Research Opportunities in Superconductivity

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Preface

This is the report of the Workshop on Problems in Superconductivity held at Copper Mountain, Colorado on August 22 and 23, 1983 under the sponsorship of the National Science Foundation through D. H. Liebenberg under agreement number DMR-8314163 and the Office of Naval Research through E. A. Edelsack. Any opinion, findings and conclusions or recommendation expressed in this publication are those of the authors and do not necessarily reflect the views of the National Science Foundation or the Office of Naval Research.

The workshop was organized in three half-day sessions with M. Tinkham, M. R. Beasley and D. C. Larbalestier as discussion leaders for each half day. A. F. Clark and D. K. Finnemore were the co-chairmen. The workshop was organized under the auspices of the National Bureau of Standards, Boulder, Colorado.

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1. INTRODUCTION

The science of superconductivity is not one whose development is limited by the lack of ideas. It is limited by the need for consistent support and for major new facilities to prepare and characterize the materials and physical structures necessary to further basic research and advance the technology. Impressive foreign capabilities in both fundamental understanding and technological skills exist today. A commitment to long term, broad based, multi-agency support is required for the U.S. to maintain its status in this field and to nurture its technological promise, both immediate and long term. While the field of superconductivity may not be alone in such claims, this fact in no way makes the situation less urgent.

Superconductivity is a ready technology. The National Accelerator Laboratory has recently activated more than 1000 6-meter-long superconducting magnets in its Tevatron, which should allow the U.S. to leapfrog ahead in high energy physics while saving about $5 million per year in power costs. Whole-body NMR, made possible with superconducting magnets, has just barely tapped its potential in medical diagnostics and is anticipated to be a $2 billion industry in two years. Superconducting microcircuits form a class of the world's most sensitive electromagnetic instruments, approaching the quantum limits of measurement from dc to far-infrared frequencies. Superconducting circuits for high speed signal processing exceed the performance of competing technologies.

Superconductivity is a future technology. The essential role of superconducting magnets in magnetically confined plasmas for controlled thermonuclear fusion is well known. The widely accepted belief that future ultra-fast computers must ultimately operate at cryogenic temperatures, almost certainly involving superconductivity in some fashion, is in no way diminished by the recent reduction in the industrial Josephson Junction (JJ) computer effort in the U.S.

Superconductivity is producing a stream of new ideas which show significant promise for future advances. Phenomena in ternary compounds, artificially structured materials, inhomogeneous superconductors, organics and superconducting device structures of various sorts indicate a long range potential for new science, more usefulness and wider applicability as superconductivity extends its limits.

Superconductivity is a unique tool for the study of phenomena in many fields of science. Superconductivity provides unique capabilities to test fundamental internal mechanisms with simple external measurements for understanding basic physics problems and problems in materials science. Moreover, superconducting instruments provide uniquely sensitive means for making many physical measurements.

In spite of this progress and promise, many in the superconductivity research community felt a need for clearer focus and a concern for the problems of superconductivity research and development. (The payoff in this field can be very high and proper funding has become a national issue). As a step in this direction, a Workshop on Problems in Superconductivity was convened specifically to:

1) Identify the key problems in superconductivity in the next five to ten years;

2) Determine those areas of research where intellectual or practical payoff appear highest;

3) Specify some of the factors now limiting progress and discuss areas where breakthroughs seem possible, and

4) Investigate research opportunities which might open if new facilities or techniques were available.

This report is a condensation of the 1 1/2 days of lively and interactive discussion which transpired among 36 representatives of the superconductivity research community.

II. BASIC PHYSICS

The opportunities for advances in the basic physics of superconductors can be usefully divided into two categories: a) the internal development of the scientific understanding of the phenomenon of superconductivity and b) the external impact of superconductivity as a unique tool in other areas of science. Some of these opportunities are sketched in the following paragraphs, after which ways of overcoming barriers to progress are briefly discussed.

A. INTERNAL DEVELOPMENT OF THE SCIENCE OF SUPERCONDUCTIVITY

One of the most active areas of current research is nonequilibrium superconductivity, meaning study of the response of superconductors to external perturbations. Much progress has already been made, particularly in linear regimes involving small departures from equilibrium. For example, a unique "charge imbalance" type of disequilibrium between normal and superconducting electrons has been analyzed, whose hallmark is the existence of two distinct measurable electrochemical potentials in the same metallic volume. An outstanding problem, however, is the thermoelectric generation of flux in a superconducting bimetallic loop, where the observed effect is orders of magnitude larger than predicted, and also has a different temperature dependence. This extraordinary divergence between theory and experiment has posed an open question for several years. New ideas are required to resolve this problem.

More generally, the forefront in this area is now in regimes further from equilibrium, where the linear response approximation is inadequate. One example is the study of transient superconductivity, as when a supercritical current pulse is applied to a superconducting filament, and the time-development of the transition to the normal state is studied. Because the characteristic time scale set by the microscopic electron-phonon relaxation time is so short, the use of the Faris superconducting sampler circuit (based on IBM superconducting computer technology) has been essential in getting the necessary 10 picosecond time resolution at high voltage sensitivity. A slightly faster version has been reported in the past few months from Japan. A second example is the study of instabilities in the superconducting state when it is driven far from equilibrium by heavy tunnel injection of normal electrons. Japanese work indicates that there is a spontaneous break-up into a spatially inhomogeneous (on ~100 μm length scale) 2-gap state. This phenomenon remains unexplained in any satisfactory degree of detail, yet this sort of nonlinear effect of electron injection is the basis for the "Quiteron" device, a type of superconducting transistor. Many of these problems involve large heat flow so new techniques for characterizing the Kapitza thermal boundary resistance between thin films and substrates are required to allow reliable analysis of such dissipative phenomena, especially when the obviously desirable extension of nonequilibrium experiments to T << 1K is carried out.

On the theoretical side, microscopic theories (other than numerical simulations) presently are restricted to cases <u>very</u> near the transition temperature, T_c, where the energy gap $\Delta << kT$, and when phonon disequilibrium can be ignored. Neither of these restrictions hold in typical experimental regimes, and effective (i.e., tractable, yet reasonably rigorous) extension of the Schmid-Schön theory to a wider range of conditions would represent a major advance.

Moving away from nonequilibrium superconductivity, the study of superconducting arrays is an active field just coming into full flower. Here regular fabricated structures with up to $\sim 10^6$ superconducting elements in a 2-D array, weakly coupled by Josephson coupling (either S-I-S or S-N-S), are studied to see how the phase of the superconducting wavefunction on the islands eventually locks into a phase-coherent whole. This is the discrete analog of the Kosterlitz-Thouless vortex-unbinding transition in homogeneous films, but because of the discrete structure, the resistive transition is periodically modulated in the magnetic field, the period corresponding to adding a flux quantum to each unit cell of the array. Harmonic structures are also observed, corresponding to flux quanta in a rational fraction of the cells in a regular superlattice. The study of such phenomena is in its infancy, both theoretically and experimentally and it appears that important new phenomena may be discovered in this area.

Another type of advance now underway is an increase of our understanding of the interaction between superconductivity and other phenomena. The competition between superconductivity and magnetism has a long history of study, but new ramifications continue to be discovered. A very important recent development has been the study of the interaction between superconductivity and localization and modified interelectronic interaction in disordered metals. For example, this appears to provide the explanation of the breakdown of the Anderson theorem, which states that disorder should have minimal affect on T_c. This theorem breaks down when the localization length becomes less than the superconducting coherence length or when the repulsive Coulomb interaction increases as a result of a diminution of screening, possibly explaining the apparently universal degradation of T_c in radiation-damaged superconductors. In fact, the whole theory of dirty superconductors should be revised to take account of these new insights. The study of magnetoconductance phenomena above T_c due to localization effects has also provided independent determinations of previously "free" parameters, such as the spin-orbit scattering rate, again providing a much more global understanding of the electronic properties of metals both above and below T_c. Although less detailed results have been obtained to date, the study of how superconductivity is modified when in a material near the percolation threshold for electrical continuity of a macroscopic sample is another "interface" area where considerable further insight seems within reach.

Finally, we mention briefly the progress and promise of the theory aimed at calculation of material-specific properties, especially T_c, from microscopic electronic theory. At present, band theory calculations are good enough to do a reasonable job of accounting for T_c in simple materials, given empirical phonon spectra. With foreseeable increase in computational power, it should be possible to refine the electronic calculations sufficiently to determine force constants (which depend on derivatives of electronic energies), and hence phonon spectra and T_c, entirely ab initio. In fact, recent progress with the pseudopotential method has essentially reached this stage for materials with simple structures. The same sort of calculations also provide the basis for theoretical predictions of phase diagrams, including the important extension to high-pressure phases. At that point, a much more productive interplay should become possible between theorists and material scientists seeking new superconducting materials with improved characteristics. A much increased rate at which the promise of new classes of superconductors are evaluated may also follow from these developments. Although the problem of evaluating "exotic" coupling mechanisms other than the electron-phonon one adds another layer of difficulty, there has recently begun to be some progress even in this area, particularly due to the discovery of general methods of preparing tunneling barriers for superconducting tunneling studies. Further work is surely worthwhile. The experimental confirmation of a new mechanism of superconductivity would be a discovery of great fundamental significance.

B. SUPERCONDUCTIVITY AS A UNIQUE TOOL

Because the superconducting state is a macroscopic quantum state, it provides a uniquely suitable vehicle for investigating fundamental quantum questions as well as for making measurements with the full sensitivity consistent with quantum theory. In addition, it provides an attractive test bed for other questions in statistical physics. Currently attractive examples of these applications are discussed below.

One of the most fascinating questions currently under study using superconductivity is that of macroscopic quantum tunneling (MQT) and the related issue of macroscopic quantum coherence. The question is: can the laws governing the motion of microscopic particles in quantum mechanics be applied directly to macroscopic quantities representing (collectively) huge numbers of particles which are, in addition, in interaction with the thermal reservoir by damping forces; specifically, can a macroscopic variable such as the superconducting phase ϕ (which describes the state of $\sim 10^{23}$ superconducting electrons) "tunnel" through an energy barrier to another energetically allowed value of ϕ, and can the tunneling probability be calculated by relatively simple analogy to the classic tunneling problem of elementary quantum theory. Theorists have carried out detailed analyses which suggest that this should be true. The experimental situation is much more cloudy. Results in reasonable agreement with theory for lightly damped Josephson junctions have been reported, whereas for more heavily damped junctions, there is a clear quantitative disagreement that appears to be beyond the error limits. Even the apparent agreement for the underdamped case is questioned by other workers, and there is surely a very limited amount of data. The experiments are extremely difficult because not only must the sample be cooled far enough (typically the 10 mK range) to suppress all thermally activated processes, but also all extrinsic electrical noise must be excluded. Because of the fundamental interest in this question, and the general agreement that Josephson devices form by far the most promising test system, further experimental work which achieves a definitive resolution should be very rewarding.

A second fundamental issue is that of quantum noise. Whereas Johnson noise is that derived from exchange of energy with a thermal reservoir, quantum noise is associated with the quantum mechanical zero-point energy, $1/2\ \hbar\omega$ per harmonic oscillator degree of freedom. At high frequencies, this is often called photon shot noise, but it also provides a fundamental limit to the sensitivity of low frequency devices as well. Only superconductive electronics have low enough intrinsic noise to allow this limit to be reached. In fact, SQUIDs with energy sensitivities (in $\Delta E \Delta t$ units) of order Planck's constant, h, have been fabricated in several laboratories, but these are not available commercially. If engineering backup and fabrication facilities were available, these devices could be extended to create a broad range of quantum-limited detectors, amplifiers, and mixers capable of working from dc to the submillimeter wavelength region. Even conventional SQUIDs have had substantial impact in geophysical exploration by magnetotelluric sounding and magnetic surveying. The more sensitive SQUIDs at the quantum limit will play a key role in the instrumentation of gravity wave detectors, touching another fascinating long-range open question in physics. Perhaps "quantum nondemolition" devices based on superconductive electronics will ultimately allow sensitivities which exceed the apparent limit set by the uncertainty principle, but that remains for the future, when the quantum limit has been reached in fully operational systems. Other fundamental researches based on the unique sensitivity of SQUIDs include the long-in-preparation Stanford gyro-relativity experiment, and more recently the possible direct observation by Cabrera of a magnetic monopole passing through a superconducting SQUID-based detector. At present, several laboratories are developing more sophisticated versions of Cabrera's original apparatus, to give a more definitive signature in case further monopole-like events are detected. Not only devices based on the Josephson effect, but also those based on ordinary quasiparticle (Giaever) tunneling between superconductors have great promise as detectors of radiation. In fact, the sharp structure in the electronic density of states associated with the superconducting energy gap makes S-I-S (and S-I-N) tunnel devices the preferred choice for quantum-limit millimeter wave detectors for infrared astrophysics, where exciting new results are now coming in. Such tunnel junctions are also the basis for very promising new solid-state spectroscopic techniques, either for molecules in the tunnel barrier itself, or for the surrounding metal by emission of electrons and phonons of energy determined by the applied voltage.

In addition to the practical motivation to work down to the quantum noise limit, there are major open fundamental questions concerning the quantum noise properties of systems such as Josephson tunnel junctions which have highly nonlinear I-V relations as well as highly nonlinear reactive responses. The usual analysis based on the simplified RSJ model of such devices cannot address these questions, yet they play a direct role in analysis of the performance capabilities of real devices using the most ideal tunnel junctions. Initial data showing the expected electronic shot noise at high voltages has been obtained, but the low voltage regime associated with the transition out of the zero-voltage state remains full of unresolved questions, both theoretically and experimentally, except when thermally activated processes clearly dominate. Such attempts at rigorous theoretical treatment as exist for quantum noise in the general case appear intractably difficult for practical analysis. New ideas seem needed to yield a useable mathematical formulation, and further experiments at $T < 1K$ are needed to provide an adequate data base to test these theories.

A third general application of superconductivity is to the determination of fundamental constants. The Josephson frequency relation already provides the most precise determination of h/e, so that it is used in reverse to define the standard volt. Combined with the precise value of h/e^2 derived from the recently discovered quantum Hall effect, this permits precision determination of h and e independently. Moreover, by SQUID measurement of the flux induced in a rotating superconductor (the "London moment"), one obtains a measurement of h/m, where m is the electron mass, including relativistic corrections. Such measurements are still in a preliminary stage, and could yield valuable checks on the wavefunctions of conduction electrons in metals.

As a final class of applications of superconductivity, we cite illustrative examples of the use of superconductivity as a test bed for other general concepts in statistical physics. In section II.A., we have already mentioned the exciting new results that are emerging from the use of superconducting arrays in demonstrating the validity of the Kosterlitz-Thouless model of the onset of dissipation in two-dimensional superfluid systems, originally proposed in connection with neutral superfluids. Similar experiments have been carried on in parallel in nominally homogeneous superconducting films which give an even more exactly parallel test of the K-T predictions. Only the most recent experiments of this type have begun to establish beyond most doubt that the predictions of this model are being observed, and further ramifications continue to be exposed as new work is done. The use of flux quanta in periodic arrays to study the theoretically discussed commensurate-incommensurate transition is also in its infancy, and much remains to be done.

Lastly, we mention the important work currently underway on the onset of "chaos" or the approach to "turbulent flow" in Josephson junction devices. This subject is of great current interest in statistical physics, because it sheds light on the origin of "randomness" in deterministic classical systems with few degrees of freedom. As a result, it has been studied in many systems, both physical and computational, yet the Josephson device is particularly attractive because of its simple nature and thoroughly studied properties. In addition to this general fundamental interest, chaos in Josephson junctions is an obvious source of excess noise, up to 6 orders of magnitude beyond thermal noise, and it must be understood well enough to be avoided in the many applications of Josephson devices in electromagnetic detectors and in the NBS-maintained standard volt.

C. BARRIERS TO PROGRESS

In essence, progress in all the areas discussed above is limited not by challenges to pursue but by the absence of modern equipment and facilities adequate to meet the challenges, principally facilities for the fabrication and characterization of (primarily) thin-film superconductors, tunnel junctions and other device structures. Since in this regard the needs of the basic physics aspects of superconductivity overlap with those of other aspects, they will be described only briefly at this point, to reduce repetition. In general, one can identify two types of needs: microscopic characterization and fabrication for research. (Fabrication for application is discussed in the Device section of this report).

Clearly, studies of the microscopic properties and processes of superconductors require intimate knowledge of their chemical composition and structure. Increasingly powerful analytical tools to provide such information do exist but must become available to the community. They can be very expensive but in many cases can be shared or used as part of central facilities. Quick, repeated access is essential, however, if such tools are to be an integral part of the creative process. In some cases dedicated facilities will be required.

Fabrication for research refers to the need of many groups to be able to fabricate model systems and experimental configurations aimed at achieving new levels of performance, exploring new effects, or simply tailoring known techniques to specific one-of-a-kind experimental applications. One need not know the microstructure of the system (e.g., a tunnel junction) in detail, so long as desired characteristics can be achieved reasonably reproducibly. This reduces the need for analytical instrumentation, but leaves the need for microfabrication, preferably at a submicron level of resolution. Involved here are evaporators, sputtering machines, mask-making equipment, equipment for photo- or electron-beam-lithography, clean rooms, etc. at a cost of a few hundred thousand dollars per laboratory. Availability of such facilities would not only make many experimental advances possible, but it would also provide an important teaching environment for graduate students interested in future work in microelectronics of all sorts.

III. MATERIALS

The ability to synthesize superconducting materials with the desired properties and to fabricate superconducting devices with the desired electrical performance is fundamental to both the science and the technology of superconductivity. Advances in the understanding of practical or potentially practical superconducting materials generally translates into improved technological performance. Such advances are often crucial for the viability of a particular application. At the same time, the synthesis of new superconducting materials with novel or superior superconducting properties provides new frontiers for science and advanced materials for technology. Moreover, the quest for new and improved superconducting materials continues to involve the development of forefront synthesis techniques and tools that extend the frontiers of metallurgy and materials science in their own right.

Equally important is the proper characterization of these materials. Optimization of materials for applications and sensible interpretation of physical properties for scientific understanding require, at a minimum, detailed knowledge of the atomic composition, crystal structure, and metallurgical microstructure of the materials of interest. More elaborate information is also often required. For example, surfaces and interfaces are increasingly playing a central role in superconductivity, as are the geometries of fine scale composites, intricate microcircuits and inhomogeneous superconducting materials.

A. THIN-FILM SYNTHESIS AND NEW SUPERCONDUCTING MATERIALS

1. Thin Film Synthesis

The most important synthesis technique in the near future is clearly thin-film synthesis based on vapor deposition. The power and relevance of this approach for superconducting materials research and development have already been demonstrated. Many recent advances have been based on this approach, while the newest and best tools available have yet to be brought to bear.

For example, because of its inherently thin film nature, superconducting electronics depends crucially on thin film synthesis. The recent successes in making very much more rugged circuits based on Nb films and capable of essentially unlimited cryogenic cycling have been based on this approach, as has the progress with high T_c superconducting electronics. Thin film synthesis is a powerful approach for obtaining new materials as well. Nb_3Ge, the highest transition temperature superconductor known, was first discovered using this approach. The flexibility and versatility of thin film synthesis also renders it ideal for producing model systems for materials or physics studies. For example, recent detailed studies of the dependence of the basic superconducting properties of Nb_3Sn on crystallographic order and/or tertiary chemical additions using well characterized thin film samples have provided a scientific base for recent optimizations of practical Nb_3Sn magnet conductors. A variety of novel thin film structures with regular or percolative structures have been produced via thin film syntheses that are central to many problems in the basic physics of superconductivity. Advances in thin film deposition and tunnel barrier formation have vastly widened the range of materials that can now be studied by electron tunneling.

However, because the interesting materials in superconductivity are increasingly complex (e.g., alloys and compounds), often exhibit complex chemistry (e.g., oxidation of transition metals), and increasingly involve crucial interfaces (e.g., tunneling barriers) and specific microstructures (e.g., for flux pinning), the thin film synthesis tools must be powerful. Fortunately such tools exist, but they can be expensive. There appears to be no alternative, however, if the opportunities of superconductivity in the next decade are to be addressed. Ideally, the necessary systems involve a closed-system, in situ approach that incorporates the appropriate deposition, characterization and processing (e.g., surface cleaning or oxidation) all in the same system.

Application of these techniques will allow greatly improved control of film composition, structure, chemical purity, and material interfaces. In addition to polycrystalline film growth, entirely new opportunities are expected in amorphous, metastable, epitaxial, single crystal, and multilayer film growth. Studies of film oxidation for tunnel barriers and the new and exciting area of deposited, artificial barriers will be dramatically advanced. A more systematic, less empirical approach to the search for new superconductors and the optimization of known ones will be possible.

On a practical side the likely outcome of this approach will be, at a minimum, improved electrodes and barriers for superconducting electronic applications based on presently practical materials, improved knowledge of the tunneling behavior of real barriers of all kinds, and hopefully a detailed understanding of the physics and materials science of tunnel barriers in general. Based on this understanding, new transistor-like devices may be possible. The emergence of a complete, all-refractory and mechanically-hard superconducting electronic technology seems very likely, and that of an all-high T_c technology seems feasible.

Finally, we point out that also out of this work will come new knowledge about film nucleation and growth, low temperature crystal growth, and molecular beam epitaxy of metals in general. Similar advances in surface and interface physics and chemistry can also be expected. The value of such knowledge is hardly restricted to superconductivity and can be expected to benefit thin film science and technology generally. The increasingly critical role that thin film technology is playing in our nation's overall technology is well-known.

2. Metastable Phases and Amorphous Materials

Most known superconductors are either elements, binary alloys or binary intermetallic compounds; the best usually contain at least one transition metal. In their equilibrium form, the elements and binary combinations have been extensively studied. Thus, interest is now turning to nonequilibrium forms of these materials produced, for example, by thermal or pressure quenching. Well-known examples include the amorphous superconducting alloys and A15 Nb_3Ge (T_c = 23.3 K), which at stoichiometry is not an equilibrium phase. Clearly, these are superconductors of great interest and this area of superconducting materials research remains one of considerable opportunity in the near future. Work is needed both in new methods of synthesis and in studies of their basic properties.

Amorphous superconductors are known to be vastly different than their crystalline counterparts. They have different electron-phonon interaction spectral functions ($\alpha^2F(\omega)$), very small coherence distances and appear to be influenced by localization, at least in very thin films. Their potential for very low flux pinning makes them of interest for fundamental studies and possible applications as vortex file memories analogous to magnetic bubbles. These applications are virtually unstudied at present.

In the case of metastable materials, one is motivated by the apparent close connection between phase and lattice instability and high T_c superconductivity. Hence, there are questions of principle and good reasons to suspect that higher T_c materials should be possible by this route. A15 Nb_3Ge and possibly Nb_3Si are potential beneficiaries of this approach. It should also be noted that the high T_c phase of NbN is not stable at low temperatures.

3. Exotic Superconductors and the Empirical Search for New Superconductors

Exotic superconductors are superconducting materials that exhibit new or extreme material properties which present unresolved questions of theoretical interpretation. Interest in these materials is largely scientific, awaiting deeper understanding to assess their potential for new applications if any. Since these materials are generally new and unfamiliar, an essential part of research on these materials is the synthesis of new members of the class. Each new member brings the potential of yet more unusual properties and in any event provides additional clues in the form of knowledge of how the properties change as the material constituents and parameters are systematically varied.

Currently, important examples of exotic superconductors include heavy fermion superconductors (e.g., $CeCu_2Si_2$ and UB_{13}), extremely low electron density superconductors (e.g., $BaPb_{1-x}Bi_xO_3$), ternary superconductors (e.g., the Chevrel phases and the rare earth borides), organic superconductors [e.g., $TMTSF_2X$], and the quasi one- and two-dimensional superconductors (e.g., $NbSe_3$ and $NbSe_2$). Future possibilities include MOSFET inversion layers and artificial metallic superlattices.

At the extreme forefront of research in superconductivity is the empirical search for new superconductors, which is part of the more general search for new materials with novel physical properties via the systematic synthesis of new classes of materials. Such searches are an essential investment in the future. This point can perhaps be best illustrated in the case of superconductivity by a retrospective look. From this research over the last 25 years, we have been provided with most of the important superconducting materials for science and technology. The list includes the transition metal alloys (e.g., Nb-Ti), the A15 and B1 compounds (e.g., Nb_3Sn, Nb_3Ge, V_3Ga and NbN), and all the exotic superconductors mentioned above. Modern superconductivity science and technology would not be as we know it without them. Nevertheless, this type of research is often viewed suspiciously. This situation appears to follow more from differences of style and personality than scientific substance. When carried out systematically by creative materials researchers as part of the exploitation of new synthesis approaches, it is an essential part of superconductivity research and should be supported. The introduction of a radically superior superconducting material (e.g., a dramatically higher T_c superconductor, a ductile high field superconductor, or a superconductor that is easier to use in superconducting electronics) is always possible and of potentially great significance. There are unfortunately few guides as to how to proceed. Wisdom may lie in patiently investing in the right people rather than specifying the type of material or method of approach.

4. Barriers to Progress

As discussed above, thin film synthesis is central to many of the opportunities in the physics, materials science and technology of superconductivity. The limited availability of thin film deposition facilities and the resources to operate them is one of the greatest barriers to progress in superconductivity research and development.

Because of the high surface chemical sensitivity of most of the materials of interest, ultra-high vacuum (UHV) systems are required. Deposition techniques of proven or expected importance are evaporation (e-beam and MBE), direct and reactive sputtering (magnetron and ion beam), and possibly chemical vapor deposition (CVD). Essential surface composition and structure characterization tools (in many cases required *in situ*) include RHEED, SEM, AES, XPS, and possibly LEED and SIMS. Important processing (and patterning) techniques include ion beams, reactive ion beams and plasma processing. Appropriate lithography tools must also be available in some cases.

As already noted, these systems are expensive. The cost ranges from one to two hundred thousand dollars up to perhaps a million, depending on the degree of completeness, sophistication and level of vacuum. Clearly, the most extensive systems will not be available or even appropriate for all research and development programs. A balance will have to be struck, and perhaps new, imaginative institutional arrangements will have to be developed to make these resources widely available. The need, however, must be met somehow. It should be noted that at least in the area of superconducting electronics and related basic materials studies, sophisticated systems of this general sort have been used in Japan for several years now, at least by the best groups. Comparable systems have not been available in the U.S.

In the case of bulk synthesis the appropriate techniques of synthesis depend on the particular class of materials (e.g., the Chevrels vs. the organics). It should be noted, however, that most of the outstanding groups in the synthesis of the most advanced (i.e., exotic) superconducting materials now reside in Europe. We note also that a broad based $2.4 million program (including theorists) aimed at developing new superconducting materials has recently been initiated at the University of Tokyo. This situation is only one specific example of the general lack of research on bulk synthesis in U.S. materials science. A larger involvement of the superconducting community in solid state chemistry will be necessary to correct this situation. Concomitant support will also be necessary.

B. MAGNET CONDUCTOR MATERIALS

A significant industry has grown up to produce conductors for superconducting magnets. These conductors are now of considerable beauty and complexity, containing (sometimes) many thousands of filaments of only a few microns diameter. At present, however, only two materials, a ductile Nb-Ti solid solution alloy and the brittle compound Nb_3Sn, are made in regular production. Both are used in small laboratory magnets, as well as large dedicated device magnets. Since magnet use requires ampere-turns from the conductor, it is the superconducting critical current density (J_c) which is the property of greatest importance to a magnet builder (assuming that an adequate T_c and H_{c_2} is available). The utility of Nb_3Sn is that its T_c and H_{c_2} are about twice those of Nb-Ti (18 K vs. 9 K, ~21 T vs. 11 T) so that J_c remains high over a much larger field and temperature range. These properties are, however, obtained with significant increases in the complexity of use, due to the brittle nature of Nb_3Sn.

The fabrication process exerts a decisive role on the microstructure of the superconductor, thus determining the J_c. Conductor development has proceeded in an essentially empirical manner due to the extensive fabrication procedures necessary to produce filamentary composites, the complex and heterogeneous microstructures developed during fabrication, the great difficulty of making an adequate metallurgical characterization and the absence of any good theory of J_c in high-field, strong-pinning materials.

Superconducting magnets now find major use in High Energy Physics, Fusion, NMR, and general laboratories of many types. Important future applications lie in Energy Storage, Isotope Separation, and many other diverse areas. Most present uses of superconducting magnets fall into the field range of 7 Tesla and below, a smaller number of magnets being made with fields up to 15 Tesla (the highest field yet produced by a superconducting magnet is 17.5 Tesla). These applications have been made possible predominantly by NbTi (1000-2000 tons of composite made) and Nb_3Sn (≈10 tons made). Other materials may be attractive for new applications (e.g., tandem mirror choke coils) where a 20 T field is desired.

1. Critical Current Density

A fundamental understanding of flux pinning mechanisms is absolutely crucial for a good understanding of the J_c of high field superconductors. This requires a detailed description of the defect metallurgy of practical high-field superconductors and an understanding of the fluxoid-defect interactions in dense strong-pinning systems, as well as a synthesis of these two studies. Careful preparation of a few well-defined model systems also is needed to check the theories.

The task of microstructural characterization requires detailed metallurgical and electromagnetic characterization work on real technological materials (or their close analogues). The initial studies now being performed are presenting a number of unpredictable features (e.g., the curious modes of precipitation in NbTi and the segregation of Cu to Nb_3Sn grain boundaries). Alternative fabrication techniques may yield new defect morphologies on the ~30-200 Å scale where flux pinning occurs and may permit the development of model defect geometries with simpler flux pinning behavior. Flux pinning studies are currently at a very low level in the U.S. (The level of effort is significantly greater in Japan and Europe). Both experimental and computer modeling approaches are needed to attack the problem of pinning in strong, dense pinning systems.

A good understanding of the metallurgical and fluxoid microstructure will undoubtedly lead to significant increases in J_c (the production J_c at 5 Tesla of NbTi is about half that of the best model alloys, as is the case for the J_c at 12 Tesla of Nb_3Sn). This understanding will have a significant effect on conductor costs per amperemeter. There are some applications (e.g., high field magnets, some "Desertron" dipole magnet designs) where high J_c is vital to the technical aspects of the design. There are others, such as the electric power grid diurnal superconducting magnetic energy storage (SMES), where the cost of superconductor is 10-15% of the total capital cost and where high J_c is crucial to the economics of the device.

Some applications of magnets require persistent mode operation, an application for which superconductivity is uniquely suited. This is particularly the case for NMR applications. Persistent-mode magnets must presently operate considerably below I_c. Proper metallurgical and electromagnetic characterization will be very helpful in raising the level at which persistent mode conductors can operate.

2. Diffusional Growth of Superconductors

All presently available materials with T_c and H_{c2} superior to NbTi must be made by diffusion, e.g., Nb_3Sn, Nb_3Sn + additions, Nb_3Al, NbN, $PbMo_6S_8$. The diffusion process is usually carried out at low temperature, in order to produce fine grains, thus favoring grain boundary diffusion as the mode of phase formation. During diffusion, composition gradients exist across the phase, segregation of third elements may occur in grain boundaries (e.g., Cu in Nb_3Sn), and new forms of local equilibrium not predicted by bulk phase diagrams can control phase formation. (These are problems of general interest to Materials Science). It is already quite clear that not understanding these phenomena in Nb_3Sn is holding back our understanding of the superconducting properties in crucial ways.

The development of detailed, highly local models of defects in the microstructures of high field superconductors will be of general applicability to Materials Science. Questions of grain boundary structure, local thermodynamic equilibria, grain boundary diffusion, and precipitate morphologies on a scale of 50 Å are of specific interest in many materials, and the techniques and facilities necessary for their study are of wide use.

3. Ductility and Strain Tolerance

A conductor material needs to have high strength and a reasonable strain tolerance and flexibility to be easily formed into the necessary shapes for use in large scale high field magnets. There are large payoffs for successful research leading to techniques which incorporate brittle superconductors into composites having good mechanical properties for magnet construction. An understanding of the strain effects in A15 superconductors is in a primitive state, and improvements are needed.

4. Higher H_{c2} and T_c

Studies of materials other than NbTi and Nb_3Sn must go on. New applications of superconducting magnets are now very diverse and not always predictable. This means that demands for higher fields and/or higher temperature use will certainly make alternative materials desirable. An attractive near term candidate for study is Nb_3Al; higher risk/larger payoff materials are NbN, Nb_3Ge, and Chevrel phases such as $PbMo_6S_8$.

5. Barriers to Progress

The principal barrier to further advances in our understanding of high field superconductors is the lack of funding of the broad interdisciplinary groups necessary to produce an understanding which integrates both a physical and a metallurgical view of these materials. The problems discussed above all require a broad approach and this has generally not been possible. This situation has arisen because there is limited access to modern instrumentation and a lack of trained personnel with the long term funding needed to make full use of such facilities.

Understanding the J_c is a central problem of all high field materials studies, including those on Nb-Ti and Nb_3Sn. Detailed metallurgical characterizations of the defect microstructures are required. This demands modern analytical instrumentation of the highest order such as, for example, analytical electron microscopes and scanning Auger microprobes. These are expensive instruments ($250-500K), but they are of wide use in materials science. It is vital that the groups active in this field have access to such facilities; provision of equipment grants of several hundred thousand dollars to several groups may be needed to ensure this (cost sharing should be very feasible because of the widespread use of these instruments). Modern analytical facilities do not give instant answers, however; an even more important need is for the training and funding of personnel skilled in the use of these techniques over a period of five years or more, so that the difficult job of extracting detailed, quantitative characterizations can be performed.

An understanding of J_c requires a strong program of flux pinning studies, in addition to those of metallurgical characterization. Such studies are currently at an extremely low level in the U.S., and several groups should be encouraged to embark on studies of high field materials. Both theoretical and experimental work is required. It is desirable that such groups be closely linked to the metallurgical characterization groups.

Many new materials (e.g., NbN and $PbMo_6S_8$) cannot be fabricated using the extrusion and wire-drawing techniques that have been so successful for Nb-Ti and Nb_3Sn. Considerable funding will be required to permit groups to set up dedicated rapid quenching, powder metallurgy, CVD and other unconventional facilities capable of producing lengths suitable for preliminary conductor evaluation. The characterization, both metallurgical and electromagnetic, of these new materials and the understanding of the diffusional growth of compounds also requires an analytical approach similar to that necessary for the study of J_c. This again emphasizes the need for interdisciplinary work, first within each group that is active in the field and then among the different groups pursuing these studies. The danger of continuing to fund individual groups at too low a level is that they can then only pursue limited aspects of the problem. Future developments in many areas of materials science depend strongly on the development of the broad interdisciplinary view described here. The impact of superconducting materials studies on materials science as a whole will be greatly enhanced by such an approach.

IV. DEVICES

A. JOSEPHSON JUNCTION DEVICES AND SUPERCONDUCTING ELECTRONICS

1. Applications and Devices

The electronic applications of superconductivity have many dimensions. Consequently, in light of the recent decision by IBM (taken after this Workshop) to cut back significantly its Josephson junction super-computer program, it is especially important to clarify the opportunities and needs in this area. Roughly speaking, superconducting electronics can be divided into four categories: ultrasensitive instruments and electromagnetic detectors; fundamental standards; microwave and high-speed signal processing applications (both digital and analog); and large, high-performance computers. The IBM decision pertains only to the last of these, and even here, as noted in the introduction, the arguments for the eventual need for a cryogenic computer remain in force. Superconductivity continues to be relevant for high-performance computers in the long run. A new switching device will probably be required, however.

Superconducting devices in the remaining categories, on the other hand, continue to exhibit significant advantage over their competitors. Particularly promising in the near future appear to be systems applications built around fast JJ analog-to-digital converters and high-speed analog signal processing devices (e.g., converters and correlators). At the same time, advances in superconducting instruments and detectors are impacting significantly in basic science, e.g., medical science (SQUIDS for magnetoencephalography) and radio astronomy (SIS mixers).

In support of the present technology and as a scientific base for new device structures and concepts, great opportunity appears to lie in better understanding tunneling barriers. These barriers are the single most critical part of the fabrication of any superconducting device involving tunneling. It must be accomplished with extremely close control and reproducibility. Presently our scientific understanding of the actual chemical and physical structure, and the physics of the tunneling process itself in real barriers is very primitive. By comparison to the effort spent studying the electrodes, barriers have received much less attention overall.

At the same time, whole new approaches to forming tunneling barriers based on artificial barriers (amorphous Si, oxidized Al, etc.) have emerged in recent years and proven very successful. Along with this have come new fabrication processes such as the selective niobium anodization process (SNAP) and selective niobium etching process (SNEP) approaches. These whole-wafer approaches, in which a junction is formed over the whole wafer and then subsequently patterned into the desired small structures, is fundamentally different from the standard SiO mask/ lift-off patterning approach. In principle it appears superior. This approach needs further development to ascertain its true potential. Similarly, the rapid advances that have occurred recently in making all-refractory integrated circuits need to be further continued. All-refractory circuits will ultimately be desired and appear to be a real possibility if they are aggressively pursued. Here we are definitely behind the Japanese, at least in the sophistication of the circuits that have been built in an all-refractory fashion.

The situation with all-high-T_c circuits capable of operating with small closed-cycle cryogenic refrigerators is more complicated. Such circuits are of perhaps crucial importance for the smaller scale superconducting electronic applications where only a few devices or chips are involved and the technical and economic penalty of refrigeration is most significant. Very good tunnel junctions incorporating high-T_c base electrodes can be made, but high-T_c counterelectrodes have proven more difficult. The problem lies in forming a high-quality film at sufficiently low substrate temperatures so as not to damage the underlying barrier. The two most likely candidate materials are one of the A15 superconductors or NbN. Success will depend on a combination of lowering the deposition temperature required for counterelectrode formation and/or raising the temperature that barriers can withstand.

At the present time, NbN is the most advanced in that it can be deposited at relatively low temperatures with $T_c > 10K$. All-NbN tunnel junctions have been made in Japan (and more recently in the U.S.) but their characteristics, and indeed the T_c of the counterelectrode, will have to be improved before high operating temperature superconducting electronics becomes a real possibility. High T_c SNS Josephson devices already exist but are likely to have more limited application than tunnel junctions. They are superior for some applications, however, and should continue to be developed. Finally, we note that the practical reality of superconducting electronics capable of operating with a small refrigerator is intimately tied to developments in miniature cryogenic refrigerators. Developments in this field need to be watched carefully and attempts need to be made to package and interface the circuits on the refrigerators as appropriate.

Another area of great opportunity in superconducting electronics is new devices, particularly a three-terminal transistor-like device. The introduction of such a device would greatly open up the variety and nature of the applications which are possible. It is almost certainly a sine qua non for any new approach to a general purpose, high-performance superconducting computer. The QUITERON is one possibility, but it has definite limitations. Other approaches based on nonequilibrium superconductivity are imaginable and should be investigated. Radically new approaches should also be encouraged. What is desired is a tunneling triode or FET-like device with high speed, low power and high gain. Vortex file memory devices are also of interest. New circuit concepts and applications are always possible and could have great impact. Finally, the physical and technical limits to microminiaturization of all superconducting devices and circuits needs to be explored as one looks to future applications and needs.

2. Barriers to Progress

Perhaps the greatest overall need (and opportunity) of superconducting electronics is to get more teams and facilities working in the technology. The stimulation and vitality resulting from new ideas, new applications, and new approaches cannot be overstated. In this same spirit, it is clear that one of the greatest challenges in the near future is to bring to fruition the various applications now being developed, and to increasingly involve the electrical engineering community.

Like the other areas of superconductivity research and development, progress in superconducting electronics is most greatly limited by fabrication capability, fabrication for application and fabrication for device and processing research. With the almost certain closure of the IBM development line at East Fishkill, there will be no facility in the U.S. capable of producing superconducting electronic chips of even modest complexity (say greater than 15-20 devices). Obviously the field can not develop under such circumstances. Unfortunately the IBM decision was reached after this Workshop and so no discussion of this issue was possible. This is clearly an issue of national importance.

Fabrication facilities for device and process research and development parallel closely those for thin film deposition discussed already. This follows from the multi-layer thin-film nature of all superconducting devices and circuits. Unlike fabrication facilities for application, where the line must be highly disciplined, for research the premium is on flexibility. Hence, unfortunately, the two functions are not easily married. The cost of both types of facilities will be substantial. No estimates were prepared in detail, but it seems clear that the capitalization and operation of these facilities can not be carried out under the existing funding levels for the field.

Finally, as superconductive electronics achieves greater maturity and standardization, more applications will occur in which the superconductive element is just an "engineered" device, fabricated in a standard way, to specifications. To get over the financial barrier to the point where these applications occur on a scale large enough to support private industrial fabrication, it may be necessary to set up some sort of subsidized "Josephson junction foundry" to carry out this function on a national scale. However, in the opinion of most participants, such a step at present would be premature, and divert resources from more pressing needs, however desirable it might be in later years.

B. MAGNET CONDUCTORS

1. Practical Requirements

The device which the magnet conductor serves is of course the magnet. Superconducting magnets and their technology were only briefly surveyed at the meeting and are not addressed in this report. It is pertinent, however, to consider how to encourage the engineering of a magnet conductor from a material which previously had been only of purely scientific interest. A major bottleneck in the development of new conductors lies in the metallurgical engineering and physics which are required to take a superconducting compound with a high H_{c2} and T_c and prepare a few short lengths of useful stabilized conductor in wire or tape form. The crucial step here is to prepare the material in a form relevant to its end use in a magnet; thus its I_c and J_c must reach useful values and the configuration of the superconductor must be electromagnetically and mechanically stable. This stage marks the transfer from that of first synthesis to that of potential manufacture. The first stages of grappling with the problems of fabricating conductors of adequate stability, strength and superconducting properties are generally carried out in the research laboratory. The later stages of this development will certainly require a substantial industrial involvement; the early stages may occur either in universities, national laboratories or industry, preferably with good collaboration between all the groups concerned with realizing a magnet conductor.

The issues involved in bringing a new material to full scale conductor manufacture are many and varied. Since the expenses of bringing a material from the interesting property stage to useful lengths of magnet conductor are considerable, it is clear that the step will be dominated by the specific needs of actual devices, rather than the intrinsic interest of a new material. It thus seems appropriate to leave the particular issues of developing, say, a 20 or 25 Tesla magnet conductor of specific configuration to those projects which need such conductors.

However, the general aspects of fabricating new materials into potential conductors are important subjects of study, independent of specific project need. Our present understanding of the properties of even Nb-Ti and Nb_3Sn is still poor, let alone our knowledge of newer materials such as Nb_3Al or $PbMo_6S_8$. Much useful work, relevant to their eventual use as magnet conductors, must still be done on these materials by investigating small scale fabrication techniques, proper microstructural characterization and understanding the way in which T_c, H_{c2} and J_c are determined by fabrication conditions. The resistive power demands of conventional magnets with fields greater than about 15 Tesla are so enormous that such fields can only be supplied in a few laboratories in the world. The demands for access to high fields quite outstrip the number of facilities available. Conductors suitable for higher fields will not appear without magnet conductor research; such studies are quite basic to any plans to construct superconducting magnets with fields in the 20 Tesla range.

2. Barriers to Progress

An obvious barrier to progress is the lack of funding for high field superconducting magnets. The practical limit for superconducting magnets has remained at about 15 Tesla for several years and such magnets continue to be based on Nb_3Sn tape technology, rather than the preferable technology of filamentary Nb_3Sn. The world's highest field superconducting magnet (~17.5 Tesla) is in Japan and is now also several years old. It too utilizes tape technology and can be regarded as a partially successful attempt to push this technology to its limit. What is needed now is a program to utilize filamentary Nb_3Sn and its alloys in high field solenoids of 15 to 20 Tesla.

Extending the capability of superconducting magnets to higher magnetic fields is of intrinsic interest in itself, as well as being of broad interest to many areas of study in basic science. The Japanese have a very vigorous program aimed at producing superconducting magnets of 18-20 Tesla. Small high field solenoids, built as inserts to existing magnets, can generate incremental fields of a few Tesla from a few pounds of conductor, while doing it in a realistic stress and field environment. This same approach is also desirable for other promising high field materials when they are ready to make the transition from that of laboratory material to that of magnet conductor. These construction efforts require high J_c at high fields in order for magnets to be feasible and detailed electromagnetic and metallurgical characterization is required, of course, at both the first laboratory synthesis and the prototype magnet conductor stage for these conductors too. Successful extension of the field capability of superconducting magnets, even in small volumes, can give a powerful push to technologies such as Tandem Mirror Fusion which need high fields in large volumes. Large engineering programs can then efficiently expend their resources for the development of large scale manufacture.

V. SUMMARY

The national investment in superconductivity research has already paid handsome technological returns. Recent examples are:

- Whole body scanning with NMR, made possible with superconducting magnets, has just barely tapped its potential in medical diagnostics, and is projected to be a $2B industry in two years.
- The National Accelerator Laboratory has recently activated more than 1,000 6-meter-long superconducting magnets in its Tevatron, which will leapfrog the U.S. ahead in high-energy physics while saving immense amounts of electrical energy.
- Superconducting microcircuits for detectors represent the state-of-the-art in the measurement of low level signals.

This report addresses the question "Where do we go from here, and what will it take to get there?"

A. RESEARCH OPPORTUNITIES

Key areas where basic research is needed for the internal development of the field of superconductivity include the study of:

(1) transient response far from equilibrium,
(2) nonlinear effects under heavy electron injection,
(3) thermoelectric effects,
(4) phenomena associated with localization, and
(5) new mechanisms of superconductivity.

These are major problems where new concepts are needed. Other areas where superconductivity can be used to solve fundamental problems of broad interest beyond superconductivity include:

(1) the fundamental issue of quantum noise which is central to the development of detectors, amplifiers and mixers, and
(2) the use of flux quantization and the Josephson effect for measurement of fundamental physical constants and for the search for new particles and new phenomena such as the magnetic monopole and macroscopic quantum tunneling.

In the field of superconducting materials, key areas of opportunity include:

(1) thin film synthesis,
(2) the preparation of high quality tunnel barriers,
(3) the study of interface effects,
(4) the growth of epitaxial, single crystals and multilayer thin films, and
(5) exploitation of new types of bulk synthesis.

Especially important new materials include:

(1) metastable and amorphous materials,
(2) ternary superconductors,
(3) heavy Fermion systems,
(4) the superconducting oxides, and
(5) organic superconductors.

The search for new superconducting materials can be expected to provide new materials for broad application in both science and technology.

For the development of practical conductor materials for magnets, key areas with high payoff are the development of:

(1) new techniques for the understanding and control of microstructure,
(2) new methods of optimizing the performance of brittle filament superconductors in order to produce composites of high strength and strain tolerance,
(3) fabrication processes for new materials with higher H_{c2},
(4) new studies of J_c for the high-field, strong-pinning materials, and
(5) an understanding of the diffusion growth of high H_{c2} compounds.

In superconducting electronic device development, critical areas to be studied include:

(1) fundamental research on barrier materials to understand in detail the physical processes involved in electron transfer through different kinds of barriers,
(2) the development of materials preparation techniques for all refractory and all-high T_c superconducting electronics circuits, and
(3) the development of three-terminal superconducting devices which might have the broad impact that transistors have in semiconductor electronics.

B. SPECIAL FUNDING NEEDS

In essence, progress in all areas discussed is limited by the absence of modern equipment and facilities adequate to the tasks of synthesizing, characterizing and/or fabricating the desired superconducting materials and devices, be they exotic superconductors, practical materials, complex composites or tunnel junctions. Roughly speaking, there are four categories of need: synthesis (thin film and bulk), microscopic characterization, fabrication for research, and fabrication for application.

In terms of breadth of impact, the greatest need in synthesis is for enhanced capability in thin film synthesis. Such capability is important for physics studies, new materials and applications. On the other hand, bulk synthesis is underrepresented in the U.S. as gauged by the world scene. Here, nurturing of people is as important as improved facilities.

The central characterization problem in superconducting materials is precise knowledge of the chemical composition and the atomic structure. In superconducting electronics, it is the microscopic nature of the Josephson junction, particularly the barrier. In magnet conductor materials, it is the microscopic nature of flux pinning defects such as precipitates and grain boundaries. One is often concerned with complex polyphasic or inhomogeneous materials with position-dependent chemical composition, interfaces, defects, localized electron states, etc. It is essential to understand these properties so they can be controlled to obtain desired characteristics for high performance or simply to understand the material. Such work requires a very extensive array of analytical tools, often in a complex high-vacuum system, as well as training and long-term funding of personnel skilled in the use of these techniques. Such tools are very expensive, but fortunately can be shared or used as part of central facilities.

Fabrication for research and application refers to the need of many groups to achieve new levels of performance in one-of-a-kind experimental applications. This work can proceed without detailed characterization, as long as desired characteristics can be reproducibly achieved. Dozens of laboratories need to be upgraded in this way, at a cost of several hundred thousand dollars each.

Thus, it was perceived that the following two categories of research facilities provide opportunities for funding that would have widespread impact within and beyond superconductivity research:

- Several (10-20) dedicated facilities (estimated cost $200-500K each) focused on specific problems or applications such as tunnel junctions, a particular material class or flux-pinning. This should include a balance of synthesis, characterization and fabrication facilities appropriate to the specific problem.
- A few (3-4) larger facilities (estimated cost $1-3M each) broadly equipped to tackle generic problems or applications such as advanced materials, advanced magnet conductors or advanced electronic circuits. By virtue of their cost, such facilities should be research and training centers widely available through collaboration or visitor programs.

It is important to point out that all of these facilities and the research done with them are at the cutting edge of growth in broad fields of science, and have implications far beyond superconductivity. Out of the work will come new knowledge about thin films, materials behavior, and fundamental physics. Thin film technology is playing an increasing role in the nation's overall technological advancement.

C. CONCLUDING STATEMENT

The equipment and investment in people needed to meet this challenge are costly, but they also are essential if the opportunities before us are to be pursued. This fact is recognized abroad, especially in Japan, where well-funded, long-term national commitments have been made both to develop a supercomputer and to make a broad advance in the development of superconducting materials. In pursuit of this goal, several Japanese laboratories are working with a range of sophisticated equipment which no U.S. group is able to apply to these problems. If the U.S. is not to concede technological leadership by default, comparable investments and a similar long-term perspective must be undertaken here. Surely the needed resources are modest on a national scale; the question is: do we have the vision to make them available? This is truly a national issue.

LIST OF ATTENDEES

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M. R. Beasley - Stanford University
A. I. Braginski - Westinghouse Research and Development Center
R. A. Buhrman - Cornell University
B. Cabrera - Stanford University
A. F. Clark - National Bureau of Standards
J. Clarke - University of California, Berkeley
B. S. Deaver - University of Virginia
E. A. Edelsack - Office of Naval Research
D. K. Finnemore - Iowa State University
S. Foner - Massachusetts Institute of Technology
F. Y. Fradin - Argonne National Laboratory
H. C. Freyhardt - University of Göttingen
J. C. Garland - Ohio State University
A. L. Giorgi - Los Alamos National Laboratory
A. M. Goldman - University of Minnesota
R. L. Greene - IBM
A. S. Hebard - Bell Laboratories
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