Technology (HGST). The Western Digital media operation is one of five media operations that still survive today, which are Western Digital, Seagate, HGST, Showa Denko and Fuji Electric. When I started Komag with Jim Shir and Scott Chen in 1983 there were perhaps 30 to 40 separate media operations, some internal to large companies and many start-ups. It is with some pride and consolation that the Komag media operation still survives as one of five remaining, even though the name has changed.

So, looking back now to 1983 when IBM owned perhaps 80-90% of the HDD business and it was one of the most profitable business within IBM, the world has completely changed in a direction and manner that no one could have imagined back then. Who would have thought that Seagate, which was only a small start-up selling their drives for PC-AT computers to IBM, and Western Digital which only started to produce HDDs in 1988 with the acquisition of Tandon, would together come to own over 80% of the HDD storage business. The technological advances made in the HDD are also remarkable and could not have been imagined. But it was due to many people working diligently on problems and coming up with solutions that led to this outcome. Solving any problem requires a systematic approach based on sound scientific principles and knowledge. I hope that I have provided plenty of examples of this in the way we have solved the many difficult problems that we faced. Of course, some luck and fortuitous breaks were also involved, but perseverance and having total focus on problem solving are good traits to have. I would hope that such an approach was instilled on the many people who have come to work for me. These people are now spread out in many different companies and organizations. Taken all together, I have no doubt that much hard work and focus have led to great and amazing results, which we are seeing with HDD technology and the magnetic media that it uses.

It may be useful now to review the areal density growth in HDD since the invention of the first disk drive, the IBM RAMAC 305 in 1956. Comparing the history now with the projection that we had in 1980 shown in figure 4-12 (page 79), it can be seen that compound growth rate (CGR) projection badly underestimated the growth rate provided by the introduction of thin film media. The actual areal density grew at 25% CGR for about 12 years. CGR accelerated to 60% and to 100% with the introduction of MR heads and GMR heads. Since 2004, the CGR has slowed down to around 45% but pushed along by the introduction of perpendicular media. Today, this technology has matured and there is no new big technology breakthrough to take its place. Current CGR is probably under 20%. In the 1980 prediction, optical recording and vertical (perpendicular) recording were thought to be the next new technologies that would raise the CGR. Optical recording did not replace the HDD, and perpendicular recording prediction was off by 20 years. However, predicting the future of HDD areal density curve remains just as difficult today as it was in 1980.



Areal density trend since the invention of HDD. Chart was created in 2010. (Figure 13-1)

At the moment, is not clear at all what new technology will be used to continue the increase in areal density. There are near term options such as shingled magnetic recording which attempts to closely overlay recorded tracks to increase track density with some overhead in how the data is written and accessed. This technique can only work on certain types of drive applications; therefore it may not contribute so much to the CGR growth. Leading candidates for more radical technology change are two-dimensional magnetic recording (TDMR), patterned media recording and heat assisted magnetic recording (HAMR). All of these technologies have considerable problems and appears to be far from being manufacturable yet, let alone being able to even demonstrate their capability. Only time will tell which of these paths will lead to next spurt in CGR.

The technologists that were the experts, including myself in the 1980s, were not particularly accurate or insightful in predicting the state of affairs today in magnetic recording. Nor were the business people that ran the companies any better in predicting the future. Xerox, IBM, Hitachi, Fujitsu, who were all forces to be reckoned with 30 years ago, are all but gone from the storage business, and even from computing hardware business. I believe that we are no clearer in being able to predict the future today than we were 30 years ago. But I do believe that magnetic recording will carry on in one form or another much longer than we can imagine today. No doubt there are some bright and young innovators that are preparing to pave the way to the future now, that are still relatively unknown. I wish you good luck with all my blessings, whoever you might be.

Note from Tom Yamashita

I was very fortunate in having met Tu Chen for the first time in 1979 at Xerox Palo Alto Research Center. I had visited Xerox PARC earlier, tagging along with a post-doc that worked with my advisor, Professor Robert Sinclair at Stanford. This post doc was Birgit Jacobson, and she had worked for Stig Hagström who was then PARC's General Sciences Lab director. Stig Hagström is credited with having developed the x-ray photoelectron spectroscopy technique (XPS) while he was a graduate student working for Kai Siegbahn in Sweden, who won the Nobel Prize for this technique. He also helped to found Linköping University in Sweden as professor and Vice Chancellor, and he was Birgit's Ph.D. advisor. Because of this relationship, Birgit had excellent access to PARC's facilities, and I would accompany her when she visited the PARC facility to do her work. Stig Hagström would become chair of Stanford's materials science department from 1986 for many years, and he held several prominent positions in Swedish academia thereafter, including serving on the Nobel Award selection committee. There were many relationships such as this between Stanford University and several companies located inside the Stanford Industrial Park. I believe that I benefited greatly by such relationships, as did many other Stanford students that went on to work in Silicon Valley. About a year later, I decided to continue my education at Stanford to pursue a graduate degree in materials science, and my advisor assigned me to a project to work with Tu Chen on magnetic thin films. Again, this was a project that was sponsored by Stig Hagström and the materials science department.

My first meeting with Tu Chen was quite interesting. He had a beautiful office that overlooked the outside through a large window, where you can still see scenic pastures behind Stanford University. One could see horses grazing in the field out of the window. PARC itself was an incredible place. I saw the Alto computers that the secretaries used, and it was something that was just truly amazing. I never saw anything like it before. Tu Chen was busy doing some calculations on a piece of paper at the time, and he was muttering his multiplication table aloud, and he was using the Japanese mnemonic for multiplication. This was quite peculiar as I didn't know that he was from Taiwan or that he had received a Japanese education in Taiwan. My first thought was, who is this guy and why is he using the Japanese mnemonic for the multiplication table. I had immigrated to the U.S. when I was 10 years old with my parents, and my multiplication table is also in Japanese mnemonic to this day. From Tu Chen, I learned about magnetic thin film technology and the measurements that are used in the field. As I pursued my graduate degree, I decided about a year later that it would not be a good idea to be tied to an industry project for my thesis work because of concerns that I had about possible restriction on publications and disclosure, so I switched my focus to working on solar cell materials and on transmission electron microscopy. Tu Chen continued to call me from time to time however, asking me to do some measurements or take a look at his films. We also went fishing a few times as well, which both of us have some passion for. When Tu Chen started Komag in 1983, I was getting ready to finish up my Ph.D. thesis and I had started to write it. I also interviewed with several companies and had an offer from HP Labs in Palo Alto, and also from Bell Labs at Holmdel, New Jersey. Both were very nice offers, with a starting salary of around $45,000. By late 1983, Tu Chen had set up shop at a very small warehouse location in Milpitas and asked whether I might be interested in joining him at the start-up. I paid a visit and talked for a while with a few people that had already joined. I thought about this for a while, and decided that it would be quite exciting and interesting to join. I think the key reason was that the job offers from HP and Bell Labs involved mainly materials characterization and transmission electron microscopy, which I had developed considerable skill and reputation for already. However, I was not so sure that I wanted to make materials characterization my main life's work. I really wanted to participate in developing something that would be sold and would enjoy wide spread use. I still did not know all that much about magnetic recording and thin film technology at the time, and I frankly did not know what Tu Chen would have me do at the start-up. I do not think he knew either, but he knew enough about me so that he was willing to offer me a job and put me to task on whatever that was needed to be worked on.

Upon hearing about the job offers from Bell Labs and HP Labs that I already had, Tu Chen said that he could not possibly offer anything like that sort of salary. His offer turned out to be $32,000/year, with no long-term benefits such as pensions that both HP and Bell Labs offered at the time. Of course, it was high risk as well, but I decided to take it anyways. Even Tu Chen told me that I was foolish and crazy to accept his job offer, as did many of my friends. It seemed that the situation at the new Komag demanded that I start right away, and I wanted to come in and start to contribute from the ground up. I joined in April of 1984, and I was the 12th employee of Komag. They had just moved to the new Milpitas building on Yosemite Drive, and they were in the middle of developing all the new tools and processes, and quickly realizing that there were many problems. The big mistake I made was that I had not finished writing my thesis yet, and left Stanford to join Komag. It turned out that I would never complete my Ph.D. The initial few years of Komag were full of crisis as well as much excitement. I was able to make significant contributions to the design of media, and also to the company's success. I learned a lot about running the business, and how a company like Komag can start out so small and grow to be a large company. It has been an interesting experience, which I would probably never have gotten if I had gone to HP Labs or to Bell Labs. Therefore, I have no regrets. The single biggest thing in my career was the relationship that I was able to develop with Tu Chen as my mentor and partner in development of technology. I do not think I have met someone quite as dynamic and full of energy and passion as Tu Chen, and his total commitment to a task at hand. His passion got the better of him at times, but I always knew that he cared about everyone. We always managed to work very well together and went on to solve some very difficult problems along the way. It is with some pride that I can contribute to helping him write this story. As Tu Chen is proud of his accomplishments in the development of sputtered thin film media and starting up Komag, I am also proud of my own contributions to the materials and processes used to make thin film media. In 2007, we reached a milestone of sorts at Komag when we marked the sale of one-billion thin film media sold by Komag since starting in 1984. The old Komag still survives today as the media operation for Western Digital Corporation and it is continuing to produce well over 200 million sputtered thin film disks a year, which are used in Western Digital's disk drives. They continue to use many of the processes that I had a major part in developing to make the disks. My objective of being able to contribute to something that people will actually use has been accomplished with great success, all thanks to that fortuitous meeting with Tu Chen at Xerox PARC so many years ago.

Tom Yamashita
August 2013

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