As the Presidents See It ...

The Ideals and Ideas that Led to MRS

Rustum Roy, 1977 MRS President

My year as official President, 1977, started in 1976 and differed little from the decade 1967–77, which was, in materials science terminology, the nucleation phase of the Materials Research Society. Who we are today is stamped with the structure of the nucleus that eventually survived and reached critical radius. I will write about our ideological roots, if not our space group and symmetry.

Max de Pree, CEO of Herman Miller, which is consistently rated as one of the best-run corporations in America, has written in his widely respected book, *Leadership is an Art*, that every successful organization and its people must be thoroughly imbued with that organization's own vision and "story." What was the common vision of the Materials Research Society founders? Where did they acquire that vision? How did they bring it to fruition? Bringing the story of MRS to its members is the focus of this short essay.

Although, as the reader can see in my article on page 74, the beginnings of the Society are linked, for 16 years, to the Materials Research Laboratory at Penn State, the existence of the Materials Research Society (MRS) is one more proof of Margaret Mead's wonderful admonition:

> *"Never doubt that a small group of thoughtful, committed citizens can change the world; indeed it's the only thing that ever has."*

This article is about such a group and their vision.

People

MRS was brought into being by a small group of "conspirators" who were committed to changing the world of professional meetings and society affiliations. They were committed to genuinely engendering, fostering, and sustaining interdisciplinary interaction. They knew that something *new* had to be done because, by the late 1960s, in spite of rhetorical lip service and massive "incentives" by the federal government, most of the university world had hardly budged from its disciplinary moorings. Many of the names of these early founders of the Society will appear in this article and, with due apologies to some of them, I believe that Harry Gatos, Ken Jackson, Mark Myers, I. Warshaw, and I were the key players in creating MRS. I served as host and facilitator of the group. Two *institutions* provided most of the (not inconsiderable) bootlegged support for several years: Penn State's Materials Research Laboratory (which I directed) and the Bell Telephone Labs, via Jackson, backed by Bruce Hannay, vice president, and, of course, Bill Baker, president.

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Vision

In the early phases (1967–83), during most of which the Society and its precursor activities were administered out of our lab at Penn State, the vision which guided those involved with the Society had certain key elements. First, the Society was to focus *exclusively* on *interactive* factors in three dimensions of materials research:

- Interdisciplinary research, with materials science and engineering clearly identified as one discipline (department), alongside electrical engineering, physics, chemistry, etc.
- Interaction along the science-to-engineering-to-technology axis.
- Interaction among institutions of engineering and science: industry, university, and government laboratories.

Since there were half a dozen materials societies in place, our vision and opportunity, we knew, lay in the interstices and overlap. That is what MRS had to emphasize to find its place in the lineup.

A second element of the MRS vision was to be constantly attentive to innovation in the Society's business, which was enhancing communication among the Society's members, and from them to the larger society beyond. Three or four key innovations, present from the beginning of MRS history, were:

- The use of simultaneous "topical" symposia (like Gordon Conferences) as the focal points for the entire program. This model also provided a unique mechanism for involving a whole series of new-to-MRS scientists in each symposium. (To this "invention," now copied almost universally by most disciplinary societies, MRS owes much of its numerical success.)
- Maintaining a genuine balance of topics within the ellipsoid of interaction to involve the widest spectrum of interests, disciplines, and potential members. (A healthy pattern was established by the bringing in of two materials groups—the cement and the "rad waste" groups. Penn State's MRL had the major university research efforts in both these fields, making it possible, therefore, to offset the high-tech laser-processing and electronic materials bias which would have set in.)
- Providing sufficient opportunity for specialists to apprehend the overall picture of materials research. Symposium X has always been very successful in doing this for senior experienced managers, but the fraction of attendees taking advantage of it has become smaller, instead of larger.
- Continuing always to experiment in the heartland of the Society's business—innovation in communicating information about science among scientists—beyond meetings, journals, and proceedings volumes. This was the motivation behind our experiments with live satellite broadcasting of selected symposium sessions, our videotaping of sessions, and the video history of our field.

Present and Future

What would that small group of conspirators who founded the Society say of it today?

They would be duly proud of the place MRS has taken among all the materials societies. They would be especially proud of the success of their model for using topical symposia as the substance of the meetings. Also, they would be proud of the involvement of large numbers of new people, and of the ability the Society has shown for broadening its base to encompass many different fields of materials. They would know that we grew on the back of the rising tide of U.S. technology and science of the 1960s, 1970s, and 1980s and they would look ahead to prepare for the ebbtide.

Yet there would be, I suspect, some regrets. Small remains beautiful. The unwillingness to tame the growth and grandiosity virus that affects American institutions has, perhaps, led MRS to doing more of the same, instead of con-

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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 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.

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

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How It All Evolved

Harry C. Gatos, 1973–1976 MRS President

Views on MRS and materials research from former MRS presidents.

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 billion 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

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