

tinually experimenting with the new. We are intellectually and emotionally ill-prepared for real downsizing. The necessary centripetal forces and sessions which are essential to our self-understanding as a community have simply been drowned out by a proliferation of symposia and papers.

The Society has not been able to involve the polymer community to a major extent. (Less than 5% of the *Journal of Materials Research* is devoted to polymers.) Nor has MRS yet been able to

We grew on the back of
the rising tide of U.S.
technology and science
of the 1960s, 1970s,
and 1980s.

bring in pre-existing, specialized societies such as Carbon, EMSA, etc. The focus on all three dimensions of interdisciplinarity has been fuzzed up, not sharpened. Remarkably, MRS is the least active among many societies in engaging its members in societal matters.

So be it. Let these be the challenges which the Society will tackle for the next 20 years.

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How It All Evolved

Harry C. Gatos, 1973–1976 MRS President

The founding and operation of MRS was the culmination of my ten years of frustrated effort in searching for a professional home (old, renovated, or new) for the young, homeless materials science. The leaders of the existing materials societies strenuously resisted accepting that materials science existed outside the materials they dealt with, be they metals, ceramics, or polymers. The founders of MRS were just a small but “driven” minority with a vision of a “materials-blind” materials society. With due respect for all the founders, I must say that among all of us, Rustum Roy had the greatest vision. He was the most vocal, the most energetic, the most convincing, and the greatest doer.

The early years of MRS have been highlighted in the *MRS Bulletin* by other former presidents. I need not elaborate further. I would like, instead, to trace the roots and shaping of our discipline. I will also comment on my experiences with professional societies prior to the founding of MRS.

It all began with the discovery of the transistor in the late 1940s, which led to the transformation from the vacuum tube to solid-state electronics. This transformation cannot, however, be referred to as a development, an improvement, or a change. Going from the vacuum tube (or radio tube) to the silicon chip, which can contain many millions of vacuum-tube-equivalent devices, cannot be called an improvement. A hand-held computer cannot be referred to as an improvement of ENIAC (the first advanced computer built in the 1940s) which had 18,000 vacuum tubes and, when it was turned on, allegedly dimmed the lights of the city of Philadelphia.

The transformation to solid-state electronics resulted in the birth of a new and different era for science and technology. That birth took place in 1948, the first infant steps were taken in the early 1950s, and from then on there has been runaway growth.

Let us look at the heart of this transformation. Vacuum tube electronics is based on the generation and control of electrons in vacuum. In semiconductor electronics, the current carriers (electrons and holes) originate in the atoms within the solid, and their characteristics depend on the atomic-scale structure and composition of the solid.

It is instructive to go back to about 1950. The understanding of the conduction of carriers in semiconductors and their manipulation to achieve device functions were at a respectably high level, even by today's standards. In fact, the book by William Shockley, *Electrons and Holes in Semiconductors*, a classic in that field, was published in 1950.

What about the state, at that time, of suitable materials for fabricating working semiconductor devices? For all practical purposes, such materials did not exist. Germanium single crystals with reproducible characteristics were necessary (Ge was then the key semiconductor). Technology for single-crystal growth from the melt was hardly in existence. Starting materials with background impurities less than a few parts per bil-

lion were needed; that meant many orders of magnitude beyond the prevailing limits. No crystalline defects—planar, line, or point—should be present. At that time, these requirements were just fantasies; so was the realization of devices which were being conceived, and even patented. Some of these theoretically conceived devices were fabricated many years later (as suitable materials and processes were developed) and were proven to be valid and valuable.

I quote from the book *The New Alchemist* by Dirk Hanson, a journalist, reporting on this period of the early 1950s. He states: “At first the financial arguments of sticking with the vacuum tube were persuasive, and tube engineers could readily temper the enthusiasm of the solid-state people with the weight of experience. Maybe the transistor was not going to be such a big thing after all. The early fuss died down. For one thing, the manufacturing methods were completely ad hoc and seat-of-the-pants. Controlling electricity by rearranging the atoms was nice practice, in theory, but not quite so awe-inspiring when it came to the production line, where almost anything could go wrong, and frequently did. It was like trying to do surgery on the head of a pin. It was wondrous that transistors worked at all, and quite often, they did not. Those that did varied widely in performance, and it was sometimes easier to test them after production and, on that basis, find out what kind of electronic component they had turned out to be. If they failed, it could have been due to any number of undesirable impurities that had sneaked into the doping process. It was as if the Ford Motor Company was running a production line so uncontrollable that it had to test the finished product to find out if it was a truck, a convertible or a sedan.”

Actually, the situation was worse than

Views on MRS and materials research from former MRS presidents.

that described in the above quotation. The electronic characteristics of the devices that tested well after fabrication did not remain constant with time. A fraction of them, presumed to be able to live forever, died within months. For a while, in the early 1950s, it appeared that semiconductor electronics was heading for an early crash. The embryo of our transformation, rather than growing, was being consumed.

Fortunately, a handful of industrial organizations realized that solid-state technology had no learned disciples and that no single existing technical discipline could accommodate it. They decided to go forward from ground zero, building on intimate interactions among the classical disciplines.

Bell Telephone Laboratories (BTL) was the world's most advanced center. The point contact, the p-n junction, and the field effect transistors were discovered there. There, also, single crystals had begun to be grown from the melt, and purification breakthroughs (such as zone refining) were taking place. In parallel, extensive studies were initiated on the preparation of clean and reproducible surfaces and on their electrical characteristics, chemical properties, and structure. Following the lead of BTL in the early 1950s, traditional disciplines in other semiconductor research centers in this country were merging and joining forces. Among such centers were the industrial research laboratories of General Electric, IBM, Raytheon, RCA, Texas Instruments, and Westinghouse. In Europe, the research laboratory of Philips followed.

The merging of the classical disciplines was, from the start, neither particularly smooth nor effective. Communication was not at a very high level because of "technical language problems," basic training, and each discipline's tradition. It was not uncommon to encounter physicists developing advanced measurement techniques, only to apply them to chemically very poorly prepared and assessed samples. Conversely, chemists did engage in carrying out primitive physical measurements on chemically state-of-the-art samples.

The pressure was immense, however, to develop materials, processes, and structures that would bring to life phenomena and devices already conceived theoretically and even patented. Key electronic characteristics needed to be related quantitatively to structural and/or compositional parameters. Thus, it is easy to see that aspects of physics, chemistry, metallurgy, chemical engineering, electrical engineering, and possibly other disci-

plines were needed to enter this undertaking and to function in an interactive mode. From such new polygamous relationships among all these disciplines, inevitably a new offspring began to take shape: *materials science and engineering*. Reliable quantitative "property-structure" or "property-structure-processing" relationships were imperatively important. The reason is simple: The semiconductor properties relevant to solid-state electronics are the energy gap, the electrical carrier concentration and mobility, and the minority carrier lifetime and diffusion length. Of all these properties, only the energy gap can be assumed to be reliably constant from crystal to crystal for a given semiconductor. All other properties can be modulated by orders of magnitude, intentionally or unintentionally, by small changes in chemical composition, defect structure, and chemical or structur-

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al homogeneity. No other class of materials exhibits such broad and sensitive "property-structure" relationships.

Semiconductors were perfectly suited to bring about the birth of materials science and engineering. Their covalent, highly directional tetrahedral bonding is a major advantage from both theoretical and experimental points of view. This bonding, as compared to metallic bonding, simplifies theoretical approaches. It limits the solubility of other components. Accordingly, attaining high purity becomes much easier. (Today, single crystals of Ge and Si can readily be obtained, with total impurities not exceeding one part in 10^{12} .) Furthermore, preventing defect formation is relatively simple. (Si single crystals, eight inches in diameter and a few feet long, are routinely grown dislocation-free). As a result of their covalent bonding, semiconductors exhibit cleavage at a well-defined crystallographic orientation. It was by cleavage of Ge in high vacuum that the first "virgin" surfaces were obtained and studied. In addition, tetrahedral bonding lends itself to atomistic approaches both of the bulk and of the surfaces. Such approaches, in con-

trast to the more rigorous statistical ones, lend themselves to the making of predictions leading to the development of materials and structures with needed properties. For example, the discovery of III-V semiconductor compounds was based on the simple "octet rule." The behavior of the surfaces of these compounds was, at first, accounted for on the basis of a simple atomistic model. Finally, it is due to their covalent bonding that their electrical properties can be readily modulated by many orders of magnitude. For example, one part per million of an impurity from either group III or V of the periodic table increases the electrical conductivity of intrinsic Si by five orders of magnitude.

The techniques, processes, and instrumentation developed for semiconductors were in time used with other classes of materials such as metals and ceramics. Crystal growth methods were adapted for the growth of high-quality single crystals of metals and ceramics. Processes for achieving high-purity semiconductors found their way to metal technologies. Advanced techniques for chemical and structural characterization, widely used today, were developed or refined in conjunction with semiconductors. Examples of such techniques are low-energy electron diffraction and its variations, Auger spectroscopy, x-ray photoelectron spectroscopy, infrared spectroscopies, and others. Actually, the ultrahigh vacuum technology necessary for the wide use of these techniques was developed for the study of "virgin" semiconductor surfaces. The characterization and understanding of defect structure in solids made striking advances with semiconductor materials. For example, dislocations were first observed, in two and in three dimensions, in semiconductors. It was in this way that the concept and theory of dislocations were experimentally verified and further refined.

The theoretical framework developed for understanding and utilizing the behavior of semiconductor materials served as the basis for the theoretical treatments of all classes of materials. It is of interest to note that John Bardeen's brilliant theoretical concept of surface states on semiconductor surfaces led directly to the discovery of the point contact transistor. Some years later, he was awarded his second Nobel Prize for accounting for the phenomenon of superconductivity in metals.

In the 1950s, rapid advances in relating the composition, structure, and processing parameters to the electronic characteristics of semiconductors were communicated and discussed in meetings orga-

nized primarily outside professional societies. The tradition, atmosphere, and bureaucracy of the existing professional societies could not accommodate the excitement associated with those meetings. The Metallurgical Society of AIME formed an electronic materials committee and helped organize some meetings; but those meetings were not included in the regular meetings of the society and the proceedings could not be published in the society's transactions. We argued for years that the proceedings should be a part of an archival journal rather than be published in self-standing volumes. When I became chairman of that committee, we finally convinced the society to publish our proceedings. Indeed, they appeared for the first time in March 1964, in volume 230 of the Transactions of the Metallurgical Society of AIME. One or two years later, however, this practice was discontinued. I know that today's leaders of that society regret their predecessors' attitude and actions.

The Electrochemical Society formed an Electronics Division which organized meetings on topics ranging from materials to devices. This society exhibited some flexibility. It was a relatively small society and had the potential to become a materials society. However, the name of the society was not quite appropriate; in addition, the traditional membership viewed the electronics people as out-

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siders. I was a member of the Corrosion Division before I joined the Electronics Division, and thus a member in good standing. When I became president of The Electrochemical Society in 1967, I convinced the directors to consider changing the society's name. For obvious reasons, I suggested the J. Willard Gibbs Society as a potential name. We brought the matter to the membership for a vote. I wrote an editorial, "Our Society's Name," in the *Journal of the Electrochemical Society*, (July 1967), in which I pointed out the benefits to be derived from a name change. The membership voted down any name change and a great opportunity was lost.

Soon after MRS was founded, I felt strongly (and so did Rustum Roy) that

we should honor Arthur von Hippel. He was the earliest true pioneer in materials science and engineering. As far back as the early 1940s, his Laboratory for Insulation Research at MIT was an interdisciplinary center. Doctoral thesis research was carried out there by graduate students from the departments of chemistry, physics, electrical engineering, and metallurgy (including ceramics). He most appropriately defined his work as molecular science and molecular engineering. It was unfortunate that Professor von Hippel's vision and experience were not utilized when the Materials Research Centers were organized at MIT and elsewhere in the 1960s.

As one of its founders and as its first president, I am proud of MRS. We created a forum for the young, exciting materials community. The growth of MRS shows the magnitude of the need we met. We must appreciate, however, that rapid growth has inherent hazards. The potential for attracting opportunistic and/or limelight-seeking individuals is one such hazard. The possible loss of flexibility is another. A third hazard is the potential loss of readiness to respond promptly to emerging materials fields. I trust that MRS's leaders are addressing those challenges head-on.

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EDUCATION EXCHANGE

Science/Math Carnival

Among the many letters sent to Sandia National Laboratories/California from local teachers and students during the past two years, we find letters from:

- A first grade teacher in Tracy, California, who wrote: "These kinds of experiences are so important in keeping science alive for students and teachers. It could not have gone better. Many thanks.";
- A teacher who was signing up for some summer courses on science so he would be able to better teach his students, who were asking for more science;
- Students writing to thank us for our Science/Math Carnival, describing their favorite activity (and why it was their favorite), and explaining why they want to be scientists when they grow up;

- Students who sent drawings of how a laser works, of a floating bubble, or of an idea they want to patent;
- Parents asking us what the Science/Math Carnival is, because their children came home "really excited, and kept telling us about the things they learned in school that day.";
- Teachers saying how they would continue to teach a certain "Carnival" science topic in their classes;
- The director of a bilingual migrant worker program, who was so thrilled by our presentation that he wanted to arrange a partnership with Sandia on future programs (two joint activities have already resulted from this); and
- Pictures from a fifth grade class of what a scientist looks like, showing about a third as women and half as minorities. (The same drawing assignment prior to a

presentation by Sandia National Laboratories resulted in 30 drawings of white males.)

What prompted these letters? They were all written after a **Science/Math Carnival** had been held at their schools. The impact of Carnival has been immense in the local schools, and in this article I hope to give you a feeling of what it is, why it was started, and how much fun and excitement it generates (not only from teachers and students, but from the staff as well).

In April 1991, in an effort to increase our education outreach activities, Sandia National Laboratories/California decided to go directly to local teachers. We invited a group of elementary school teachers in the Bay Area to an after-school meeting, whose purpose was to ask how we, a national laboratory, could