SPECIAL ISSUE

HIGH-TEMPERATURE SUPERCONDUCTIVITY

One can argue that superconductivity in "the oxides" was discovered in 1964, 1986 or 1987, depending on which oxide is chosen: the first to show superconductivity at any temperature, the first with a T_c greater than 20 K or the first with a T_c greater than 77 K. For many of us in the field, the time we remember is probably early December 1986, when experimenters from the University of Tokyo announced at the Materials Research Society meeting in Boston that they had confirmed the earlier findings of Georg Bednorz and Alex Müller at IBM in Zurich. There followed a frenetic period of a year or so that was unique in the history of science, with the American Physical Society meeting of March 1987 representing its peak of intensity (or hype, depending on one's point of view), although a close second must have been the Washington gathering where President Reagan gave the plenary talk.

Luckily for the health of those of us who remain in high-temperature superconductivity, that period has passed. The field remains large and active, but is only one of many exciting areas of study in condensed matter physics. It is even possible to see occasional issues of *Physical Review Letters* that do not contain any papers on the topic, and no increase in T_c above the commonly accepted maximum of 125 K has been reported for over three years. Now that the field has become simply busy, rather than frantic, it seems entirely appropriate that PHYSICS TODAY should devote a special issue to summarizing the progress in the roughly five years since the start of high-T_c superconductivity.

In my role as guest editor, I asked the authors to cover the field by addressing each corner of what I used to know as the "synthesis loop" of materials science, but which became a tetrahedron in the recent national Academy report "Materials Science and Engineering for the 1990s." But whatever geometry one gives it, the theme is the same: To make progress in materials science there must be a balanced attack—make a new material (see the article by Arthur Sleight on page 28), understand the structure of what you've made (see the article by James Jorgensen on page 34), determine its physical properties and why it behaves the way it does (see the articles by Bertram Batlogg on page 44 and by Philip Anderson and Robert Schrieffer on page 54), and finally, once you know what it does, make something useful out of it (see the articles by Randy Simon on page 64 and by David Larbalestier on page 74). (For some of us, there are subsequent steps, such as selling products and perhaps making money.) Of course, the process is not sequential. While some of our colleagues grow large single crystals for structure studies, others puzzle over the microscopic mechanism, and at the same time the more practical-minded of us try to wind magnets or fabricate microwave components or SQUIDS.

As you read the articles, I suspect many of you will have similar reactions. First, you will notice that the days of rash promises—of levitated trains, of faster computers, of room temperature superconductors—are gone. The articles seriously assess the problems that remain, from synthesis to theory to pinning in multifilamentary wires.

Second, you will be impressed by the progress that has been made. All the power of interdisciplinary materials science has been turned loose on these oxides, from the creative art of the solid-state chemist, the analysis made possible by the pulsed neutron source, the delicate touch of the experimentalist measuring a tiny crystal, the "little gray cells" of the theorist and finally to the determination and pragmatism of the applied scientist attempting to establish a technology in the absence of the science.

Third, you will be surprised by how many challenges remain. You will perhaps feel, as I did on reading the drafts of these articles, that the oxides are Nature's mocking answer to those of us who thought solid-state physics was a mature field. These materials have exposed our weaknesses. There are multiple examples cited in the articles that show how inadequate our skills and knowledge were, and in some cases still are, to deal with these fascinating materials. We still have to understand the synthesis of metastable and highly defective structures, the theory of strongly correlated systems, the measurement of highly anisotropic crystals, the properties of grain boundaries (which are anathema to those making conductors but make very respectable Josephson junctions for SQUIDS), and we have yet to observe undisputed evidence for an energy gap.

What are my own guesses about the next five years? Here my comments apply rather specifically to US activities. In common with many other areas of science, essentially all "derivatives" of trends except for the scientific progress itself are negative. There are two positives: the sense of need for common action that seems to be emerging among researchers in materials science and solid-state and condensed matter physics, and the promises by Allan Bromley to insert a Presidential initiative for materials science and solid-state and condensed matter physics into the fiscal year 1993 budget. How superconductivity will fare in this initiative and later in the budget is unclear, but the signs are not encouraging. It is not obvious to me that funding will be maintained at a level that will take full advantage of the investment made in the last five years.

The lack of enthusiasm for the field in the laboratories of major US industrial companies is particularly depressing. This is especially true in large-scale applications, such as magnets, motors, generators and power transmission. The dramatic progress outlined in Larbalestier's article was made at two industrial laboratories in Germany and Japan, in work supported, I believe, by their own corporate funds. In stark contrast, no large US company has a significant commitment to research in this area. Companies that might have been expected to pursue commercialization of large-scale applications seem instead

Levitation at ISTEC, the International Superconductivity Technology Center in Japan. Improvements to the bulk ceramic form of $Y_1Ba_2Cu_3O_7$ at ISTEC's Superconductivity Research Laboratory have allowed increases in the weight that can be lifted by a magnet mounted above the cooled superconductor. Last October the laboratory's director, Shoji Tanaka, was levitated. The lab is supported by many Japanese companies and a few from the United States.

to be interested in electronics. The level of Federal support for basic research relevant to large-scale applications of high-T_c materials, at a time when US industry appears willing to concede this technology, raises questions of science policy that will have to be faced in the next few years.

In small-scale applications, such as analog and digital electronics, sensors and instrumentation, we have learned an interesting lesson in the US. I have been fascinated by the effect that the High Temperature Superconducting Space Experiment has already had on US capabilities to create high-quality films and microwave devices. In this experiment, the Naval Research Laboratory in January 1989 asked over 20 companies and government laboratories to make microwave components of high-T_c materials that would be flown and monitored on a space package in 1992. Each laboratory had to deliver before July 1990 five packaged devices that would survive in space. This experiment will be followed by HTSSE II, which will fly much more advanced devices and subsystems by 1996. The components on HTSSE I will not do anything useful in space; they will only be monitored. Yet simply by setting a goal, a timetable and deadlines and by providing very modest funding to each laboratory, the Federal government has stimulated remarkable progress. Perhaps this is a model we should use more widely.

In large-scale applications, a similar goal would be to provide specific funding for a 30-tesla magnet that would operate at 4.2 K, with funding tied to reaching this level of performance! I am not sure that similar incentives can be used to create new materials or to produce a theory that all theorists will happily agree upon, but they do seem appropriate for more engineering-oriented objectives.

Many of you who are younger than I will believe that the past five years were the most remarkable period in the 80-year history of superconductivity. Heike Kamerlingh Onnes would have argued differently, and those of intermediate experience would suggest that the five years from 1957 to 1962—which yielded the Bardeen–Cooper–Schrieffer theory, the observation of the energy gap by tunneling, and the discovery of high-field superconductivity, the prediction of the Josephson effect and the construction of the first superconducting magnets—were unusual times too. I am confident that the next five years will see exciting scientific and technological progress, despite the storm clouds of nontechnical problems it is easy to see gathering ahead. My primary concern is that the US, and especially US industry, seems to be losing interest in the field just when our knowledge base has been built to the point that industry's interest should be growing rather than shrinking. Perhaps the accusation that we as a nation do not have the will to stay the course for the long term will prove to be true.

If I were forced into the dangerous game of predicting progress worldwide in the next five years, I would venture that the goal I described above—magnets operating at fields of 25 T or greater at temperatures up to 20 K—seems well within reach, with higher fields and operating temperatures following later. This technology base in magnets will encourage progress in motors, generators and other power applications.

Given the progress in microwave applications that has been created by HTSSE I and will be driven in a logically planned manner by HTSSE II, it seems very likely that microwave systems of oxide superconductors will be performing useful functions in space within five years. I expect infrared detectors and SQUID magnetometers made of oxides to have an impact on a variety of military and commercial sensor markets well before those five years are over. Simple digital circuits, say, using 1000 Josephson junctions of the weak-link type, will be operating at about 50 K. To be even more speculative, it seems to me inevitable that new electronic devices that use both superconductivity and the other properties of oxides (insulating, semiconducting, magnetic, ferroelectric, pyroelectric and optically active) will be invented. The oxides certainly appear to have more potential for such hybrid device structures than the old superconductors such as Nb and Nb_3Sn ever offered. Finally, I would guess that the major impact of oxide superconductors will be in hybrid electronic systems using semiconductors such as Si and GaAs, superconductors (both Nb and oxides) and optics. Demonstrations of such systems should begin before the next five-year review in PHYSICS TODAY.

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