Advanced Materials R & D— Planning for the Future

Editorial Essay

By James Economy*

1. Introduction

One of the major questions confronting materials scientists concerns their future role with respect to the emerging technologies of the 21st century. Today, we hear of many activities among the various nations of the world directed at achieving leadership in future industrial technologies through major commitments in advanced materials R & D. In Japan, seven specific areas in materials R & D were identified by the Ministry of International Trade and Industry (MITI) in the early 1980's and large joint industrial programs were established to carry out leading edge research in these areas.[1] In the USA, emphasis is being placed on more effectively coupling the materials research efforts in the universities and the national laboratories to industry. In Europe a number of activities can be identified; e.g., in Germany the government is beginning to foster closer interactions between the large chemical giants and the Max Planck Institute for Polymer Research in Mainz, while in France close collaborations between CNRS and industry are in place. As a backdrop, a number of the global technology companies have been acquiring key materials industries such as composites fabricators or advanced metal alloys manufacturers with the intent of better participating in these obvious growth areas.

One might ask which of these approaches if any provides a reasonable probability of fulfilling the hopes and anticipations of the countries or organizations who have committed significant resources to these activities? Stated somewhat differently, can one predict future needs with sufficient accuracy to lay out and justify an advanced materials program with the necessary effort and continuity to address the future technologies of the 21st century? Clearly the track record of industry has not been very good in the past two decades in developing new materials let alone in addressing the short-term needs for those industries which are dependent on advanced materials for their new product lines. Part of the problem lies in the fact that the chemical and metal companies who traditionally have provided most of the initiative to new materials development have dramatically changed their posture in the past twenty years. Thus, most of the metal industries have either sharply reduced or eliminated their R & D and that which remains tends to focus on very near term goals. Much of the chemical industry has emphasized forward integration into high value added markets where their skills in materials may provide significant leverage. On the other hand, many of the major industries such as electronics, aerospace, automotive etc. who have critical needs for new or improved materials for the most part still depend on the traditional materials suppliers who are actively pursuing the above indicated goals. Unfortunately, many of the R & D managers in the materials dependent industries are trained more in the needs and opportunities of the particular industry rather than in the synthesis and development of the advanced materials upon which the future of their industry may depend. Hence, programs on new materials development may not be as readily supported in such environments. Other factors that may be cited which work against the successful pursuit of advanced matrials R & D in the USA include the preoccupation of high level management with potential takeovers and the demands on R & D management to demonstrate an early return on investment in materials research.

In this paper, I first look at the field of advanced materials from a historical perspective and then examine closely two industrial R & D efforts with which I was directly involved. In the first case, the research was directed at development of new materials for diversification of the company into new areas, while in the second example, work on new materials was aimed at meeting the internal

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needs of the corporation. These two approaches are discussed in terms of their respective risks and payoff and then a number of generalizations are drawn concerning future directions for advanced materials research.

2. Historical Perspective (early 1900's–1960)

In spite of the current enthusiasm for R&D on advanced materials, a strong perception exists among many industrial leaders that the field is mature and that there are very few new materials with significant commercial value remaining to be discovered. In addition, recent experiences with the time required for development of new materials suggest that it takes far too long and is too risky bringing a new material to the marked place. Although these views are undoubtedly correct for a number of materials categories, the danger lies in the tendency to generalize them to the whole field of advanced materials. In this section, I would like to explore some of the sources for these perceptions and also describe why industrial scientists of an earlier era were indeed more successful in the commercial development of advanced materials.

When one examines the development of new materials prior to 1960, one can identify certain distinct patterns to these discoveries. Thus in the early 1930's when *Carothers* and his team were laying the foundation for the field of condensation polymers, they came up with the unexpected discovery that melts of aliphatic polyesters could be melt-drawn into filaments and then cold-drawn to achieve much higher strengths. The implications were undoubtedly obvious to the researchers and especially management that they had an alternative to silk and within several years a material (Nylon) had been selected, optimized and scaled-up to a semi-works facility. This also was the first example of melt-spun filaments, and required significant innovation in the design of the spinning system.

In an earlier era, *Acheson*'s work on the reaction of SiO_2 with carbon may have yielded the anticipated SiC, however what was unexpected was the very high hardness of the material. Its potential use as an abrasive must have been immediately obvious and within a short period of time, *Acheson* proceeded to scale up the reaction for commercial development.

In the early 1950's, the discovery of linear polyethylene by several groups of workers in different laboratories appears again to have been fortitous, but at least two of the groups recognized the significance almost immediately and within five years one of them, Phillips Petroleum, had put a 50 million pounds per year facility on stream. In fact, Phillips sold the know-how for such a plant to a number of other companies and undoubtedly quickly recouped its initial investment. Certainly, the earlier development of high pressure, branched polyethylene permitted easy identification of markets for the linear polyethylene with its greatly improved thermal resistance and stiffness.

These three examples, although taken from quite different periods of time, have a number of points in common which appear to characterize many of the major developments in commodity materials such as polymers (see Table 1 for a listing of the major commodity polymers). In each of these cases, there were readily identifiable markets which could be addressed almost immediately. Potential existed for low cost routes to the product and high level management had the technical background to commit resources for rapid scale-up in a timely way. To a considerable extent, one can understand why investment in industrial R&D during the period of 1930 to 1960 proved to be highly profitable. However, by 1960, it was becoming clear that it was unlikely that there would be any new commodity polymers and greater attention was focussed on specialty polymers. In fact from our vantage point today, the commodity polymers listed in Table 1 can only be considered as a mature field since the last one was introduced over 30 years ago. It is noteworthy that a significant industrial R&D effort continues on these polymers but it is directed primarily at improvements in the product and/or process. This kind of incremental research appears to have had significant payoffs in recent years — witness the recent progress in the manufacture of polyolefins.

Dr. Economy received his BS from Wayne State University (1950), his PhD from the University of Maryland under Professor W. J. Bailey (1954) and was a research associate at the University of Illinois (1954–1956) with Professor C. S. Marvel. He worked at Allied Chemical as a general research leader on polyolefin research from 1956–1960. From 1960–1975 he was Manager of Chemistry and of the Research Branch in the R & D Division of the Carborundum Co. From 1975–1989, he was Manager of the Polymer Science and Technology Department in the Research Division of IBM. Just now, in February 1989, he joins the faculty of the University of Illinois as Professor and Head of the Materials Science and Engineering Department. He has carried out research on high temperature polymers, flame resistant materials, high performance composites and advanced materials for microelectronic devices. He has received a number of distinguished awards including the AIC Chemical Pioneer Award (1987), ACS Phillips Medal (1985), the Southern Research Burn Institute Award (1976), Schoelkopf Medal (1972), and 14 IR 100 Awards for Outstanding Technical Developments in American Industry. He has published over 150 technical papers and received more than 70 US patents. He has served on numerous government committees and was chairman of the Polymer Division of the American Chemical Society (1985). He is a member of the National Academy of Engineering, the National Materials Advisory Board (since 1984) and is the USA representative to IUPAC's Macromolecular Division (since 1985).

Table 1. Commodity polymers.

Nylon
Polyester (PET)
Polyacrylates
Low Density Polyethylene
High Density Polyethylene
Polypropylene
Polystyrene
Poly(vinylchloride) (PVC)
Acrylonitrile-Butadiene-Styrene Copolymer (ABS)
Phenolic Resins
Urea

During the late 1950's the US Government began to pursue a more aggressive role in stimulating R&D on advanced materials to fill specific needs such as advanced military hardware, aerospace applications and nuclear reactors. Incentives in the form of large government grants were made available to encourage industry and university scientists to pursue these goals.

Another phenomenon which could be traced back to that particular era was the desire of many companies to diversify from their traditional businesses into areas with high growth opportunities such as plastics. If done properly, this kind of diversification could be extremely effective at insuring the long term stability of a company. However, the chief executive officer (CEO) often lacked the technical expertise or intuitive skills necessary to manage these new businesses and in such an environment high level decision making could lead to disastrous consequences or at least to delays on critical decisions. In the late 1960's, this problem would be further exacerbated by a new generation of CEO's drawn primarily from the financial ranks.

Undoubtedly the most dramatic change during this period was the dominating role assumed by the US government in defining its advanced materials needs. From an industrial viewpoint it was easy to focus on these government needs with the expectation of financial support by the government followed by the potential for a wider commercial development. As shown in Table 2, the needs of the Department of Defense (DOD) encompassed a wide range of exciting materials opportunities. It was only natural that many of the best scientists from industry and academia would be attracted to these areas, especially with the added inducement of financial support. Thus, this intrusion by the US government into the traditional market driven R&D of the past tended to focus attention of industrial R&D toward developments where potential for commercialization either might not exist or was much further off in the future.

Table 2. Advanced materials thrusts in the 1960's.

High temperature polymers
Flame resistant fabrics
Superconducting filaments
High performance composites
Pollution control systems
Ultrahard shapes

In recent years another factor acting to further complicate development of new materials has been the increasing level of sophistication and complexity of the new specialty materials. This increased complexity has unfortunately been more than matched by the large number of advanced techniques available for characterizing these materials. However, the investment in such tools and skilled personnel can be prohibitive, particularly for mid-size industries.

3. R&D at Carborundum Co. (1960–75)

Let me now consider the research program that I managed at the Carborundum Company, since it illustrates effectively a number of the issues raised in the previous section. As described in a recent paper,[2] the R&D work on advanced materials at Carborundum was highly successful from a technical viewpoint. Over 30 new materials were

Table 3. New materials developed at the Carborundum Co. (1963–1973).

	Field Tested	Now being worked on	Commercially Available
Engineering Plastics			
Ekonol	×		×
Ekkcel C-1000	×	×	
Ekkcel I-2000	×		×
Crosslinkable pitch			
Flame Resistant Fibers			
Kynol	×		×
Acetylated phenolic	×	×	
Chlorinated polyethylene			
Superconductors			
NbCN multifilament yarn	×		
Reinforcing Agents			
B_4C multifilament yarn	×	×	
AlB_2 single crystal flakes	×	×	
BN yarn (high modulus)	×		
Ekonol fiber (high modulus)	×		×
Glassy carbon fiber	×		×
Composites			
AlB_2 flakes in Al		×	
B_4C fibers in SiC		×	
BN fibers in BN	×		
SiC whiskers (oriented yarn)			
Purification Systems			
Molten metal filter	×		
$BaCrO_4$ exhaust catalyst	×		
Ion exchange fibers	×		×
Activated carbon fibers	×		×
Hyperfilter (activated carbon film)			
Glass fabric filter (high-temp. coating)			
High Temperature Insulation			
BN fiber	×		×
Pitch fiber	×		
Bn/SiO_2 fiber			
Ceramic fiber			

discovered and brought to the level of scale-up and test marketing. In Table 3 is provided a partial list of the materials developed during my tenure at Carborundum. The success of the program is best illustrated by the fact that eight of the materials are commercially available today, while work continues on at least six of the others. Considering the relatively small size of the group (30-40 people) it is all the more remarkable that so many materials could be developed and marketed in such a short period of time. There were indeed many novel features with respect to the organization and operations and these are discussed in detail elsewhere.[2] I do not intend to review the achievements of the group, but rather to focus on those features which greatly complicated commercial development. One point that is immediately obvious from an examination of Table 3 is that most of the materials were developed primarily in response to the general goals defined by the US Government.

To better illustrate the problems confronting researchers during that era it is instructive to review one of the programs in Table 3, namely, that on the aromatic polyesters of *p*-hydroxybenzoic acid (PHBA). Today, this class of polymers is being pursued aggressively by a number of companies with the anticipation that their use as engineering plastics will provide a major advance over metals such as aluminum or steel. Of particular interest here is the examination of key decisions made that critically influenced the directions of the program.

As shown in the chronology in Table 4, this program had its beginnings in early 1963, when the homopolymer of PHBA was successfully prepared for the first time and shown to be processible by sintering or metal forming techniques. This was followed by the successful synthesis in 1966 of the copolymers of PHBA which could be melt-processed at about 400 °C but still retained their properties to well over 300 °C. Our studies were prompted by the rather large DOD effort on high-temperature polymers initiated in the late 1950's. At the time most of the DOD effort was directed at heterocyclic and related ladder-like polymers that could be processed from solution into coatings and fibers and might have use at temperatures up to 800 °C. In our approach we had realized from a study on carbon fibers that the thermal oxidative stability of carbon was limited to 400 °C. Hence, it was clear that no carbon based polymer would have utility much above that temperature. We therefore undertook to look at potentially low cost polymers that were fabricable by melt or solid state processing with the idea that such systems would have far broader utility.

In early 1970 we commercially introduced the homopolymer of PHBA (Ekonol) as a material which could be fabricated by metal forming techniques such as plasma spraying or high energy forging. This was followed in 1972 by the copolymer of PHBA with biphenol terephthalate (Ekkcel) which was commercialized as an injection molding grade polymer with a use temperature in excess of 300 °C.

Table 4. The aromatic polyester strategy.

1963-1966	Discovery of the homopolymer of PHBA (1963) Discovery of copolymers of PHBA/BPT (1966)
1970-1972	Introduction of Ekonol/Ekkcel to market • Also developed high modulus fiber • Purchased Three Plastics Co. • Far east joint venture with Sumitomo Chemical • Metco markets Ekonol as plasma spray powder however • Acquisitions don't work out • Market for Ekkcel develops too slowly
1974-1978	Carborundum comes under financial duress • Tries to decommit from Ekonol • Cuts back effort on Ekkcel • Fiber is forgotten • Is acquired by Kennecott however • Metco holds Carbo to Contract on Ekonol • Dartco licenses Ekkcel • Sumitomo continues work on fiber
Today	Program is Highly Successful • Carborundum still manufactures Ekonol—very profitable • Dartco has 20,000,000 lb/yr plant on stream—Xydar (Ekkcel) • Sumitomo introduces Ekonol fiber (1985) • Many companies poised to introduce similar polymers however • Large potential may lie elsewhere • Fundamental understanding lags

To help accelerate commercialization of these new materials the Carborundum Co. acquired several US plastics companies (total value ca. $ 30 million) and also established a joint venture with Sumitomo Chemical in the Far East on these new polymers. In the meantime, in 1971, we also demonstrated that the copolymer could be melt drawn into a high modulus filament with properties similar to the Aramid fibers. Of more immediate importance, the Metco Co. began to make significant progress in marketing the Ekonol resin as a plasma spray powder. However, other than for that application, the markets for these materials developed very slowly in the ensuing three or four years. As it turned out, the acquisitions were not particularly good fits and contributed little to the commercialization of these new polymers.

In the meantime, the CEO of Carborundum began to explore other more direct routes to diversification that did not require as much commitment in time and resources, and the momentum to the materials R&D effort began to disappear. In fact by the late 1970's, attempts were made to license the polyester technology and to decommit from prior sales agreements. Fortunately, the Dart Corporation had identified an important use for the injection moldable polyester, Ekkcel, for high temperature cookware, and went ahead and licensed the technology from the Carborundum Co. By 1983-1984 they had made sufficient progress to justify putting a 20 million lb plant on stream to manufacture the polymer under the trade-name Xydar. In the case of Ekonol, although Carborundum tried to decommit from manufacturing the polymer. Metco held them to an earlier agreement to supply the powder. Fortunately, for Carborundum, the use of Ekonol became and has continued to be a highly profitable specialty polymer. Subsequently, the Carborundum Co. was purchased by Kennecott which was acquired by SOHIO which was then taken over by British Petroleum.

There are several lessons to be gleaned from this brief narrative. First of all, the discovery, optimization, scale-up and test marketing of these new polymers was readily achieved within five to seven years, and this was true for practically all of the materials listed in Table 3. The next step which involved a much larger commitment of financial resources required a high level management better skilled in the nuances of the new technology and able to make key decisions in a timely fashion. Certainly our CEO was sufficiently committed that he acquired several engineering plastics companies with the intent that they would accelerate the use of these new polymers. Since these were already profitable organizations the risk in the acquisitions was minimal and at least created a very desirable image. Unfortunately it turned out that the product lines and skills base of these acquisitions were far removed from the potential uses of our new materials. Undoubtedly, the most serious problem was the absence of a clear market need which typified the developments described earlier in the "Historical Perspective". In retrospect, it is clear now that we should have been able to distinguish the needs of the market place from the wish list of government agencies.

4. Polymer R & D at IBM (1975-present)

Let me turn now to consider in some detail a different approach to advanced materials research in industry and one with which I have been involved with since 1975 at IBM. Here, the primary goal is to develop new or improved materials for the advanced devices currently in development in the operating divisions. There is however, a unique constraint, namely, these materials must be developed, optimized and available in reproducible quantities from a vendor within two to four years. This relatively short time represents the lead time normally provided in the design and development of a new device.

It is instructive to reflect on the environment within IBM when I first jointed the company in 1975, since the attitude toward development of new materials and especially polymers was and is typical of many industries whose product lines depend on development of new or improved materials. The group that I inherited and which included a number of excellent scientists, was accustomed to working closely with the operating divisions in a response

Table 5. Advanced polymer requirements for the information industry.

Semiconductor devices	Submicron resists Low and insulating layers Etch barriers (RIE)
Packaging	Temporary binders Low TEC substrates Photosensitive insulators Encapsulants
Storage	Binders Optical substrate Moisture barrier Plastic lens
Printers	Fuser Photoconductors Toners Plastic hammer Rough ribbons Damping Inks

mode to solve problems with materials that were provided by various vendors. At the time it was clear that there were needs within IBM for advanced polymers for the many new devices under development within the company (see Table 5). It was equally apparent that the traditional suppliers of new polymers were not particularly motivated to develop such systems primarily because of the very small volumes needed for many of the applications (<1000 kg/yr). Yet these new materials were critical for the next generation of devices planned for the 1980's.

Hence, through the late 1970's, the polymer group was restructured to include a much stronger component dedicated to polymer synthesis. And by the early 1980's, the group was positioned to begin to make significant contributions to the advanced technologies of IBM. In fact, from 1982 to the present, the polymer group has on average successfully transferred two new materials each year to the operating divisions for use in a wide range of advanced devices. Some of these developments represented modifications of existing materials while others were at the leading edge of advanced materials development. By closely coupling our synthetic scientists with the device designers in the divisions, we were able to make modifications in the material adjusting in real time to the frequent design changes. As can been seen in Figure 1, this kind of internal

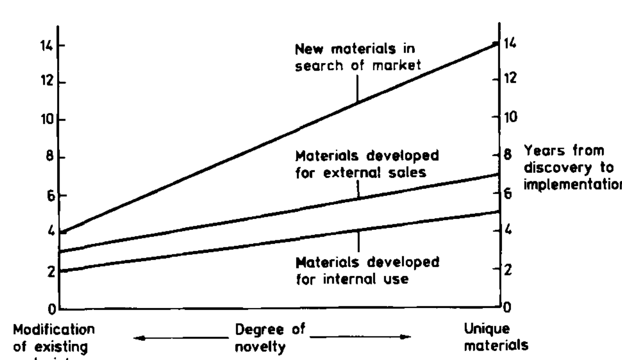

Fig. 1. Time scale for new materials development.

collaboration greatly reduces the time from conception to implementation as compared to similar research carried on by a typical vendor.

The need to conceive of, optimize and make available advanced materials within a two to four year time period has led us to challenge the scientists in our department in a unique fashion. First of all, it is essential that we pursue synthetic studies directed to what are perceived as the longer range needs. Toward that end, the scientists in the group are encouraged to spend ca. 50% of their time on basic studies in relevant areas. This aspect of their performance is measured primarily by the quality of their external publications and presentations. As specific needs emerge from the operating divisions, our scientists are able to draw upon this accumulated knowledge base to design solutions tailored to the specific needs. To further facilitate the transfer of advanced materials, we carry out studies to test the compatibility of these materials with device processing. Obviously, a critical part of the technology i.e., the design of the recipes used in device fabrication resides with the development groups in the divisions. We also have established a Synthetic Development Laboratory whose main role is to develop the optimum synthetic route, provide development quantities to the divisions and identify and work with vendors to provide commercial lots. Typically our involvement continues well into the development cycle and includes detailed characterization of the optimized material and establishment of materials specifications.

The success of the above approach is illustrated not only by the excellent track record in technology transfer to IBM's operating divisions over the past seven years, but also by the many scientific breakthroughs generated by our group (see Table 6). The scientific advances indicated here have either been cited as part of the five to six major scientific achievements of IBM's Almaden Research Center from the past three years or have been the basis for major external scientific awards.

Table 6. Significant polymer science and technology contributions from IBM's Almaden Research Center during the past three years.

Ordering in semiflexible polymers
Diffusion processes in the melt
Ageing/deageing processes
Nature of liquid crystalline polymers
Nature of monolayer films at surfaces
Mechanism of adhesion in polyimides
Measurement of attractive/repulsive forces at surfaces
Surface tunneling microscopy of gold atoms vs. organics on gold
FT Raman spectroscopy
Ab initio calculations of polymer conformations and properties
Polysilane conformations

An important component to the success of the polymer group in IBM has been the continuity of the effort over the past decade. To meet the increasing needs of the operating divisions the group has been increased from 25 people in 1975 to the present figure of ca. 60. In addition, we support between 24–27 post-doctoral scientists and host five to seven professors on sabbatical each year. The presence of these scientists greatly enhances our ability to address the basic aspects of a given problem while still focussing our attention on the short-term needs.

5. Conclusions and Recommendations

From the previous discussions in this paper, one can draw a number of conclusions concerning the future opportunities in advanced materials research.

1. The polymer materials research program at IBM represents a particularly effective model for industries which depend on the availability of advanced materials. In our group, requirements for a given application of the polymer are clearly understood and the scientist can make the incremental changes in a timely way to match the device needs. Obviously, other industries besides the information industry can greatly benefit from internal research groups whose charter includes designing the new materials which will be pivotal to their future product lines. This does not argue that joint efforts by major chemical companies and large users of polymers should not be pursued on optimizing polymers for specific uses. However, if the materials dependent industry has a core of scientists trained in the synthesis and processing of materials as relates to the company's need, then that group can take a leadership role in the interactions with the traditional materials suppliers.
2. The role of high level management is critical for those new materials programs which require significant investment. It is essential that such management be technically astute and be able to commit the resources of the company in a timely way. One can usually identify the presence of such management in those organizations which maintain an excellent track record on commercializing new materials. The converse hardly needs further discussion. It should be noted that in programs such as the one at IBM, the decision to scale-up a new material is not as expensive because of the smaller amounts involved and such decisions are made at an intermediate level of managements.
3. A number of companies are committing significant R&D resources to work in specific areas such as conducting polymers, non-linear optical materials, toughened ceramics, high-T_c superconductors, diamond films etc. with the expectation that the eventual payoff justifies the investment. Experience teaches us that programs which still require major technical breakthroughs can be stifled through early commitment of large resources. Typically, breakthroughs in these areas depend

on talented scientists working in small groups who understand and address the problem in its entirety. A commitment of larger resources aimed at increasing the likelihood of success often works in the opposite direction since the effort to be effectively managed is broken up into a series of interrelated tasks which are readily managed through PERT charts.

4. Recent attempts by the US Government to stimulate industrial R&D by building up efforts in universities and/or the National Laboratories does not address the critical problems confronting much of US industry. There is absolutely no substitute for industrial scientists who are effectively interfaced with the needs of the particular industry and its markets and have available to them the latest techniques to probe and address the future needs in materials.
5. It is essential that universities undertake to train high quality materials scientists skilled in synthesis and processing of new materials. It should be obvious that the development of new materials tailored to specific needs or equally as important the unexpected discovery of materials with unique properties lies primarily within the purview of the clever scientists trained effectively in synthesis and processing of materials.

Finally, let me return to the question posed in the opening of this article; namely, can one organize an advanced materials R&D effort which addresses the opportunities of the 21st century? I would argue that for industry, the primary emphasis should be directed at fulfilling the perceived materials needs of the next two to five years as discussed in conclusion "1". Such an approach will not only impact the company's immediate needs, but hopefully would have sufficient latitude to permit for unexpected breakthroughs which address longer range opportunities. With respect to planning and implementing a materials R&D effort for the 21st century, past experience shows that it is extremely difficult to identify needs of the next five to seven years let alone 10–15 years from now. Thus, one must think in terms of national or even international programs to establish an infrastructure which 1) attracts talented young people into the science programs of our universities, 2) provides for continuity of high risk research in all sectors of the R&D establishment, and 3) places responsibility for commitment of funds and resources by the government and industry in the hands of technically astute leaders. In this way one can greatly increase the potential for successful development of advanced materials critical to high technology industries of the 21st century.

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Magneto-Optical Recording and Data Storage Materials**

Rare-Earth Transition-Metal Alloys
Thermomagnetic Writing
Lorentz Microscopy

By Frans J. A. M. Greidanus* and Stefan Klahn

Amorphous rare-earth (RE) transition-metal (TM) alloys are used for magneto-optical (MO) recording a rapidly developing technology, which combines the possibility of achieving high bit densities with practically unlimited erasability and rewritability. During the last years new insights, relating material properties to recording performance, have been obtained. New experimental techniques, such as the observation of magnetic contrast in the electron microscope, have made a major contribution to the understanding of domain formation processes. The RE-TM alloys have been most successful in recording applications until now. In these materials the RE-TM composition determines both the compensation and Curie temperatures and has a strong impact on the recording characteristics. Improvements in deposition techniques and the application of dielectric layers resulted in carrier-to-noise ratios of 61 dB. Despite major improvements, problems related to corrosion and structural relaxation, which lead to long-term instabilities, have not yet been solved completely. Another important topic is the so-called direct-overwrite problem, which will be discussed in relation to the material properties.

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